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This issue of *JASO* has been specially enlarged so as to accommodate three papers on the subject of Death in Portugal that were originally presented at a conference held at St Antony's College, Oxford, in June 1982.

The theme of death is a classic one in social anthropology, where the pioneer studies of Robert Hertz and A. van Gennep still provide important reference points for recent works in the anthropology of death in Western European societies as well as in other parts of the world. The 'new history of death', initiated by French historians in the 1960s - the key names here being Ariès and Vovelle - has provided an important inspiration and stimulus for a number of the papers that appear here. These works form part of the intellectual background for a reconsideration of the ways in which an analysis of death practices throw light upon forms of life and cultural change in modern Portugal.

The Editors of *JASO* record their grateful thanks to the *ad hoc* group of sponsors whose subsidy has made possible the publication of this enlarged issue of the Journal: the Anglo-Portuguese Society (London), the Instituto Português do Livro (Lisbon), and the London branches of the Banco Espírito Santo e Comercial de Lisboa, the Banco Nacional Ultramarino, the Banco Português do Atlântico and the Banco Totta e Açores. This group of sponsors, together with the U.K. branch of the Calouste Gulbenkian Foundation of Lisbon (under the patronage of which the Conference was organised last year in Oxford), have also helped with the simultaneous publication, by *JASO*, of a volume entitled *Death in Portugal: Studies in Portuguese Anthropology and Modern History*. This contains the three anthropological essays published here together with four further essays - by Maria de Fátima Brandão, Margarida Durães, T.F. Earle and Augusto Santos Silva - on the subject of Death in Portugal from other perspectives in social studies and the humanities. *Death in Portugal*, which appears as *JASO* Occasional Papers, no. 2, is edited by Rui Feijó, Herminio Martins and João de Pina-Cabral; it is available, price £6.25 post free, directly on application to the Editors of *JASO*. We owe a special debt of gratitude to Rui Feijó for enlisting the support of the above-named sponsors and for his help in the preparation of the papers presented in *Death in Portugal* as well as those in this issue of *JASO*.

1983 marks the fiftieth anniversary of the establishment of Portuguese Studies in Oxford. By devoting a substantial part of this issue to the anthropology of Portugal, the Editors of *JASO* are pleased to associate the Journal with the commemoration of this jubilee anniversary. A note on Portuguese Studies in Oxford has accordingly been appended to this special group of papers.

THE GOOD DEATH:  
PERSONAL SALVATION AND COMMUNITY IDENTITY

I. *Introduction : The Concept of the Good Death*

In Portugal and Brazil, Nosso Senhor do Bom Fim, or Nossa Senhora da Boa Morte, are addressed in prayer and have churches and chapels dedicated to them. This would seem to be the remnant of a much wider worship throughout the Catholic world, that in the Portuguese case takes a very concrete form, with statues and churches erected as symbols of the concern prominent in the Middle Ages of 'making a good death'.<sup>1</sup> In the area of Geres there are chapels dedicated to Our Lady of the Good Death (e.g. in Covide) and there are chapels to the same saint in the area of Guimaraes and Amarante as well. Villagers throughout the north often make dedications and *promessas* to either Senhor Jesus da Boa Morte or to Nossa Senhora da Boa Morte. The medieval notion of the good death involved resisting the temptation to despair or pride, together with sincere

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I am grateful for the financial assistance given to me by the Instituto de Alta Cultura during my initial fieldwork (1972-3), and by the Calouste Gulbenkian Foundation who funded short trips to Portugal in 1974 and 1976. The fieldwork was conducted in the District of Braga, in villages of the Geres region. The village of S. João is a pseudonym used in accordance with anthropological convention. The ethnographic present used in the article refers to 1972-3, my initial fieldwork period.

<sup>1</sup> Cf. Philippe Ariès (*L'Homme devant la mort*, Paris: Seuil 1977) for an account of attitudes to dying from the Middle Ages to the 20th century.

repentance for sins committed. Apart from the metaphysical aspects, there was also a very practical side to a good death; dying with good warning, in bed, enabled one to prepare for death with the appropriate, traditional ceremony and to do a public accounting - forgiving enemies, blessing friends and children, and paying off one's debts. Villagers in some areas of northern Portugal retain this total concept as a cultural ideal. It is considered a terrible thing to die away from the family, away from the village or as now happens, away from Portugal. Those who die suddenly, or in an accident, are especially mourned. A man should die in his home, surrounded by family and neighbours, and with time to settle his affairs, both spiritual and temporal. To receive the Last Sacraments, to establish the number of masses to be said for one's soul and the dedication of the masses to particular saints is still important to many villagers. Involved in the concept of the good death are a complex of factors, both spiritual and practical, that imply organising the end of life in strict accordance with the accepted religious and social norms that continue through life. Thus a man who dies after a long life, leaving industrious children, no debts, his land already apportioned to his heirs, with no dispute overhanging his property, and fortified with the rites of the Roman Catholic church, is thought by fellow-villagers to have done well and achieves a certain respect and even a little envy of his good fortune in arranging his end so satisfactorily. In this way the 'good death' constitutes a cultural ideal.

Dying well, like living well, is not a solitary activity; one needs partners, accomplices, witnesses. The 'good death' involves not just the principal actor but the supporting cast who must fulfil their roles on his behalf. Relatives who fail to fulfil the religious ceremonies of death, burial or the subsequent obligations can condemn their kinsman to an unhappy post-life existence. For a 'good death' one needs the co-operation of one's kin. Dying away from the village is regarded as an especial sadness and the dead or dying are brought back to the village for burial if at all possible. Most usually nowadays this means being brought from the hospital as a terminal case, but there have been instances of dying men being brought back from Lisbon or France or even Canada to die at home. Just as the children of migrants to industrialised Europe are returned to the village for baptism, so the dead and the dying are brought back from France to be buried at home. The kin group and the small community welcomes back its own, and in death, the family can be thus assured that the proper rites have been observed.

## II. *Rituals of death*

The procedures followed when a death occurs have changed little

over time. Until recently, the church bell would toll out from the moment of death until the grave was closed - to remind others to pray; now it peals only for the time it takes for the body to be taken from the house into the church, marking the transition from the secular world to the religious, tolling an uneven number of peals for a man, and an even number for a woman. Usually, if death occurs at home, burial takes place the following day, and the intervening night is spent by the bereaved and their neighbours in a 'wake' at the house of the dead person. Close friends and neighbours, one from each house, gather together to pray, to cry, to eat and drink and to tell stories of the dead person's life and qualities. The basic ritual prayers are 30 Our Fathers, 30 Hail Marys and 30 Glorias. The atmosphere is one of restrained mourning; there is no singing and only village wine is drunk. Food is brought to the house by each visitor from the other houses in the village, even by those who do not stay the night, so that the widow or widower is free from daily chores for the day of the funeral and several following days, and can then spend all the time grieving. People say that food is sent in by neighbours to avoid 'frying the soul of the dead'. Cooking by the deceased's close kin is to be avoided. Any small children are taken from the house of the dead person into neighbours' homes and do not attend the 'wake' or the funeral the following day.

The burial procession follows a set procedure. It is always led by the village *Procurador*, carrying the large crucifix from the Parish church. Like a church warden, the *Procurador*'s other duties include ringing the bell for mass each week, collecting Easter dues and looking after the collection money. It is an office that rotates between the married heads of houses each year, and refusal to serve carries the penalty of a fine. Behind the *Procurador* comes another household head, carrying the church's banner; then the coffin, carried low by six men including the President of the Parish/Village Council (*Junta*) with one man from each house following on behind. After them come the parish priest and the women. No children walk in the funeral procession, nor do immediate family of the dead person go to the church for 'only those who can trust themselves not to weep go to the church'. As the body is lifted to leave the house, wailing women, relatives and non-relatives of the dead called in on a reciprocal basis, break out into screams and cries that echo through the whole village, as the door and windows of the house are flung open. This formalised wailing accompanies the procession until the funeral mass begins in church. Not everyone nowadays uses the services of wailers; some people feel that it is too distressing while others still think it is a fitting and proper tribute to the dead. Apart from the paid wailers (paid only in food) close female relatives sometimes take part in the 'keening' too. During the requiem mass the coffin is left at the rear of the church surrounded by candles. Although there are more women than men in the congregation - because of migration and conscription - at least one man from each house is present throughout the service. After mass, the whole congregation goes behind the coffin to the

cemetery as before, with the *Procurador* leading the way in procession and the priest bringing up the rear with the women. When the body has been interred with the customary Catholic ritual, the wailing breaks out again and continues until sunset.

After mass, small sums of money are given by relatives of the dead man to local beggars who wail outside the church, and small gifts of food - salt cod, bread or cheese - are distributed afterwards to those attending the service. The mattress and bed-clothes of the deceased are burnt. Funeral processions follow fixed routes to the church, so-called 'sacramental paths', that are the same that should be followed at times of baptisms, weddings and funerals. When people from outlying villages come to the parish church to bury their dead, wooden crosses or twigs mark the places where the coffin can rest on the way, and prayers are recited at each halt. The procession from Vilarinho to Campo, for example, was preceded by a woman who went ahead to the Campo church with an offering (*obrada*) 'for God', received by the priest. The *obrada* consisted of half a *raza* of corn and half a kilo of ham (*toucinho*). The woman who performed this service should not talk to anyone, even the priest, until the dead person was interred.

Burial is not the end of obligations to the dead. Each Sunday after mass anyone who has lost a relative in the previous twelve months goes to the cemetery to pray by the graveside. Nowadays, with the long absences of men from the villages it tends to be women alone who perform this ritual for their in-laws as well as for their own kin. The more recent the bereavement, the less restrained are the manifestations of grief, and newly widowed women will throw themselves prostrate on their husband's grave, eventually to be led away by a neighbour.

Each evening the church bell sounds for the recital of the *Trindades* (an evening Angelus) - three bells for three Ave-Marias plus two long strokes, one for the Holy Souls and the other for 'the sinners of the parish' (*os pecadores mais necessitados da freguesia*). People stop whatever they are doing, and pray for a few moments. Since the bell rings usually at sunset most people are in their homes before eating their evening meal, and traditionally the family prays together, aloud, by the open window, so that the sound of the prayers would be heard rising from all the houses. In recent years, it has become a more private ritual, but most people will still stop what they are doing, in the house or in the fields or in the street; in the cafe, however, life goes on without interruption.

The dead are thus remembered each day through the *Trindades*, and especially on Sundays. They are also commemorated by the wearing of mourning black by their relatives. The degree of kin or affinal ties is reflected directly in the period of time for which mourning is thought proper; as long as seven or ten years for a parent, and two to three years for an in-law. For a husband many widows decide to stay in mourning for the rest of their lives as in the rest of Latin Europe. Older widows also keep up the habit of wearing over their heads the *avental de costas*, a rough-

woven black woollen apron-style shawl. Children's deaths are not commemorated by any of these rituals. No one goes into formal mourning for children; although they receive a requiem mass, only immediate family are expected to attend. The stillborn are, traditionally, but to a lesser extent now than in the past, not even interred in the cemetery but buried in the floor of the *corte* below the house, where the after-birth is customarily buried.

### III. *Death Rituals in Historical Perspective : The Religious Background*

Most northern residents are at least nominally Catholic, while outside the cities adherents of other religious faiths are of insignificant number, and the proportion of those actively participant in Catholic religious activities is very high indeed, in many areas representing over 90% of the local population. The district of Braga, whose archbishop was Primate of Iberia, was early on an important religious centre; the diocese was first created in A.D. 37.

At the Council of Braga in 411 the Lusitanian bishops, threatened with invasion by the German tribes, resolved to remove and hide church valuables, such as the bodies or relics of saints, the church silver and statuary, in remote places in the mountains, in caverns or grottoes, where in later centuries their reappearance gave rise to the foundation of shrines and pilgrimage sites.<sup>2</sup> Despite the Suevic occupation of Braga, the city again became Catholic in 563 A.D. with the conversion of the major Suevi residents. At the second Council of Braga in 610 A.D. the payment of *dízimos* or church tithes was ordered by the bishops, along with the injunction that churches were no longer to be used as burial places, but the dead were to be interred outside the wall of the church. By the eleventh century the dead were again being buried within the church, but it took over 1200 years for the ecclesiastical injunction to become public policy in the 19th century. By 836 the earliest known pilgrimage - to Rome to visit the tombs of Saints Peter and Paul - was again very much alive, and it is probably the origin of the continuing and current practice of rural Portuguese who go on *romaria* (pilgrimage), from the term referring to the ancient pilgrimage, *Romalia*.

Until 1835 there were no public cemeteries in rural Portugal:

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<sup>2</sup> For references to the Bishop's records of hiding places see Padre José de Matos Ferreira, *Thesouro de Braga*, 1728 (Biblioteca Nacional de Lisboa, MS 583).

all burials took place in the floors or *adros* of churches, and in the private cemeteries of military hospitals and misericords. Only after the cholera epidemic of 1832 were some areas of land blessed for burial. The majority of Portuguese cemeteries are 19th-century or 20th-century in origin: in the rural north many date from the 1920s and 1930s. Church burial was then customary over a long period, and the Health Laws of the 1830s and 1840s provoked rural uprisings in protest against proposed changes in this practice.<sup>3</sup> Death and burial practices are part of a value system, a shared world-view common in this case to a large area of upland, frontier Portuguese Galicia.

Religion within the village stresses the importance of local ritual, regional shrines and village pilgrimages, in all of which the priest's presence is important as public functionary. The influence of the national Church or its hierarchy is very low in this area; local attitudes to the Church as an official institution is rather one of opposition, as shown in the hostility to clergy when they refuse to perform traditional services. Local rituals are mainly concerned with protection of the household, cattle and the agricultural enterprise, and are still performed despite the decreasing importance of animals in the local economy and the increasing reluctance of priests to perform them. The strength of local religion in opposition to the national Church is not just provincialism or a quaint survival of 'folkness', but must be seen as integral to the belief placed in the family and the community as a valid religious unit as well as an economic and social unit.

The link between the concept of the 'good death' and other parts of the belief system is clearly seen in the case of the 'wandering souls', those unfortunates who did not have time to settle their accounts before they died, or whose relatives did not fulfil the customary religious requirements regarding burial. 'Wandering souls' who appear as ghosts (*aventesmas/sombras*) or as tormented souls (*almas penadas*) can be encountered at night either outside the village or around the cemetery (which is usually situated either within the village or very close to it), or they appear in dreams at night to a close relative. The three main reasons given for the 'appearance' of 'wandering souls' on earth are that they were left unburied, and so have no resting place, and cannot enter heaven until they have had a Christian burial; or that their kin failed to fulfil the customary ritual; or that they committed some injustice that was not remedied before their death. The most common injustice cited is that of moving boundary stones, so cheating their neighbour of an extra two or

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<sup>3</sup> See João de Pina-Cabral and Rui Feijó, 'Conflicting Attitudes to Death in Modern Portugal: The Question of Cemeteries', in this issue of *JASO*; also in Rui Feijó, Herminio Martins and João de Pina-Cabral (eds.), *Death in Portugal: Studies in Portuguese Anthropology and Modern History*, Oxford: JASO 1983.

three feet of land; the area involved is always trivial, but the land holdings are small too.

Men do talk of having contact with such souls. *One soul appeared one night to João da Redonda; he recognised it immediately as his dead uncle, who asked him to get out of bed and go with him to the fields. This he did, and once there, his uncle asked him to move a large boundary stone about a metre's distance, so as to satisfy his conscience, for he had moved it in his favour many years before. João did as he was told, and the soul thanked him and disappeared.*

People who see witches or evil spirits are thought to be those who did not have 'proper' baptisms, where some of the words or ritual were missed out by accident, or where words were spoken incorrectly. Conversely, those spirits who wander about on earth are the unhappy dead, or those whose burial ritual was incomplete. These beliefs, though not currently supported by the Church, clearly originate in a period where ritual orthodox observance was heavily stressed, and bring so-called 'pagan' spirit or witchcraft beliefs within the same tradition as more commonly held 'Catholic' beliefs. Animals too could be ill omens if encountered at night: dogs, cats, goats, pigs, cocks, hens can all take on the manifestations of evil spirits at night. As a defence one should carry a mirror, a cross or a rosary at night on the road, and avoid looking at an animal encountered out on its own at night.

Accidental or violent deaths on the mountains also cause unease as it can be several days before the body is recovered, and, apart from possible ravages by dogs or wolves, it means a delay in proper burial that normally would take place on the day following the death. In these cases it is the fact that the proper ritual and order of events has not been observed that causes disquiet; the living continue to have an obligation to the dead, and chief among their obligations is the duty to ensure customary burial.

Belief in life after death is the basis of belief in the suffering souls that haunt the earth, usually because of improper burial, or because of some injustice committed by or against them during their lives. Belief in the Devil, and in the efficacy of exorcism (only recently condemned by the Church) is the orthodox basis for a very strong belief in the more generalised power of evil and the multiplicity of evil spirits. Although the forces of good are commonly believed to appear in physical form with some regularity in the Portuguese tradition - miraculous statues, springs, and the apparitions at Fatima, all with firm credentials from orthodox Catholicism - there is nothing inconsistent in an equally strong belief in the physical manifestations of the forces of evil. And if the forces of good have their human representatives in the saints, priests and *beatas* that function on all societal levels, then witches, as human representatives of the forces of evil, are their complements on the conceptual and the

practical level. Witchcraft beliefs in Portuguese villages, far from being some 'pagan' residual element of an earlier non-Christian tradition or the sign of a 'folk-culture' operating on a different logical level from the main Catholic culture, are better seen as a firm part of traditional Catholicism, albeit somewhat 'medieval' and embarrassing to a Church anxious now to stress its modernity and intellectualism - or at least the more urban education of its younger priests.

Despite the long tradition of Catholicism in the north, religious attitudes are not standard and are not without their tensions. Even in areas where most residents would call themselves Catholic, and are believers and practicers, there are signs of anti-clericalism, not unlike that in parts of Catholic France and Spain. While largely believing and practising his religion, the Minhoto peasant is not 'priest-ridden'. Contemporary independence of attitude shown in religious matters has an echo in earlier documents taken from a parish in the Geres area of Braga.<sup>4</sup> In the eighteenth century a complicated tussle ensued between the villagers of São João and the local parish priest over alleged abuses and his failure to perform his priestly functions, which was taken to the authorities in Braga by the villagers. In the ensuing correspondence the villagers accused the parish priest of taking over a piece of common land (*balديو*) for his own exploitation, as well as 'oppressing [us] at every turn, without any remembrance of humane sentiments'. The matter went to the court at Viana, when the villagers demanded the removal of the priest from his post. Correspondence between the legal bodies and the church authorities show the latter's reluctance to come to a firm decision on the matter, 'for fear of encouraging others'. The case dragged on for a period of years, during which the priest was accused of diverting village water to his own lands, and of taking over a further piece of common land for his own use.

Apart from these land disputes, there was also the question, relevant to our present discussion, of the priest's failure to provide the last sacrament to the dying, and his demanding more money from tithes to cover church expenses, such as the altar wine and oil, which the villagers felt he should provide himself from his existing income, instead of increasing charges on an already poor community.<sup>5</sup> While the outcome of the litigation is unknown in this case, its interest lies in the fact that even at the end of the eighteenth century villagers were not ready to accept a priest who did not respect property rights and made unreasonable

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<sup>4</sup> See *Registos da Igreja*, 1540, A.H.U.M., Braga, MS N188, fol 28.

<sup>5</sup> Processo de Padre Pires de Araujo e Silva, Abade da freguesia de Sam Joam Baptista, *Desembargo do Paço*, MS M281-57 (1786), and M 171-86 (1797), Lisbon (Arquivo Nacional da Torre do Tombo).

financial demands on his parishioners. In the opinion of villagers the priest had obligations too: the relationship between him and the parishioners involved reciprocal obligations and so if the parish was providing him with income in the form of tithes, he also had to provide the service demanded of him. This very modern approach to religious functionaries is perhaps surprising in this area at such a time, but it is one that is certainly reflected in the present-day village attitudes to the clergy.

Given the early implantation of Catholicism in Minho, it is hardly surprising that contemporary rural religion has retained many of the practices that were common to medieval - or earlier - Catholicism incorporating some pre-Christian beliefs and practices in its orthodoxy, which are now disowned by the present 'modern' Church. This change in the Church's tolerance for what may seem to many to be 'folk' practices, coming as it did later in Portugal than in other western Catholic countries, has introduced a new element of tension and dissension between clergy and parishioners. While parish priests are happy, and indeed obliged, to continue performing the main religious rituals of the Catholic faith - baptism, marriage, burial, and the weekly Mass - they are increasingly unwilling to take part in or condone other religious events that seem to the villager to be just as important. One priest, in the 1920s, comments on how canon law now frowns on local customs, disapproving of Corpus Christi processions, *romarias*, *ladainhas*, *cereos* and village festas, all of which are dying out, due to 'the presumptuous and fanatical spirits of the intellectuals' (*'espíritos fanáticos presumidos de sábios'*).<sup>6</sup> Most of the rituals now disapproved of by the Church centre on agriculture, the family and the community - three concepts totally integrated in the peasant's world view. Many of these rituals are common to villages in a wide area of upland Minho: I shall here look only at rituals surrounding human death itself and omit agricultural rituals designed to ward off disease, illness and death in plants and animals.

Rituals outside Catholic orthodoxy involving the village community as a whole have disappeared from village life during this century. Two practices, presently remembered only by the middle-aged and elderly, called for the participation of all village adults: the *'encomendação das almas'* was a public gathering in the church each day during Lent when the dead members of the village were named individually, house by house, and commemorated in prayer for their souls - a ritual that lasted about an hour. Today there is still a gathering in the church each day during Lent when the Rosary is recited for the souls in Purgatory, usually without the presence of the priest and led by a rota of women. It does not now draw all village members and tends to be attended mainly by women. The other community ritual remembered only by the elderly (suggesting that it stopped before 1920) was

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<sup>6</sup> Padre Francisco Manuel Alves, *Tras-os-Montes*, Lisbon 1929, p.20.

the *rezadas em comum*.<sup>7</sup> Here the villagers would meet in the open air to pray to Saint Sebastian on January 20th. Married men presided in order of their marriage each year; each individual present was called forward to say a prayer '*em louvor do mártir São Sebastião*'. As long as the prayers continued, the day was considered a holy day, and work could start only when the ceremony had ended.

While some community rituals have dropped away, others have been abandoned by the priest, much to the annoyance of parishioners, and this reluctance to keep up the old rituals contributes somewhat to an increase in anti-clericalism. Previously the priest would perform the prayers for the souls (*reza das almas*), which involved walking around the outside of the church with a representative from each house that had a death to commemorate, doing the *obrigacoes*. The practice has been abandoned but the clergy still receive payment for it. Masses for the dead are not free and in 1972 cost 50\$ (about £1 sterling). Given the contraction of parishes, and the need for a priest to cover many parishes there are not enough days to perform individual masses for the dead, with the result that priests combine them and perhaps celebrate three or four in a day. From the villager's point of view this is another area where he is being short-changed: the priest who in 1972 was getting 150\$ daily just in masses for the dead was already receiving the equivalent of a manual worker's daily wage.

While many traditional community rituals have disappeared with or without the Church's aid, people still commemorate the dead every day; there are *alminhas* - small painted niches - in the house walls, and each time one passes one is expected to say an Our Father for the souls in Purgatory. Crosses and calvaries (*cruzeiros* or *calvarios*) are also daily reminders of death, although they also have political significance - the cross on top of the pillory (*pelourinho*) marking the limits of judicial power or the territory of a church or parish limits.

Stone cairns in the hills or by the roadside are put, stone by stone, by passers-by praying for the person killed in an accident or by violence. Wooden or stone crosses on the roadside or in the fields mark places where death has occurred: and one is expected to stop and say an Our Father for the souls of the dead in Purgatory. Villagers then have daily reminders of death in their working environment. Beliefs surrounding death are but a part of the religious system sustained by the Minho villager.

Apart from those religious rituals that were once part of Church orthodoxy and are now largely discouraged by the clergy - and therefore to a greater or lesser extent abandoned by the faithful - there is the whole gamut of semi-religious practices and beliefs surrounding illness, curing and the occult towards

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<sup>7</sup> Tude de Sousa, 'Regimen Pastoral da Serra do Gerez', *Portugalia* [Porto], Vol.II (1908), p.7.

which the Church has long turned a blind eye, since they do not require priestly involvement. Here it is relevant only to say that the disapproval shown by the younger clergy is based on their dislike of the juxtaposition of acceptable symbols, such as holy water or the branches blessed at Easter, with the unacceptable symbols such as garlic, bramble-bush branches, or cow-dung; the same 'holy water' is used for blessings in orthodox ceremonies and in many of the curing rituals as one of the ingredients for infusions; olive branches, blessed at Palm Sunday, are another common ingredient for healing rituals; the sign of the cross is common also to both. When modern clergy dismiss these rural curing rituals as 'pagan' they discount the fact that they are ultimately both ideologically and ritually within the tradition of Catholicism, and are contained within the same belief system.

#### IV. *Social Aspects of Burial/Death Ritual*

Of the three life crises of individuals, while birth and baptism are regarded as a family matter, marriage and funerals conspicuously involve not only the family but the village households. The social identity of the individual is linked to his house, his name, his family and his land. Traditionally the rural farming family is producer, entrepreneur and owner of its own resource base, producing for its own consumption needs and hence with a high degree of autonomy and kin solidarity, as both livelihood and social identity depend on it. In the village, family rituals, mourning practices and the family shrine incorporate the dead and the migrant in the family unit, individually named and remembered at specific times of the religious year.

Rituals emphasise the importance of the house (*Casa*) and generational continuity: they also can emphasise village solidarity between the landowning families in the village. Each household of residents or *vizinhos* (literally 'neighbours') has the right to be represented at all village weddings and funerals - and conversely the obligation to attend and contribute. The individuality of households is mitigated and controlled by the economic organisation of the village that demands co-operation from all residents of the land-group and is symbolised by constant interaction in the ritual sphere.

Much of traditional ritual is designed to purify and protect the house - for example the *defumadas*, the Easter blessing with holy water, to protect the house and the family within from witches and evil spirits - and to emphasise the unity of the household and its separateness from other houses in the village. These recurring rituals of purification, protection and separation

reappear throughout Galicia, in Spain as in Portugal.<sup>8</sup>

All church jobs and chores, other than the priest's, rotate between household heads according to a fixed order; there is no controlling clique to organise *fiestas* or everyday rituals, and little extravagant expenditure in an area with limited resources, small surplus, and whose demonstration of faith through ritual is aimed at preserving and safeguarding the community of households.

The emphasis throughout local social organisation is on 'the house', the *casal* paradigm of Galicia. Sometimes this social reference group is coextensive with the family but not always. 'House' names are those most commonly used to identify individuals; surnames - called locally 'church names' - are used only on official occasions when dealing with outsiders. Men marrying into 'houses' of old-established village families take on the name of the house; men who start from scratch and create a 'new house' are known by their first name or family name. Actual houses in fact change hands quite often and most families in the village have lived in more than one house in the course of their family cycle. But for the older houses the names of the houses stay the same and couples moving into them are known by the name of the house and not by their church name. Alternatively, a man or woman sometimes carry with them through their lives the name of the house they have grown up in, even though they have moved out of the family home. So there is a certain social ambiguity at play despite the apparent rigidity of social organisation.

There is some variety in the significance that the *casa* or house and the family have in day-to-day village life. For some people, the building they live in and the house they are identified with has been theirs since time immemorial, and will continue in their families except through the bad luck of childlessness or the unwillingness of children to stay in the village. For others, such as those who married into a house and took on the name of the house, the strength of the self-identity varies. For others who made 'poor' marriages and who have never experienced or have lost the association with an 'important house', either fortune has come to them through luck and their own hard work, thus earning them their own status and recognition; or they have had poor luck and the children they might have been able to lean on have also let them down. Although on the ideal level the 'house' is the important determining factor in assessing status and prestige, in a situation where the values placed on land and continuity are changing because of the recognised low productivity of the land, and with the new alternatives provided by migration and education, even the status of 'the house' that before seems to have been firmly based on quantity and quality of land owned, as well as the good behaviour of those in it, has become fluid and ambiguous.

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<sup>8</sup> For Spanish Galician house-protection rituals see Carmelo Lison-Tolosana, *Perfiles Simbolico-morales de la cultura Gallega*, Madrid: Siglo Veintiuno 1974, pp.101-2.

For each individual landowning household the death of the head of household means the transference of land from the old to the new landholder. Even when *partilhas* - land divisions - are done beforehand they are not effective until the death of the head of household. *Partilhas* are done where possible within the family or within the village, so as to keep lawyers and bureaucrats out of it.<sup>9</sup> But the degree of litigation or rather litigious behaviour of a community varies within the north: land, and its satisfactory devolution, is of primary concern to heads of households, so it is more usually resolved well before death. From a sample of wills from São João the earlier emphasis of wills is on personal salvation. From the earliest parish records people have left money for masses to be said for their souls, often when they make no further provision or will. For example, Dionyzia Dias, in 1709, left money for masses to each of six priests, to be said in the church and chapels of her parish, plus money for three sung masses to be celebrated in Braga. Several wills specify the number of priests wanted to recite the masses for the dead, suggesting both that relatively large sums of money were left to fulfil those death rituals, and also that there were many priests available at parish and commune level. One will of 1710 asks for masses of eight priests to be recited in the parish church, plus sung masses in Braga; one of 1716 asks for three *officios* of five priests each, plus 50 masses for the soul; in 1730, one will asks for 3 *officios* to be recited by nine priests plus two *officios* by ten priests, together with 10 masses to the saints of the parish and the Guardian Angels, plus a gift to the priest of 6000 reis 'for use in time of war'. Most masses for the dead specified for the earlier period, 1688-1800, are to be celebrated in the parish itself, but sometimes other churches or chapels are specified, including S. Pedro, Braga, (mentioned in wills of 1710, 1717, 1730 and 1750), Bom Jesus de Braga (1717, 1790), Nossa Senhora da Peneda (1718, 1748, 1790) and Senhora da Abadia (1746, 1780), the last two being important pilgrimage sites through the last five centuries at least. For the same period, where detailed provisions are recorded in the funeral registers, most masses are directed to particular saints or angels as intermediaries; most frequently it is the name saint or guardian angel of the dead person who is appealed to, but the following saints also appear regularly: St. John the Baptist, St. Michael, Our Lady of Mercy, St. James, St. Antony, N.S. do Livramento, Bom Jesus de Codeseda, N.S. de Remedios, N.S. da Peneda, N.S. da Abadia, N.S. do Rosario, and the Angels. Although since the nineteenth century and the institution of centralised state registers for deaths, such details are no longer registered in the archives, most villagers

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<sup>9</sup> Cf. Fatimá Brandão, 'Death and the Survival of the Rural Household in a Northwestern Municipality', in Rui Feijó, Herminio Martins and João de Pina-Cabral (eds.), *op.cit.*, pp.75-87.

still direct their appeals for supernatural intercession to these same religious figures.

Of wills made in the eighteenth century, before the inheritance law was changed to what it is today, such specific references to inheritance that are made seem designed either to contradict the prevailing custom of the time, or to resolve possible conflict in situations where there was no direct heir to property. For example, Custodio Martins, in 1709, left his inheritance from his mother, along with personal goods and a small cash sum (400 mil reis) to his betrothed, while willing that his grandparents should stay on his land. In 1716 an old widow left half her property to her granddaughter and the other half to the church - the only case in the three-hundred-year period for which records exists in S. João of any property being left to the church. In 1720, one man left land and goods to a cousin temporarily resident in Lisbon. Wills involving dispositions of goods and property were made, it seems, in exceptional circumstances; on the whole in the eighteenth century at least land and the house were passed on in the customary way to whichever child or children were selected and willing to stay and work on the house's land. Gifts and inheritances were accompanied by the obligation of prayer, whether in the form of masses for the dead to be said at regular intervals, over a period of years, or less formally by the observation of traditional customs and rituals by whose help the dead souls would rest in peace and eventually achieve paradise. Where no will is made it is often the son-in-law or the father-in-law of the deceased who pays the funeral expenses and the costs of the masses, perhaps a form of reciprocation for land received by his household through marriage. The inheritance of the preferential share of the patrimony imposes obligations on the recipient: the masses have to be said, the anniversaries of the death acknowledged, the priest paid. Usually seven days after burial the first mass for the soul of the dead person is celebrated, paid for by heirs or from the estate of the deceased. On the first anniversary of the death the village priest traditionally receives payment for the mass in the form of 14 *medidas* of cereal crop (rye or maize). Even after death, the deceased, especially if he had something to bequeath, is incorporated into community life. As with burial, any failure on the part of the kin to 'do things properly' is thought to jeopardise the soul's progress after death. Personal salvation is kin solidarity.

Despite the importance of the *casa* as a social unit and the existence of some wills, and the potential for social differentiation in the practice of burial (with the alternatives of burial either in the *adro* of the church or in the church itself), no clear picture of stratification emerges in the treatment accorded the dead. Taking the parish of S. João as a case example, from 1688 onwards there is no consistency in the burial pattern; while the majority were buried in the church itself, about 30% of interments took place in the *adro*. People who made wills - perhaps an indicator in some societies of greater wealth - appear equally represented in both places. Members of the same family are buried

in both places. While a day labourer (*jornaleiro*) - a Gallego to boot - was buried in the church, a local mendicant was interred in the *adro*, along with the parish priest.

The only indication that burial in the church itself might be thought to be of some social significance remains the burial of the sister of one parish priest, Padre Carvalho Coelho, who was buried 'near the church's arch, and near the altar of St Antony'.<sup>10</sup> Neither money nor social status seems to have guided the choice of resting place before the new cemetery was opened in 1940. Since then a small charge has been levied on burial plots; people are interred individually and not in family vaults, and the memorial stones that have been erected in the past fifteen years are modest, involve no great display of wealth and are much alike. It would appear that even in death, the hill villager maintains his egalitarian ideal of himself and his community of landowning neighbours: the 'good death' aims both at achieving personal salvation and achieving or maintaining kin and land-group solidarity. The ritual itself shows the importance of village representation, with all landholding households participating in the loss of a neighbour. The secular aspects of the post-death ritual are sharply distinguished from the religious 'church' aspect: the wake, the keening, the gift-giving, the burning of the deceased's clothes and bedding mark both the separation of the individual from the community of neighbours, and register the neighbours' participation in his departure.

As children are marginal to the focus on land, so they are excluded from representation at the post-death ritual. Children are 'little angels' (*anjinhos*), and as such have no part to play either in the secular or religious aspects of death. They also do not figure in post-death land redistributions, so in a sense they do not need to be represented. The priest is essential as a functionary to perform part of the post-death ritual (giving the last rites, and performing the burial service), but as the Church's representative he stays separate from the secular aspects of the funeral. After the death of the deceased he does not enter the house for the wake; his place is not at the front of the funeral procession but towards the rear, just in front of the women; the keening stops when the church service begins and resumes when it ends. Death and funeral ceremonies can be a source of friction and anti-clericalism, with priests demanding high payments, or seeming to distinguish between rich and poor, or appearing to receive payments for services not rendered.<sup>11</sup> But given the importance attached by the villager to 'proper' burial, as a functionary the priest is essential if the soul of the deceased is not to suffer an unhappy post-death existence and

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<sup>10</sup> But St Antony is also the Patron Saint of Spinsters.

<sup>11</sup> Cf. Eugen Weber (*Peasants into Frenchmen*, London: Chatto & Windus 1977), at p.357, regarding Brittany.

return to bother the kin.<sup>12</sup>

#### V. Conclusion

The 'good death' illustrates concern with both property transfer and individual salvation. The economic integrity of the household has to be maintained by an acceptable transmission of the patrimony: heirs have to be encouraged to perform the rituals in order for the soul to achieve salvation. Spiritual debts to particular saints have to be paid, as well as human debts.

Death rituals operate on three different levels: for the individual the preoccupation is salvation, but the emphasis is on salvation in the sense of safety (freedom from torment in the form of Hell or Purgatory or return to life) rather than on ecstatic joy. For the individual and the kin, the land inheritance arrangements are also a preoccupation: kin obligations that fall on several individuals but especially on the heir/heirs stress the separation of the kin and the *casa* from the other landholding neighbours.

For the village community, death and burial rituals reconcile in death the separation (and competition) of individual *casas* in other aspects of their daily life.

PATRICIA GOLDEY

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<sup>12</sup> Juliet du Boulay also mentions the importance of the rites of death in a Greek village, in connection with vampire beliefs ('The Greek Vampire: A Study of Cyclic Symbolism in Marriage and Death', *Man*, Vol.XVII (1982), pp.219-238).

CONFLICTING ATTITUDES TO DEATH IN MODERN PORTUGAL:  
THE QUESTION OF CEMETERIES

The 19th century in Portugal witnessed a succession of ideological, social, and economic transformations which, in many cases, were set in motion by a political process drawing on principles formulated earlier under the impact of the Enlightenment. The transformation of attitudes to death and burial practices is here seen in this context.

The historiography of attitudes to death and burial in Portugal has thus far received scant attention. The following comments are mainly intended as a stimulus to further research on this topic. They are an attempt to combine insights derived from anthropological research with those available from historical material. We are encouraged in this purpose by the belief that in the recent developments in the social history of death there is an evident failure to give sufficient attention to the ethnographic material already available and to the theoretical insights of social anthropology.<sup>1</sup>

While carrying out anthropological research in rural Minho (1978-1981), João de Pina-Cabral (one of the authors of this article) became aware of the existence of a process of change in attitudes to death and burial. There appeared to be a movement from what Ariès calls the *mort apprivoisée* (tamed death), to a type of death which corresponds more closely to his model of the

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<sup>1</sup> For a similar opinion, see Stephen Wilson, 'Death and the Social Historians: Some Recent Books in French and English', *Social History*, Vol.V (1980), p.443.

*mort sauvage* (wild death).<sup>2</sup> The particular interest of this process is that it is closely related to the use of cemeteries, about which historical information had been gathered by Rui Feijó (the other author of this article). This allowed us to create a picture of the momentum and character of the change over the last 150 years. The interest of the insights thus derived is enhanced by the fact that attitudes to death and burial are by no means an isolated cultural phenomenon; indeed their change in present-day rural areas is part of a much larger process of change from a peasant world-view into a world-view which is dominated by the values of the urban bourgeoisie.

Although in the course of this paper Portugal is sometimes referred to as a whole, we are aware that, in attitudes to death, as in most other aspects of popular culture, Portugal is a profoundly divided country. Since our material is mostly derived from the north, and particularly from Minho, and since we know of no corresponding research in the south of the country, we are at present unable to correct this focus on the north - a deficiency of which we are sorely conscious.

## I

In his pioneering comparative article on the collective representations of death, R. Hertz points out that

there is a close relationship between the representation of the body and of the soul. This mental connection is necessary, not only because collective thought is primarily concrete and incapable of conceiving a purely spiritual existence, but above all because it has a profoundly stimulating and dramatic character. [...] The material on which the collective activity will act after death, and which will be the object of the rites, is naturally the very body of the deceased.<sup>3</sup>

This relation between body and soul is cross-culturally widespread and it is noteworthy how it has been a constant of European attitudes to death.<sup>4</sup> In the 6th century A.D. it took a

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<sup>2</sup> Philippe Ariès, *L'Homme devant la mort*, Paris: Seuil 1977.

<sup>3</sup> Robert Hertz, *Death and the Right Hand* [transl. R. and C. Needham], London: Cohen and West 1960, p.83.

<sup>4</sup> Cf. S. Wilson, *op.cit.*, p.447, and Pierre Chaunu, *La Mort à Paris - XVI<sup>e</sup>, XVII<sup>e</sup> et XVIII<sup>e</sup> siècles*, Paris: Fayard 1978.

new perspective when the Christian faithful started to manifest a desire to be buried *ad sanctos*; that is, it was believed that spiritual benefit for the soul in the afterlife was to be derived from being buried near the relics of a martyr. In time, this attitude was somewhat modified and there came to be a desire to be buried in or near the churches, *apud ecclesiam*. Paradoxically, however - considering the ubiquity of this practice in Europe - the Church has always formally prohibited burials inside church buildings. As early as 563, the council of Braga had expressly forbidden this practice and this position was formally maintained by the Church authorities until the 18th century.<sup>5</sup> It seems, however, as Ariès points out, that 'the only effect of the canonical ban was to submit the widespread practice of inhumation inside the churches to the payment of a fee, while, at the same time, preserving a principle.'<sup>6</sup> But if for the theologians there was an intrinsic difference between burial inside the churches and burial in churchyards, for the faithful this distinction did not apply. Burial in the churchyards was conceived of as a continuation of burial inside the churches. There was merely a gradation of value between being buried near the altar and being buried further and further away from it. In this sense, outside or inside the walls of the church made only a quantitative, not a qualitative difference.

In the beginning of the 19th century in Portugal, burials took place in or around the parish churches. This was the case both in the cities and in the countryside.<sup>7</sup> When there was no space for all within the churches, only the wealthy received the preferential treatment of lying under the church roof, the majority of parish members being buried in the churchyard, either in communal or individual graves. Although our data do not allow us to provide any quantitative estimate,<sup>8</sup> we are in a position to

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<sup>5</sup> Ariès, *op.cit.*, p.53.

<sup>6</sup> *Ibid.*, p.56.

<sup>7</sup> For a rather detailed description of burials in Portugal at the time, see A.P.D.G., *Sketches of Portuguese Life, Manners, Costume and Character*, London 1826, chapter XIII, 'Portuguese Funerals', pp. 237-249. See also Patricia Goldey, 'The Good Death: Personal Salvation and Community Identity', in Rui Feijó, Herminio Martins and João de Pina-Cabral (eds.), *Death in Portugal: Studies in Portuguese Anthropology and Modern History*, Oxford: JASO 1983; or above, pp.1-16.

<sup>8</sup> See, however, Geralda Maria Marques Ferreira dos Santos, *A Freguesia de S.Martinho de Arrifana de Sousa de 1700-1729*, Lisboa: Publicações do Centro de Estudos Demograficos 1981, pp.149-150; Maria Lucilia de Sousa Pinheiro Marques, *A Freguesia de S.Martinho de Arrifana de Sousa de 1730-1759*, Lisboa: Publicações do Centro de Estudos Demograficos 1974, pp. 133-135; Maria Celeste dos Santos Duarte de Oliveira Duarte, *A Freguesia de S.Martinho de Arrifana de Sousa de 1760-1784*, Lisboa: Publicações do Centro de Estudos Demograficos 1974, pp. 160-162.

suggest that, at least in Minho, when burials in churches were eventually banned by law, churchyards played an intermediate role between burials inside churches and burials in the cemeteries. Furthermore, and again with the exception of the wealthy and famous, there were seldom any outward markings to specify where each person had been buried, and families did not own or keep for their private use any part of the burial grounds.<sup>9</sup>

Throughout Europe, in the 19th century, the practice of burying people inside churches and in churchyards fell under criticism. Such criticism bears witness to the emergence of a new attitude to death, one which saw death as far more threatening. While the traditional attitude had been characterised by a familiarity with the dead - both in physical and spiritual terms - and by a stress on the public nature of death, the new attitude privatised death, attempted to limit the expression of mourning to the close family, and marginalised the role of the dead in public life. We find it useful to conceive of this change in terms of Ariès' model of the opposition between *mort apprivoisée* - the traditional type of death - and *mort sauvage*, the new attitude which was espoused by the bourgeois, liberal intelligentsia. We might be accused of simplifying Ariès' analysis of this historical process by reducing it to a simple polarity. It must be remembered, nevertheless, that, on the one hand, his analysis is mainly concerned with changes of attitudes amidst the European élites, and that, on the other hand, the model remains useful for it allows us to plot the specific attitudes to death which we encounter within a continuum between these two opposites, without necessarily reducing them to either.

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<sup>9</sup> See, however, William Kinsey *Portugal Illustrated*, London 1828, p. 190 ('The floors of the churches in Portugal are generally divided into so many oblong squares into which are fitted trap-doors of wood, which are easily raised. They overlay the ordinary places of separate interment, and are very frequently after the burial of an individual, covered with a piece of black cloth, which is rudely nailed on, as a testimony of mourning.') W.A. Douglass, in his book *Death in Murelaga* (Seattle and London: Washington University Press 1969) reports that each household in this Basque region owned a burial space inside the parish church. Could a similar practice have taken place in Portugal? This system would appear at first to combine both tendencies which we are here opposing. We must not, nevertheless, confuse 'household' with 'family' - for these are rather different concepts. The differences between the implications of using each of these concepts as the elementary unit of social life are rather considerable. (For a discussion of this in relation to the Alto-Minho, see João de Pina-Cabral, *A Worldview in its Context: Cultural Uniformity and Differentiation*, Oxford, D.Phil. Thesis, 1981, chapter entitled 'Household and Family'.)

II

In Portugal, the new attitude only made a real impact after the Napoleonic invasions and it came to be enshrined in law in 1835 and again in the so-called Health Laws of 1844. For the bourgeois thinkers who were behind these laws, death had become *savage*, for they lacked the ritual and conceptual structures to integrate death within the social realm.

In order to understand this change we have to refer to the ideological context within which it took place. The 18th century had witnessed the growing impact of the concept of 'preventive medicine' on medical thought. Good health was no longer seen as something doctors had to restore once someone had been afflicted by disease. Rather, it was felt to be necessary to preserve it. Concomitantly, disease was regarded as avoidable within certain limits, its causes no longer being credited to God's will alone.

But individual life is an eminently social phenomenon. Once it had been discovered how diseases are spread, the need to prevent them was recognised. The maintenance of public health called for the intervention of social entities capable of isolating the *foci* of disease. In a word, the development of preventive medicine goes hand in hand with an increased intervention of the 'health authorities' - that is, state institutions. Death, therefore, was closely associated with health. We shall, once again, borrow a term from Ariès who refers to this process as the 'medicalisation' of death.<sup>10</sup>

The idea that, once the flow of life had ceased, bodies started to decompose, liberating *miasmas*, was very popular among the authors of the late 18th and early 19th century. A *miasma* is defined in the O.E.D. as 'an infectious or noxious emanation'.<sup>11</sup> There is no doubt that these 'emanations' were regarded as a threat to public health, since they had the power to fill the air and gain a hold over every living creature they touched. Therefore, contact between the living and the dead was to be avoided, and dead bodies isolated and kept in places where their *miasmas* would not reach the living. This prompted regulations about the time mediating death and burial, and regarding the places of burial. In all instances, death was regarded in physiologic terms - no matter how incorrectly in today's views - and life was considered as something to be preserved. This *valuation* of life, so important to the evolution of medicine, was being associated

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<sup>10</sup> Cf. also John McManners, *Death and the Enlightenment*, Oxford: Clarendon Press 1981, ch. 2 ('Defences against Death: Eighteenth-century Medicine'), pp. 24-58.

<sup>11</sup> For a Portuguese translation, see under 'miasma' in Antonio de Moraes Silva, *Grande Dicionário da Língua Portuguesa* (10th edition), Lisboa: Confluência 1954.

with a *devaluation* of death. The belief that 'Life and Death are the gifts of God' was replaced by an attitude which stated that 'God gave us Life for us to preserve'.

At this point, we have to insert a caveat. For, even if these views are nearer to ours than those of the popular strata of the early 19th century, they should not remain outside our critical attention. As such, their significance as a system of symbolic classification must not be overlooked.

As R. Hertz first pointed out, death corresponds in all societies to a moment of marginality or liminality, one which presents a threat to the group, as it creates a gap in the social order. As such, it is not intrinsically different from other moments in the developmental cycle of the social group, where individuals pass from one status to another such as birth, initiation, or marriage. Social groups manifest a need to conceptualise these moments of passage and to deal with them in ritual terms, these being Van Gennep's *rites de passage*. Two aspects of Van Gennep's cross-cultural analysis of funerals seem particularly relevant to our argument. First, the fact that of all rites of passage, funerals are those which most strongly stress the theme of transition (an insight derived from Hertz); secondly, that 'of all the rites of passage funerals are most strongly associated with symbols that express the core of life values sacred to the society at hand.'<sup>12</sup>

It is not only moments of passage in the life of individuals, however, which present such a threat to the conceptual order of the social group. This feature is shared by many other phenomena which are similarly characterised by the gap they create in the conceptual order which the society imposed on the cosmos. Mary Douglas, in her book *Purity and Danger* (Harmondsworth 1966), has demonstrated that such phenomena are commonly characterised by the social group which confronts them, as impure and dangerous. She argues that there are five basic means by which societies can cope with these, so to speak, abhorrent facts: re-definition, physical control, avoidance, imputation of danger, and finally ritual or artistic utilisation. This last category is perhaps the most interesting for, according to her, society is seen to utilise the power which such abhorrent phenomena derive from the threat which they present to the conceptual order as a means of re-enforcing that same conceptual order.

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<sup>12</sup> Richard Huntington and Peter Metcalf, *Celebrations of Death: The Anthropology of Mortuary Ritual*, Cambridge: Cambridge University Press 1979, p. 19. In ch. 4 of this book, the authors develop considerably Van Gennep's original insight, using material from Madagascar.

Now, it was precisely this process which characterised the attitude to death of the popular strata of the early 19th century in Portugal, the attitude which the 'enlightened' liberals so profoundly questioned. In the context of the *mort apprivoisée*, death remained threatening and impure - a point that is not being denied here.<sup>13</sup> What allowed for the 'familiarity' with and 'public nature' of death, which distinguished the *mort apprivoisée* from the *mort sauvage* of the liberals, was, rather, the ritual utilisation which was given to it. Here, Van Gennep's insight as to the fact that in funerals society expresses and re-enforces 'the core of life values sacred to it' assumes its significance. The prime mover behind the liberals' change in attitudes is precisely the fact that this ritualisation of death had lost its sense for them. F. Steiner, talking of the Victorians, makes a point which could equally well be applied to the Portuguese liberals of the early 19th century: 'the more the links, props and joints of the socio-religious thought structure were absorbed into theories of rational ethics, the more isolated became the little islands of prescribed ceremonial behaviour.'<sup>14</sup>

Even if death had already been impure for those who experienced it as *apprivoisée* (witness the ban applied to close mourners which prevents them from attending church services for the week following the death), the fear of contact with the dead, however, was expressed far more strongly by those who espoused the new and urban attitude to death - the *mort sauvage*. In this context, it is worth noting that the justification given by the liberal legislators for the building of cemeteries away from churches and from inhabited zones, as well as for the raising of the high walls which, today, are characteristic of Portuguese cemeteries, was that these were unhygienic and might lead to the spreading of dreadful diseases. As Ariès comments in relation to France, it is not very clear whether churchyards were indeed a real hazard to public health, or whether the indignation expressed by the 'enlightened' men of the late 18th and early 19th centuries was not motivated by a new symbolic categorisation of death which saw it as a particularly impure and unseemly phenomenon.<sup>15</sup> As Mary

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<sup>13</sup> Wilson (*op. cit.*, p. 444) criticises Ariès for presuming that this was not the case: 'it is clear that in societies of the more recent past, death was feared as much as it was accepted, and that a certain taboo did surround it...'. Nevertheless, we feel that, once this point has been made, Ariès' general idea still remains useful.

<sup>14</sup> F. Steiner, *Taboo*, Harmondsworth: Penguin 1967, p. 51; also quoted by Huntington and Metcalf, *op. cit.*, p. 10.

<sup>15</sup> Philippe Ariès, *Essais sur l'histoire de la mort en Occident du Moyen Age à nos jours*, Paris: Seuil 1975, p. 159.

Douglas argued in *Purity and Danger*, we have to question our own Western attitudes to hygiene which, although they are based on revolutionary discoveries in medicine, are all too often used as an ideological umbrella for the expression of the perception of a kind of impurity which is basically rooted in symbolical considerations.

If we accept this view, then, the legislative effort of the Portuguese Liberals of the first half of the 19th century and the subsequent development of new burial practices can be seen in a new light. Those who espoused the *mort apprivoisée* dealt with the threat which death presented to society by the ritualisation of death. In this way, death was appropriated by society as a means of expressing 'the core of life values sacred to it'.<sup>16</sup> Those who were buried inside the churches or in churchyards remained at the very centre of the social life of the community to which they had belonged. When the living gathered for prayer, the dead were beneath their feet, thereby emphasising that membership of the religious community created a unity which reached beyond the grave. To this day, in rural Minho, the comments of the *almocreve* in Júlio Dinis' 19th-century novel *A Morgadinha dos Canaviais* can still be heard: the graves do indeed look nice, like those in the cities, but the cemetery is a cold and wet place, where the dead are left alone, uncomfortable, and unprotected.

For those who started experiencing death as *savage*, however, the symbolism behind the traditional ritualisation of death was no longer sufficiently strong to deal with the threat which death presented to the social group. The dead, therefore, had to be physically marginalised, for their impurity was no longer controlled and co-opted by ritual means.

### III

The view of the Liberals had a lasting effect in the development of attitudes to death in Portugal as a whole. Nevertheless, these views remained a feature of a specific group within the larger society. We do not pretend that these transformations broke what one might call a nationally unified pattern of belief - which has probably never existed. Precisely what prompted this study was the discovery that the attitudes towards death, dying, and the

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<sup>16</sup> For a more extensive discussion of this process in the Alto-Minho, see João de Pina-Cabral, *op.cit.*

dead of the peasants of the Alto Minho are still today rooted in a pre-Liberal view of death. But the crucial fact is that the group who accepted the new set of ideas, no matter how systematically, happened to dispose of an extremely effective resource: political power. This was the case after the victory of the Liberals over the Absolutists in the 1830s.

The Liberals held an unqualified belief in the powers of Law as a tool for social change and for the shaping of society. This may not be an individualizing feature of Portuguese political history, but it certainly was a hallmark of Liberal political philosophy. Oliveira Martins, although very critical of the Liberals' views, wrote the best acknowledgement of this fact when he referred to the legislation of Mouzinho da Silveira in the following terms: 'The laws of the 16th of May, 30th of July, and 13th of August [1832] are our 1789.'<sup>17</sup>

The Liberals legitimised their policies evoking several principles, among the most important of which was the concept of the 'lights of the century'. From a political stand-point the 'lights of the century' consisted of the principles of human rights and the tripartite distinction of powers. The attempt to put these principles into practice led to a growing complexity of the state's functions. Perhaps the most commonly quoted example is that relating to literacy and the school system. But we can add the implementation of a national network of 'health administrators' suggested by the 'lights' of medical science. There, the State met death.

Pascal Hintermeyer<sup>18</sup> has argued that the concern of the State with death could have its own, specific rationale, the 'control of death' entailing the extension of the State's authority over the living - be they individuals or groups - and their minds. This was certainly not an expressly stated concern of the Portuguese Liberals, although it was in keeping with their attitude of ignoring diversity and imposing uniformity. This becomes evident in their attitude towards religion.

The first bill passed on the issue of death and cemeteries was dated the 21st September 1835, and it states that the traditional ways of burying the dead were 'superstitious' and 'offensive to the respect and veneration due in holy places'. Rodrigo da Fonseca Magalhães, the minister who submitted the bill, explains there that it was due to the 'ignorance of the Middle Ages' that such a 'shameful abuse' could have been transformed into a 'religious duty'. He continues

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<sup>17</sup> J.P. Oliveira Martins, *Portugal Contemporâneo* [1882], Lisboa: Guimaraes e Cia 1976 (8th ed.), p. 352.

<sup>18</sup> *Politiques de la Mort*, Paris: Payot 1981.

Even in those days of obscurity and harshness of costumes the voice of the enlightened religion, free from prejudice, could be heard against such practices.

And he proceeds to quote the bans on burials inside churches made by the councils of Braga (563), Meaux (845), Tribur (895) and Reims (1117) - in this way demonstrating an uncommon theological erudition. Thus, for the Liberal legislators there was no question of attacking religion. Reason and Faith were not reputed incompatible. For the Liberals there was rather the need to clean up popular religion. This was possible in so far as they regarded the latter as basically centred on a core of sound Roman Catholicism from which some principles had been extracted and a lot of 'superstitions' added.

This view of popular religion which persists to this day<sup>19</sup> denies it a basic unity and identity and as such greatly misunderstands its nature. Even if, to the eyes of a literate person with a theological training, popular religion may appear to lack consistency as it fails to be structured in a strictly logical fashion, its basic unity and identity cannot be denied. To attempt to 'purify' it by subtracting all the 'superstitions' or 'pagan' elements and leaving the rest behind, is to fail to understand its unitary nature. It is precisely why such an attempt was perceived as a full-scale attack on religion, and released the fury of the people in 1846. Being convinced that 'the wise words of a handful of enlightened men are lost amidst the general blindness',<sup>20</sup> the Liberals took the risk of enlightening the blind by force. History did not accommodate their dreams. The data on cemeteries which we present below and the Maria da Fonte Revolution, which has been discussed elsewhere by Rui Feijó,<sup>21</sup> are the clearest signs of this failure.

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<sup>19</sup> For a detailed description of this attitude as it is manifested in Minho today, see João de Pina-Cabral, 'O Paroco Rural e o Conflito entre Visões do Mundo no Minho', *Estudos Contemporaneos* [Porto], Vol. II (1981), pp. 75-110. On this issue see also Antonio Joaquim Esteves, *Religião Popular: Formas e Limites do seu Poder Constituinte*, Porto 1977.

<sup>20</sup> Rodrigo da Fonseca Magalhaes, Law of the 21st September, 1835.

<sup>21</sup> Rui Graça de Castro Feijó, 'Mobilização rural e urbana na Maria da Fonte', in Miriam Halpern Pereira *et al.* (eds.), *O Liberalismo na Península Iberica na primeira metade de século XIX*, Lisboa: Sá da Costa 1981, ii, 183-193. See, in the same volume, Maria de Fatima Sá e Melo Ferreira, 'Formas de Mobilização popular no liberalismo - o "cisma dos monacos" e a questão dos enterros nas igrejas', pp. 161-168.

## IV

It is not easy to create a detailed picture of the practices of burial in Portugal in the early 19th century since they were not specifically recorded by the participants. The accounts of attitudes to death and burial at this time given by British travellers, however, prove to be particularly interesting in relation to this issue. These were written by Protestants whose attitudes to death were already akin to the *mort sauvage* and who were deeply shocked when confronted with traditional, Catholic burial practices. We shall base ourselves in particular on the account given by A.P.D.G. in his *Sketches of Portuguese Life, Manners, Costume, and Character* (London 1826), as we feel it deserves particular attention because of the author's evident knowledge of Portuguese, his sharpness of observation, and the general wealth of information conveyed. The obvious distaste which he expresses for what he describes can be taken as an assurance of partial disinterest. He is certainly led to describe facts which, today, would shock most of us but which, for the participants, at the time, were commonplace.

Two aspects of death and burial in Portuguese towns at the time appear to have shocked him most. These are precisely those characteristics of the *mort apprivoisée* which Ariès considers to be the most central: the familiarity with death and the dead, and the public nature of the process of dying.

After having described the ceremonial preparations which were undertaken when a person was seen to be approaching death, A.P.D.G. comments:

When the procession [with the Holy Sacraments] arrives at the sick person's door, the canopy and the lanterns are rested against the wall, and every one goes into the room, not excepting even the rabble who, more out of curiosity than devotion, have followed the host. For such is their incontestable right: and thus are the sufferings of the dying exposed to the gaze of an uncompassionating mob.<sup>22</sup> It is true that the Portuguese see this in a very different point of view from that which we take, when we attach ideas of decency to dying peaceably and quietly. They deduce very advantageous consequences from a numerous collection of persons round the sick bed; as they conclude that,

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<sup>22</sup> 'A revolting custom universally prevails here of suffering the street door to remain open on such occasions, to the intrusion of every rude and careless observer', [Anon.], *The World in Miniature: Spain and Portugal*, (Frederic Shoberl, ed.), London 1827, ii, 239.

however scanty the prayers of each may be, every little aids; and no one is therefore excluded.<sup>23</sup>

Death was public in the sense that everyone was encouraged to participate in the process of each person's death (as it is still the case in the rural parishes of Minho), but also in the sense that the familiarity that was felt towards the bodies of the dead made it unnecessary to rid the public eye of their presence. Indeed, it made no sense for the Portuguese city-dwellers of the early 19th century to bury someone without religious ceremony. For this, however, the presence of priests was necessary and these had to be paid for. The poorest, therefore, were reduced to having to beg for alms after a death. This the English travellers found shocking. A.P.D.G. tells us: 'Many are seen laid out just in the state in which they died, with an earthen bowl upon their bodies, to receive the alms of the people who may chance to pass the way',<sup>24</sup> and, again, another traveller: 'it occasionally happens that a dead body is seen lying on the back in the open streets, with a little cup or pan placed upon the breast, for the reception of voluntary subscriptions to defray the expenses of burial'.<sup>25</sup>

Indeed, still today, the burial of anyone without religious ceremony in the eyes of the *minhoto* peasants is nothing less than absurd. It is via the ceremonial actions of the representatives of the Church that death is ritualised and is appropriated by the social group. In the deeply conservative region where João de Pina-Cabral carried out fieldwork there was an upsurge in 1976 against the local presence of members of the Portuguese Communist Party. In one town, a local man who had expressed pro-communist views was attacked by a mob and killed. It was then necessary to bury him, for which purpose the priest was of course called. Following the orders of the Archbishop, however, the priest refused to bury him if any Communist Party flags were flown at the funeral. In the middle of the funeral procession someone brought out a flag, at which point the priest went home. This caused such havoc that it was necessary to call another priest and, this time, no one dared to show a flag. The man was finally buried as a Christian, to everyone's satisfaction.

The construction and utilisation of cemeteries, something which was imposed by law when the liberal bourgeoisie came into power in the 1830s, presented a direct threat to the population. In the cities (as opposed to the rural areas), this was less true, for, by the middle of the 19th century, attitudes were already changing, and cemeteries had become necessary as a result of rapid demographic increase.

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<sup>23</sup> A.P.D.G., *op.cit.*, p. 239.

<sup>24</sup> *Ibid.*, p. 246.

<sup>25</sup> [Anon], *The World in Miniature*, ii, 242.

Two innovative aspects are noteworthy in relation to burials in cemeteries. First, cemeteries are apart from churches even though they are holy ground. This represents a change in attitudes in which the previous familiarity with the dead is transformed into a stronger need to separate the realm of the living from that of the dead. The congregation no longer prays on top of its dead. Interestingly, as the *minhoto* peasants started to adopt urban practices and build cemeteries, they found it nevertheless impossible to give up completely the previous sense of familiarity with the dead which was an expression of the strong feeling of parish community. When cemeteries were built, they were placed as near to the churches as possible, in the path which the processions follow when they go round the church and the *cruzeiro* (big stone cross) on feast days. This is, in fact, in contradiction of the original Health Laws which specified that a cemetery should be on the borders of inhabited zones. In none of the cases presented in Table 3 below was the cemetery further than 100 yards from the church. Furthermore, in many parishes, neighbours always go to visit the dead after Mass on Sundays, thus re-establishing the link which the building of the cemetery had threatened to sever.

The second feature of cemeteries which represents a radical change in attitudes is the existence of separately marked graves belonging to different families. As Ariès pointed out,

The need to reunite in perpetuity, within a separate and closed space, the dead of a family corresponds to a new feeling which eventually spread to all the social classes during the 19th century [in France]; the affection which ties the living members of a family is extended to the dead.<sup>26</sup>

This tendency seems to have begun in France in the 16th and 17th centuries. At that time, and particularly among Protestant families, there appeared a new tendency to stress what Natalie Davis calls 'the family arrow in time and space', by means of a concern with family history, a limitation of active kinship links, and a stronger concern with family planning. Among Catholics this tendency was checked by a strong concern with inter-familial, communal links which were expressed in 'the traditional Catholic forms [of burial which] were connected directly or symbolically with corporate institutions hardly moribund in the 16th and 17th centuries, such as village assemblies and vestries, professional groups and craft guilds, confraternities and the like'.<sup>27</sup>

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<sup>26</sup> Ariès, *Essais*, p. 153 (our translation).

<sup>27</sup> Natalie Zemon Davis, 'Ghosts, Kin and Progeny: Some Features of Family Life in Early Modern France', *Daedalus*, Spring 1977, pp. 87-114; at pp. 99-100.

In cemeteries, neighbours are not placed in an indistinguishable group as they had been in churches or, in some cases, in churchyards; people no longer pray for the parish dead, but for 'their own dead' (*os nossos mortos*) - the dead members of their family. The two tendencies, however, co-exist - the pull of the community and the pull of the family. This co-existence, which is not a recent phenomenon, is reflected in burial practices. The burial in separate, decorated family graves in the cemetery merely comes as an indication of the progressive weakening in rural Minho of the 'feeling of community'. Nevertheless, this tendency should not be exaggerated. Ariès reports that the practices attached to the day of All Saints changed considerably during the 19th century. Previously, the physical presence of the tomb, he argues, had not been necessary.<sup>28</sup> In rural Minho the practices followed these days are very similar to those in urban settings where death is more feared, but with at least this one significant difference: while in the cities people visit the graves of their family and those of their close relatives, in the rural areas the visit to the cemetery is used as a ritual of parish unity and the graves visited, apart from that of one's own family, are those of one's favoured neighbours (the 'friends'), and of neighbourly households which had a death in the course of that year.

## V

At this point, we have to distinguish between the ritual utilisation of death as an expression of a 'feeling of community' attached to particular social groups - such as the parish in rural Minho - and death as an expression of a feeling of identity or equality among all men - something akin to Turner's concept of *communitas*. The expression of a kind of *communitas* at the moment of death is central to Catholic thought. Anyone who has lived in

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<sup>28</sup> Wilson, *op.cit.*, p. 444, argues against Ariès, saying that 'there is reason to believe...that the Day of the Dead was a traditional and not a modern festival, and that people visited and dressed graves collectively on this and other occasions, as well as leaving ritual offerings of food and drink for the dead'. For a discussion of All Souls' and All Saints' Days that relates them with the expression of *communitas* which is discussed below, see Victor W. Turner, *The Ritual Process: Structure and Anti-Structure*, Harmondsworth: Penguin 1969, p. 171.

Portugal for any length of time is familiar with the expression of this attitude in statements such as 'in death we are all equal', or 'we all land up here, poor and rich alike', etc. Another such manifestation is found in the shrines to the Souls of Purgatory which line the roads in the north of the country, where one often finds amidst the flames kings, bishops, men, women, old and new - all naked or partially so, and all similarly burning. But, while in the countryside this attitude is deeply related to the expression of the 'feeling of community' which unites all the neighbours of a parish, in the urban areas the identity among all persons is manifested independently as a value in its own right. The death of each is a *memento mori* to the others, as it reminds them of the basic identity of their fates and the vanity of their strife for difference. In a chapel in Evora where the walls are completely lined with bones, there is an inscription which is placed in such a way as to be the last thing the visitor sees before he leaves. It manifests this conception powerfully and concisely: 'The bones that here lie, for yours await.'

In the context of Portugal this separation between the expression of 'community' and of 'communitas' seems to apply to all urban groups even when they espoused the *mort apprivoisée*. This is evident from the English travellers' accounts of burial practices in early 19th-century Lisbon, cited above. In spite of the public nature of death, and in spite of the expression of a symbolism of 'communitas' in the burial practices, in the cities, death is more a manifestation of differences between men than of 'community of interests' and therefore of active cooperation. Once again we are speaking of a continuum between two poles, for even in the most isolated *minhoto* parish where the death of one neighbour involves practically all the other neighbours in manifestations of mourning, the rich receive a 'better' or 'prettier' (*mais bonito*) burial than the poor. There is, however, a resistance to the expression of this difference which is very strongly felt. A student of one of the authors, who is from Minho, said that he caused panic and anger among his rural neighbours when, on coming back from the city for his uncle's burial, he insisted on the use of a motorized hearse for which not every one could afford to pay.

But, as in all such cultural areas where ideology enters at least partially into conflict with the material state of affairs, the symbolic expression of values is here rather contorted. In Minho, one finds many manifestations of the kind of symbolic somersault which Natalie Davis encountered when she speaks of the 'ostentatious doles to the poor' which were given by the rich under the guise of a feeling of human brotherhood, but which were really manifestations of status inequality.<sup>29</sup> Similarly, in rural Minho, one finds the practice of calling more priests to attend the burials of the rich than those of the poor. The reason

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<sup>29</sup> Davis, *op.cit.*, p. 94.

for this is ostensibly that the services of more priests procure a greater benefit in the afterlife. The poor neighbours, however, explain it differently, saying that the rich should have more priests, not so that they should have a spiritual advantage, but because by virtue of being rich they have more sins for which to answer. The utilisation of a greater number of confraternity flags as a mark of prestige, and the presence of a greater number of people at the burials of the rich and famous, may be interpreted in a similar light.

## VI

The first law regarding burial practices (21 September 1835) specified in basic terms that:

- (i) in every locality there should be established a public cemetery in a piece of ground far from dwelling areas, to be ready within four months from the publication of the law;
- (ii) Each corpse was to be interred in a separate grave, although the persistence of locally traditional practices regarding funerals, interments and graves was allowed;
- (iii) Any priest allowing a dead body to be buried outside the public cemetery would be deprived of his job and become unable to be appointed to any other.

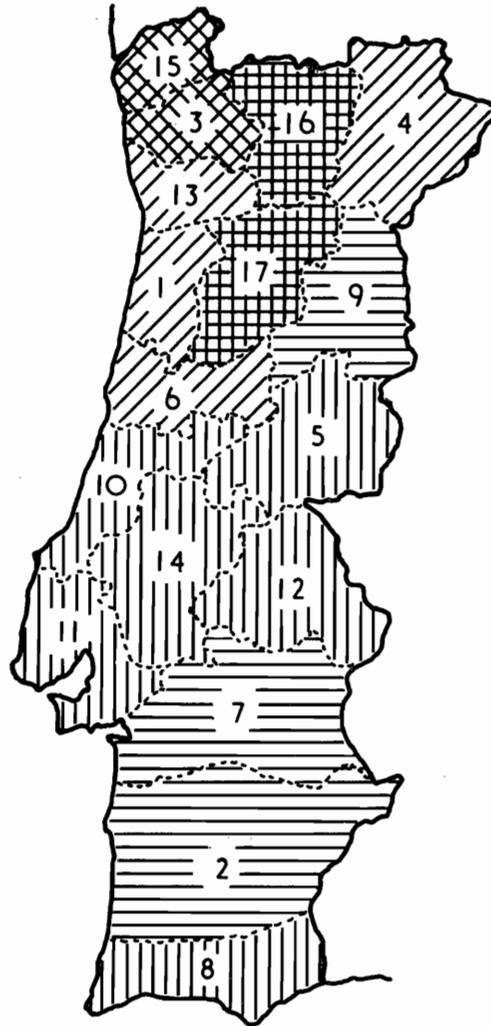
Feeling that these laws had not had the desired effect, Costa Cabral, a powerful Liberal minister, passed a new, more comprehensive and radical set of 'Health Laws' (18 September 1844). These, once again in basic terms, ordered that:

- (i) A national network of 'Health authorities' be established. Amongst the duties of the new functionaries were that they should oppose burials inside churches, confer death certificates, and charge fees for each burial permission - except to the very poor;
- (ii) The dispositions of the previous law regarding the construction of cemeteries be renewed and the control over burial practices tightened.

Public resistance to the enactment of these laws was very strong, particularly in the northern rural areas which were predominantly characterised by a system of independent peasant farming. On the one hand, there were popular uprisings which event-

MAP I      OUTLINE MAP OF PORTUGAL, 1862

- 1 Aveiro
- 2 Beja
- 3 Braga
- 4 Braganca
- 5 Castelo Branco
- 6 Coimbra
- 7 Evora
- 8 Faro
- 9 Guarda
- 10 Leiria
- 11 Lisboa
- 12 Portalegre
- 13 Porto
- 14 Santarem
- 15 Viana do Castelo
- 16 Vila Real
- 17 Viseu



Numbered districts correspond to those shown in Table I

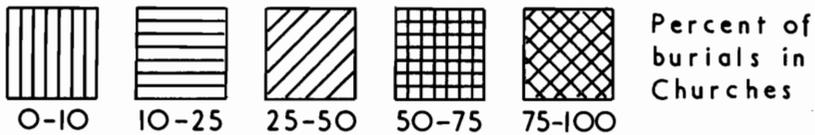


Table 1: Burial Patterns in Portugal, 1862

| DISTRICTS             | 1    | 2   | 3   | 4    |
|-----------------------|------|-----|-----|------|
| Aveiro (1)            | 51.6 | 0.1 | 1.2 | 47.1 |
| Beja (2)              | 82.2 | 0.1 | 0.1 | 17.6 |
| Braga (3)             | 6.0  | 0.4 | 0.9 | 92.6 |
| Braganca (4)          | 55.4 | 0.4 | 0.2 | 43.9 |
| Castelo Branco (5)    | 99.8 | 0.1 | 0.1 | 0    |
| Coimbra (6)           | 63.4 | 0.1 | 0.2 | 36.3 |
| Evora (7)             | 77.5 | 0.2 | 0   | 22.3 |
| Faro (8)              | 95.9 | 1.0 | 2.4 | 5.9  |
| Guarda (9)            | 85.2 | 0.4 | 0   | 14.3 |
| Leiria (10)           | 96.3 | 0.3 | 0.2 | 3.1  |
| Lisboa (11)           | 98.0 | 1.2 | 0.1 | 0.8  |
| Portalegre (12)       | 92.8 | 0   | 0   | 7.2  |
| Porto (13)            | 46.7 | 0.6 | 5.1 | 47.6 |
| Santarem (14)         | 97.3 | 0.3 | 0   | 2.4  |
| Viana do Castelo (15) | 17.6 | 0.3 | 0.4 | 81.7 |
| Vila Real (16)        | 46.6 | 0.2 | 1.7 | 51.5 |
| Viseu (17)            | 44.6 | 0.2 | 0.3 | 55.0 |
| AVERAGE               | 67.5 | 0.4 | 0.9 | 31.2 |

## Notes:

- a. Column headings: 1 = Public cemetery  
 2 = Private vault in a public cemetery  
 3 = Private vault outside a public cemetery  
 4 = Church
- b. Parenthetic numbers in this table correspond to the districts shown on Map I.

Table 2a: Burial Patterns in Porto, 1862

| MUNICIPALITIES     | 1    | 2   | 3    | 4    |
|--------------------|------|-----|------|------|
| Amarante           | 5.0  | -   | 1.5  | 93.5 |
| Baiao              | 15.8 | -   | 0.2  | 84.0 |
| Bouca              | 90.2 | 3.7 | 0.6  | 5.5  |
| Felgueiras         | 0.7  | 0.2 | -    | 99.1 |
| Gondomar           | 90.3 | 1.4 | -    | 8.3  |
| Lousada            | 3.8  | 0.3 | -    | 95.8 |
| Maia               | 92.6 | 1.2 | -    | 6.2  |
| Marco de Canaveses | 4.5  | -   | 1.0  | 94.5 |
| Pacos de Ferreira  | -    | -   | 0.7  | 99.3 |
| Paredes            | 5.9  | 0.3 | -    | 93.8 |
| Penafiel           | 12.2 | 0.2 | 0.2  | 87.4 |
| Porto              | 71.2 | 0.1 | 17.8 | 10.9 |
| Povoa de Varzim    | 52.5 | -   | 1.3  | 46.2 |
| Santo Tirso        | 27.0 | -   | 0.4  | 72.6 |
| Valongo            | 89.0 | -   | -    | 11.0 |
| Vila do conde      | 29.8 | 1.5 | 6.4  | 62.3 |
| Vila Nova de Gaia  | 87.3 | 2.1 | 0.9  | 13.3 |
| AVERAGE            | 46.7 | 0.6 | 5.1  | 47.6 |

## Notes:

- a. Column headings: 1 = Public cemetery  
 2 = Private vault in a public cemetery  
 3 = Private vault outside a public cemetery  
 4 = Church

ually led to the Maria da Fonte Revolution in 1846, and which were definitely connected with the attempt by the civil authorities to enforce the Health Laws (as well as other laws which were seen as being similarly repressive); on the other hand - and this is the aspect with which we are concerned here - there was passive resistance, the extent of which can be judged from the data on burials provided by the 1862 census.<sup>30</sup> This source, which Joel Serrão argues to be rather reliable,<sup>31</sup> registers all deaths occurring in that year, and presents a large amount of information about the dead person: age, sex, marital status, place of birth and death, occupation, and the place where the body was interred. The census specifies the following four places of burial:

- (1) public cemetery
- (2) private vault in the public cemetery
- (3) private vault outside the public cemetery
- (4) church

We are here concerned with the opposition to burials in public cemeteries and the building of these cemeteries. We are, therefore, mostly interested in distinguishing categories (1) and (2) from categories (3) and (4), particularly since those who accepted the new laws and bought a permanent site in a cemetery for their family's dead - category (2) - must not be merged together with those (mainly monks, nuns, and aristocrats) who were buried in private chapels or vaults inside religious buildings, category (3). The distinction between categories (1) and (2) must be seen as one of wealth and not one of belief.

Table I presents the proportion of burials taking place in each of the four categories described above for every Portuguese administrative district. Map 1 represents graphically the proportion of burials reported to have taken place inside churches. Both of these show clearly, we hope, how the practices relating to burial in the traditional way differed radically throughout the country. Some of these figures appear to be unreliable, and it is probably true that (perhaps for political reasons) the number of people buried in churches was underestimated. It seems to us improbable that no such burials had occurred in Castelo

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<sup>30</sup> *Mappas Estatísticos dos Baptismos, Casamentos e Óbitos que houve no Reino de Portugal durante o anno de 1862*, Lisboa: Imprensa Nacional 1869.

<sup>31</sup> Joel Serrão, *Demografia Portuguesa*, Lisboa: Horizonte 1973, p. 173. In spite of the general value of the source, we feel that two or three of the figures presented must be considered with some suspicion until further research proves them to be correct.

Branco, or that the difference between Guarda and Viseu could be quite so large (14.3% as opposed to 55.0%).

It is, nevertheless, clear that a fault-line divides the country. The south is generally below 20% (except for Evora with 22.3%); while the north is above 30%, that is, above the national average of 31.2%. Furthermore, in the north, it is possible to distinguish a zone of moderate resistance to the legislation (Coimbra, Aveiro, Porto, and Bragança - all below 50%) and a hard core of resistance in Braga and Viana, where the proportion of those accorded the traditional form of burial was over 80%.

This distribution of the percentages of burials in churches suggests that the main explanation for the variation is to be found in the different cultural characteristics of each region. In a nation so often said to be not one country but several bound together this is hardly surprising; yet, it is only a partial explanation.

In order to have a better understanding of this process in Minho, we consider in more detail the districts of Porto, Braga, and Viana which compose this province, bringing the analysis to municipal level (see tables 2a, 2b and 2c; and Map II).

In the district of Porto the capital city and its surroundings had a proportion of burials in churches below 20%; the municipalities on the northern coast (Vila do Conde and Póvoa do Varzim) had intermediate positions; and the mountainous interior rejected the legislation with percentages of over 70%, reaching in some cases 99% of all burials (Felgueiras and Paços de Ferreira).

In the district of Braga - the see of an Archbishopric and a traditionalist religious centre - there was not one single municipality where less than 80% of burials were in churches. Here, the capital city, unlike the city of Porto, did not represent a focus of obedience to the law, and the distinctions among municipalities are not worth further comment.

The picture in the district of Viana is similar to that of Braga, except for two municipalities which are credited with surprisingly low proportions of burials in churches: Caminha (52.8%) and Valença (15.2%). The former case may reflect the action of a particularly dynamic or authoritarian municipal authority, but the second case appears so much in contradiction to the others that a mistake in the census cannot be ruled out.

The inter-municipality differences are here noteworthy, as in Porto. Viana, Caminha, Cerveira, Valença, that is the coast and part of the River Minho valley, the area with the best communication network and with some of the largest population centres, had their proportion at least 20% lower than the mountainous area. This is perhaps enough to suggest that both the communication network and the process of urbanisation were in some ways associated with the spreading, if not of new ideas, at least of new practices in accordance with the law. This is a fact which may be witnessed still today since some of the most isolated parishes of the district of Viana still have no cemetery, and many of the cemeteries in this region were built only in the 1960s and the 1970s.

Table 2b: Burial Patterns in Braga, 1862 (District 3 on Map I)

| MUNICIPALITIES    | 1    | 2   | 3   | 4    |
|-------------------|------|-----|-----|------|
| Amares            | 16.4 | 0.5 | 1.6 | 81.5 |
| Barcelos          | 2.4  | -   | 0.1 | 97.5 |
| Braga             | 3.5  | 0.7 | 1.2 | 94.6 |
| Cabeceiras        | 19.0 | -   | -   | 81.0 |
| Celorico          | 0.2  | -   | 0.2 | 99.6 |
| Esposende         | 12.3 | -   | -   | 87.7 |
| Fafe              | 11.5 | 0.2 | -   | 88.3 |
| Guimaraes         | 8.0  | -   | 3.7 | 88.3 |
| Povoas de Lanhoso | 2.6  | 4.6 | 0.8 | 92.0 |
| Terras de Bouro   | 4.1  | -   | -   | 95.9 |
| Vieira do Minho   | 0.4  | -   | -   | 99.6 |
| V.N.Famalicao     | 6.7  | 0.2 | 0.9 | 92.2 |
| Vila Verde        | 1.5  | -   | -   | 98.5 |
| AVERAGE           | 6.0  | 0.4 | 0.9 | 92.6 |

Note:

Column headings: 1 = Public cemetery  
 2 = Private vault in a public cemetery  
 3 = Private vault outside a public cemetery  
 4 = Church

Table 2c: Burial Patterns in Viana do Castelo, 1862 (District 15 on Map I)

| MUNICIPALITIES     | 1    | 2   | 3   | 4    |
|--------------------|------|-----|-----|------|
| Arcos de Valdevez  | 8.2  | -   | -   | 91.8 |
| Caminha            | 47.2 | -   | -   | 52.8 |
| Coura              | 1.5  | -   | 1.0 | 97.5 |
| Melgaco            | 6.8  | -   | -   | 93.2 |
| Moncao             | 7.9  | -   | 0.2 | 91.9 |
| Ponte da Barca     | -    | -   | 0.5 | 99.5 |
| Ponte de Lima      | 1.1  | -   | 0.1 | 98.8 |
| Valenca            | 80.6 | 2.3 | 1.9 | 15.2 |
| Viana do Castelo   | 21.8 | 0.3 | 0.7 | 77.2 |
| Vila N.de Cerveira | 22.3 | 0.6 | -   | 77.1 |
| AVERAGE            | 17.6 | 0.3 | 0.4 | 81.7 |

Note:

Column headings: 1 = Public cemetery  
 2 = Private vault in a public cemetery  
 3 = Private vault outside a public cemetery  
 4 = Church

Table 3: Data on Cemeteries in Viana

Date of inauguration of some cemeteries in rural parishes of the district of Viana and the distance between them and the parish church.

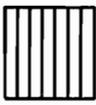
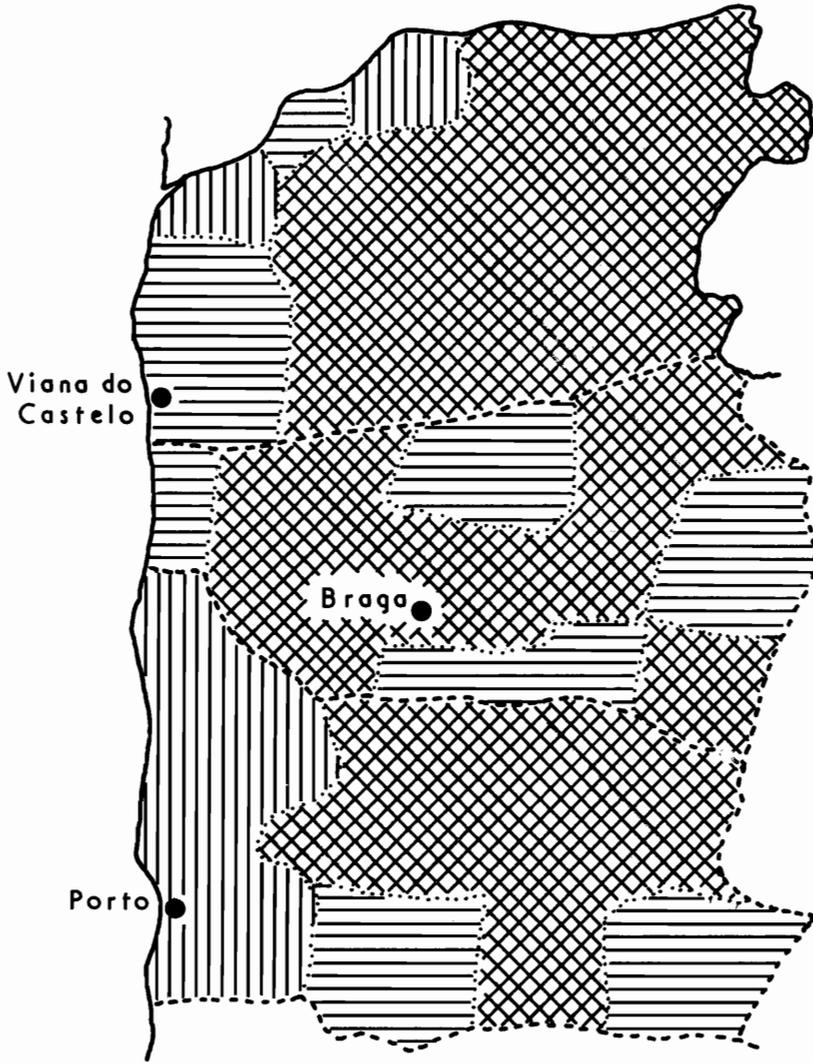
|                                 |               |                |
|---------------------------------|---------------|----------------|
| Santa Leocadia de Geraz de Lima | 1885          | 15 metres      |
| Castelo do Neiva                | 1888          | 3 metres       |
| Lanheses                        | 1888          | 80 metres      |
| Cabacos                         | 1890(approx.) |                |
| Vitorino dos Piaes              | 1900(approx.) |                |
| S.Joao da Ribeira               | 1911          |                |
| Subportela                      | 1912          | 100 metres     |
| Facha                           | 1914          |                |
| Correlha                        | 1916          |                |
| Santa Maria de Geraz de Lima    | 1917          | 90 metres      |
| Amonde                          | 1919          | 100 metres     |
| Meixedo                         | 1928          | 30 metres      |
| Crasto                          | 1943          |                |
| S.Juliao do Freixo              | 1947(approx.) |                |
| Santa Marta                     | ?             | "just outside" |

Sources:

The source of the data in Tables 1 through 2c is *Mappas Estatisticos dos Baptismos, Casamentos e Obitos que houve no Reino de Portugal durante o anno de 1862*, Lisboa: Imprensa Nacional 1869. The data in Table 3 was provided by the parish priests.

MAP II

MINHO, 1862



Percent of  
burials in  
Churches

VII

The opposition to the Health Laws was, therefore, stronger in Minho than elsewhere in the country. One of its most important manifestations was a declared resistance on the part of the population to the actual building of cemeteries under the conditions specified by the law. The story of the cemetery of the capital city of the district of Viana, extracted from the records of the municipality, exemplifies this point clearly.

In 1838, the General Administrator (a non-elected representative of the Government at the municipal level) ordered the municipality to fulfil its duties regarding the construction of cemeteries, and to demarcate and enclose land for this purpose in all parishes *within thirty days*.

In 1839, the municipality again ordered all parishes to choose the location of their cemetery. Later in the year, in response to the General Administrator, the municipality admitted that not a single cemetery was reported to have been constructed, the presumed reason being a 'lack of financial means'. Four years after the enactment of the law, no practical result had yet been achieved.

In December 1840, the cemetery of Viana, the first in the district, was inaugurated. The minutes of the municipality, dated the 30th of December, read:

Yesterday at noon, in the woods of the extinct monastery of St. Antony's, the public cemetery was blessed in all solemnity by His Reverence the Archdeacon of the *Julgado*, who had earlier on left the parish church incorporated into a procession in which His Excellence the General Administrator of the District, many clergymen, the municipal authorities, and armoured troops also took part.

In August 1841, the records of the municipality refer twice to the cemetery, first deploring the careless manner in which it was kept, and then suggesting it should be transferred to another place, a transference which was 'much desired by all the inhabitants of this city'. In July 1842 they state that 'it is evident that the present location of the cemetery does not please the inhabitants of Viana'. By then, the cemetery had already been closed on the orders of the Civil Governor. The opposition to the cemetery must have been very strong, and the municipality was faced with the fact that it had been unable to implement the national legislation.

At this point the story takes a new, unsuspected turn - the municipality decides to call for an examination of the cemetery's soil. 'The burial ground', the report of the experts reads, 'is absolutely inappropriate for its purpose, not only because of its argillaceous soil, but also for the reduced space for the graves it provides, and *for being too close to the city*' (our emphasis).

This was as 'scientific' and 'rational' a conclusion as the local intelligentsia was able to formulate. It saved the face of the municipality, but it did not reflect the real causes of popular protest. It also opened a door to the municipality, to find another place for the cemetery.

The new cemetery was inaugurated in 1855, and this time there is no mention of any ceremonies celebrating the event, and we presume there were none. In fact, the inauguration was the result of strong pressure exerted on the municipality by the Civil Governor of the District at a time when *cholera morbus* was threatening the city. Nevertheless, the municipality, still 'unwilling to go against the popular feeling so clearly manifested', exempted itself from the responsibility, claiming that the district authorities had abused their power. This time, however, twenty years after the first bill, the cemetery of Viana, a city of over 8,000 people, was open for good.

The opposition to the construction of cemeteries has continued right until our own days. In two separate questionnaires addressed to parish priests of the district of Viana we asked for details regarding their parishes' cemeteries. In some cases, the priests based their answers on parish registers; in others, on the date that is to be found in the gates of most cemeteries; finally, in some, on their own personal experience as builders of cemeteries. Their answers have been summarised in Table 3.

This is a random sample, made possible by the kind co-operation we received. No definite conclusion can be derived from it as regards the patterns involved in the timing of the cemeteries' construction, but it is certainly a good enough sample to show the failure of a law which was passed in 1835 making it compulsory for every parish to build a cemetery before the end of the year. In some cases, fifty years were necessary; in many, more than one century elapsed before the population accepted the idea of being buried in a cemetery away from the church.

## VIII

Various explanations have been given for this popular resistance to cemeteries. Most of these must be discarded, being based on the values of the bourgeois élite which formulated them, rather than on those of the popular strata of the population. One of these is that burial outside churches is similar to that given to animals. Another is the fear that, by lack of adequate protection of burial grounds, animals might unearth corpses. The 1835 bill expressed these feelings in the following way:

One of the reasons offered to justify the refusal to bury the dead outside churches in the past is a

plausible one. The authorities charged with preventing burials in churches did not act as diligently to build cemeteries, sometimes limiting themselves to the choice of a piece of land, not enclosed and open to all sorts of animals, which provoked a scandal among people who could not accept so irreverent a treatment of their dead.

But, if burials in churchyards were indeed practised widely, and were even offered as an alternative to burials inside churches, it becomes difficult to see such an argument as at all valid. No ground could be less protected than that of the churchyard, if the activities of stray animals were really a concern. We feel that the best answer to this argument has been given by Camilo Castelo Branco, the famous 19th-century novelist:

In March that year [1846] the Septembrists of Braga incited the popular riots in the borough of Lanhoso. In England, in the House of Commons, Lord Bentinck explained, with tragic pomposity, the origin of the revolt, which with disdain he called 'a rebellion of the rabble'. He said that the Cabrals had ordered the construction of cemeteries, but had had no walls built round them, so that dogs, cats and wild boars got in in such numbers that they dug up the corpses. Nations and naturalists alike must have had a rather inflated idea of the size of the Portuguese cats which dug up corpses, and of the good relations between our dogs and the said cats in the task of exhuming them. They would have been no less surprised by the familiar behaviour of the boars who came from Gerês to collaborate with the dogs and cats in extracting rotten flesh from the soil of Lanhoso. And so the origin of the national insurrection of 1846 is defined in the annals of revolutionary Europe. It was a reaction, a battle fought by society against the swarms of dogs and cats and their allies, the profaning muzzles of the wild boars. And so it was that the journalists of Germany (a serious nation) wrote that the revolution in Minho was 'the model of legality'. The corpses served at the illegal night-time banquets of the boars and their friends, the big cats, growling with their hair on end, and the bloody-muzzled hounds - this was a case which greatly impressed the Teutons, because it was an act forbidden by the Constitutional Charter. Whether it was the Septembrists of Braga, or the united confederation of wild beasts, the fact is that the insurrection in the Alto Minho laid waste that province and Trás-os-Montes, the destruction embracing printed notices and the wines in the upland taverns. The war started by the cats and their accomplices cost the country a

capital loss of 77.5 million *cruzados*.<sup>32</sup>

In fact, the fear of contagion through the indirect contact with dead bodies is a typically bourgeois phenomenon, and not a popular one. The Liberals were not only concerned with the actions of stray dogs and cats, they were also perturbed by indirect contact via the vegetable species that grow on the decaying bodies underneath the soil. The 'Rules to be Obeyed'<sup>33</sup> of the cemetery of Viana, laid down at the time of its construction, read

It is absolutely forbidden to grow any fruit tree, vegetables, or other kinds of food, or to keep poultry or other animals within the cemetery.

Eça de Queiroz, another famous 19th-century novelist, alights on the same issue: indirect contact with the dead presents no problems for those who experience death as *apprivoisée*. In his novel *A Capital*, a central character, Artur, visits his aunt's grave in the cemetery of Oliveira de Azemeis. There, he meets the cemetery keeper who is cutting grass. Artur ponders on how the grass which is growing on the grave, contains 'something' of his aunt, and enquires why it is being cut. 'This grass?', the cemetery keeper answers. 'This grass is very good. I cut it for my rabbits'.

To the 'enlightened', modern bourgeoisie, the dead were considered rotten substance, capable of transmitting disease, and were entities outside the community of the living. For the popular culture, they remained full members of a larger community, devoid of any negative stigma.

## IX

In this paper we have attempted to plot out and understand a conflict between two views of the world which, in spite of their differences, must still be understood to belong to the same wide cultural tradition. In many senses, the conflict we described is best seen as the striving of one complex and internally diversified social group to cope with the constant becoming of its own perception of the world. This becomes clear when we, so to speak,

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<sup>32</sup> *A Brasileira de Prazins* [1882], Porto: Lello 1974, pp. 187-8. We would like to thank T. Earle for his help with the translation.

<sup>33</sup> M.S., *Livro das Sessões da Camara Municipal de Viana do Castelo* (16.9.1855-19.12.1857), Municipal Archive of Viana do Castelo.

look at the state of play. Even if everyone now accepts contentedly the fate of being buried in a cemetery, how can the Liberals' ideas be said to have won? The change took over a century to be effected, the cemeteries were not built where the law specified they should, and, when they were built, it was as much due to demographic reasons as to the acceptance by the popular culture of Minho of the 'lights of a century' that have long since passed, and whose 'lights' are no longer those of the 20th century.

We would like to end on a note struck long ago by Van Gennep when he argued that funerals are the primary occasion for the manifestation of the 'core of life values sacred' to a social group. This seems clear in relation to those who 'medicalised' death. By marginalising it, they were symbolically manifesting their desire for life. But the same applies to their opponents who, contrary to them, were willing to share their daily life with the dead and to look frontally at death. They only did so because death was co-opted and became the moment *par excellence* of the ritual manifestation of those values which they saw as the driving force of their social life.

JOÃO DE PINA-CABRAL  
RUI FEIJÓ

## DYING AND INHERITING IN RURAL TRÁS-OS-MONTES

### I. *Introduction*

This paper deals with the two closely linked processes of dying and inheriting in a small rural hamlet in North-east Portugal. I place major stress upon the element of *time* involved in the transmission of property over the generations. Hence, I am not concerned here primarily with the strictly ritual aspects of death as a rite of passage in the traditional anthropological sense, nor with purely descriptive elements of the discrete events of 'death' and 'inheritance'. Rather, I will look at a specific form of property transfer (*post-mortem* partition) and its long-term effects on the entire social structure over time. Following Jack Goody,<sup>1</sup> we might expect that the precise timing of this property transfer has repercussions throughout the society. This is indeed the case. Fontelas is a most peculiar place, especially if viewed through the eyes of 'Mediterranean' anthropological theory. I propose that this peculiarity itself is a function of a particularly rigid form of delayed inheritance at death.

This paper follows a simple sequence. After a brief introduction which situates the hamlet under study, Section II deals with the process of 'dying' in two respects. I look first at the

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<sup>1</sup> Jack Goody, with Joan Thirsk and E.P. Thompson (eds.), *Family and Inheritance: Rural Society in Western Europe 1200-1800*, Cambridge: Cambridge University Press 1976; see 'Introduction' and 'Inheritance, Property and Women: Some Comparative Considerations', pp.1-36.

short periods of time immediately preceding and following a death, and secondly at the extreme stress placed by this society upon ties of 'descent' and the *downward* transmission of property over the generations. It is at death and not at marriage that key transfers of property and domestic power take place in Fontelas. Finally, Section III further elucidates the link between dying and inheriting by examining (a) the short-term steps involved in the partition of property, and (b) the long-term effects of delayed inheritance upon the social structure as a whole. I conclude that this specific mode of *post-mortem* property transfer constitutes the principal 'clue' to an understanding of the society's major features.

'Fontelas' is a rather tiny rural hamlet located in the northern Portuguese province of Trás-os-Montes.<sup>2</sup> In the middle of my period of fieldwork (mid-1977) the hamlet comprised 57 inhabited households with a total population of 187. Although two daily buses connect the hamlet with the municipal town (Vinhais) and the district capital (Bragança), Fontelas' general geographical isolation is obvious. The hamlet is situated within a high plateau region characterized by predominantly manual agriculture. Three tractors have been purchased over the last two decades (the first in the 1960s and the following two in 1970), while two mechanical reapers and a threshing-machine have also been incorporated within the hamlet's productive system. However, most tasks are still carried out with sickles, scythes, hoes, and picks. Wooden ploughs and ox-carts constitute a rural household's basic farm equipment. The Mediterranean trio of wheat, wine, and olives is quite absent here, where the cooler climate and mountain location favour the Central European crops of rye, potatoes, and chestnuts. Only a handful of wealthy households ever sell surpluses of the little wine they produce, while smaller quantities of maize, legumes, and walnuts are also grown. Some quantities of chestnuts and potatoes are also sold, but certainly not on a scale that would properly merit the term 'cash crops'. Agriculture in Fontelas is basically small-scale: it is directed almost exclusively towards household consumption and internal hamlet exchanges.

Internally, the hamlet's physical appearance presents a sharp contrast between the older, stone houses and the more recently built brick and cement houses constructed by emigrants and their relatives. Despite the arrival of such painted and flamboyantly coloured houses in recent years, the general contours of the hamlet remain dominated by the older stone structures. At the time of my fieldwork there was no electricity in Fontelas, no televisions other than the priest's (itself only battery-operated), and for

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<sup>2</sup> Fieldwork in 'Fontelas' (a pseudonym) was carried out continuously from March 1976 to September 1978. Special thanks are extended to the Calouste Gulbenkian Foundation in Lisbon for a research grant, and to José Cutileiro, Julian Pitt-Rivers, and Peter Loizos for supervision of my doctoral thesis.

most of the year the unpaved hamlet streets and paths were veritable piles of dirt and mud. Most houses used the open hearth rather than butane-gas stoves for cooking throughout most of the year and for the smoking of pork in the winter.<sup>3</sup> The interiors of kitchens in the older stone houses were invariably pitch-black. Only the priest and the bus-driver (a temporary resident) had cars. Despite the wave of emigration to France and West Germany in the 1960s, the general aspect of Fontelas as late as 1978 was still that of an isolated, 'backward' peasant community.

Four major social groups have constituted the social hierarchy of Fontelas since at least the mid-nineteenth century. My survey of all of Fontelas' landholdings in 1978 indicates that these social groups continue to maintain distinct economic and social characteristics linked closely to the ownership of land. The 4 *proprietários* (large landowners) own landholdings of an average size of 43 hectares each, and tend to rent out some of their land to poorer villagers. These 4 households own virtually all of the agricultural machinery in the hamlet, and have historically constituted the wealthy élite of local notables occupying key posts on parish and municipal councils. The second group is that of the 6 *lavradores remediados* (well-off 'ploughers') who own landholdings averaging 25 hectares each. Many of these villagers are related by kinship or marriage to a number of the wealthier *proprietário* households.

The third social group is that of the 14 *lavradores* ('ploughers') who own landholdings averaging 8 hectares each. The social origins of individuals in this group are quite distinct from those in the two upper groups: many of today's *lavradores* were in fact poorer day-labourers, servants, or shepherds in the past. Most villagers in this group have managed to consolidate just enough disparate plots of land (through borrowing, rental, and clearing) to be able to maintain a plough-team of two draught animals. The fourth and poorest social group is that of the 31 smallholders. These villagers were formerly *jornaleiros* (day-labourers) who worked for wages in kind or money. Today they own landholdings averaging only 2½ hectares each. None of the smallholders owns a plough-team, and most must borrow equipment or pay for the ploughing services of other co-villagers.

This picture of current hierarchy is reflected by both civil and ecclesiastical documentary sources from the nineteenth century,

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<sup>3</sup> By 1982, a number of substantial improvements within the hamlet have altered this picture dramatically. An entire series of newly constructed houses on the outskirts of the hamlet have come to constitute a 'new section' called *o bairro novo*; electricity has been installed, bringing with it refrigerators and televisions; the central hamlet paths have been laid with cobblestones; a new schoolhouse has been built; and a new *Casa do Povo* (People's House) serves the inhabitants of Fontelas and its surrounding hamlets in a social-service welfare capacity.

and should warn us against viewing Fontelas as an 'egalitarian' community in which 'everyone owns their little plot of land'. This hierarchical structure will provide an important background to the processes of inheritance examined in Section III below.

A number of corporate institutions also characterize the hamlet's social organization, but these elements should not imply that villagers continue to live (or indeed ever lived) in a state of pristine communal bliss. There are four fully collective water-mills (*moinhos do povo*) in Fontelas, only one of which currently functions. Also in use today are three corporate baking-ovens (*formos do bairro*) located in three of the hamlet's poorer sections. But alongside these forms of communal and corporate property, there are also three individually owned water-mills (not functioning) and 20 private baking-ovens. Both collective and private property thus co-exist in Fontelas. The hamlet council (*conselho*) is called together to discuss common hamlet concerns and to implement the reconstruction and repair of communal property. Also of interest is the rigid system of summer irrigation turns: this system of rotation allocates strict turns (*tornas*) of water to the co-owners of three corporate water-pools. The presence of all of these corporate institutions as well as the practice of unpaid day-labour exchange (*tornajeiras*) situate Fontelas within the context of Iberian collectivism.<sup>4</sup> This does not mean that these villagers are any more 'democratic' or naturally cooperative than other villagers in different regions of the Iberian Peninsula, but rather that the specific ecological constraints of Fontelas' location have necessitated particular forms of the pooling of property and labour.

We will see below that this particularly bleak geographical situation sets strict limits upon villagers' economic activities. European mountain agriculture has probably always been a relatively precarious endeavour.<sup>5</sup> But it is this generally poor mountain agriculture that also conditions key elements in Fontelas' social system. Elderly villagers must be assisted throughout their old age, and the hamlet's predominantly manual agriculture requires a particularly rigid form of the *avoidance* of patrimony division. In this sense, we should bear in mind that the processes of dying and inheriting analyzed below occur within an especially small and 'peripheral' ecological niche.

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<sup>4</sup> See Jorge Dias, *Rio de Onor: Comunitarismo Agro-pastoril*, Porto: Instituto Para a Alta Cultura [Centro de Estudos de Etnologia Peninsular] 1953; and Susan Tax Freeman, 'Corporate Village Organisation in the Sierra Ministra: An Iberian Structural Type', *Man* (n.s.), Vol.III (1968), pp.477-484.

<sup>5</sup> See B.H. Slicher Van Bath, *The Agrarian History of Western Europe: A.D. 500-1850* [Trans. O. Ordish], London: Edward Arnold 1963; and Fernand Braudel, *Capitalism and Material Life*, London: Fontana/Collins 1973.

II. *Dying as a Social Process*

The process of dying in Fontelas is in no way limited to the physical reality of sickness or death. This is perhaps an area that has not been treated with great attention in the ethnography of European rural communities. A general analytical focus on the strictly religious aspects of the mourning period, and on formal obligations to the dead, has perhaps underplayed the equally significant period of time *prior* to a death.<sup>6</sup> Let me briefly stress here one aspect of this period by highlighting the proliferation of gossip which abounds as a death approaches. A close look at this gossip will lead us directly into the link between dying and the inheriting of property.

Gossip about a villager's property may begin well before a terminal illness. In this case (as in those of old age) such gossip understandably heightens as the prospect of imminent death increases. Two of the major topics touched on by most co-villagers are: (a) the specific pieces of property owned by the dying villager, and (b) the tenor of the social relationships between the latter and his/her closest relatives. These two topics do of course intertwine, as property relations and kin relations clearly blend together. Many subjects are discussed: the villager's life-history, reputation, personal relationships, and his/her inherited and acquired property. The most minute characteristics of the villager's personality, and the most specific examples of past behaviour are described, discussed, and evaluated. But the approach of death adds a rather dramatic note of expectation and uncertainty to these comments. Upon the villager's death, who will get what? Who are the person's legitimate heirs, and what are their specific economic circumstances? Will the property be partitioned equally, or does a will favouring one legatee enter the picture and complicate matters? The speed with which this gossip spreads is quite striking, and in the priest's eyes is indicative of these villagers' 'obsession' with *terras* (plots of land).<sup>7</sup>

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<sup>6</sup> This is not meant as a criticism of these major ethnographers' treatment of death. Rather, for reasons that will become clearer throughout this paper, this 'pre-death' period has a very important and specific role within this particular society's inheritance system. I suspect that comparable structures, in communities where death and inheritance are *not* so closely linked, may of course not catch the eye so quickly.

<sup>7</sup> The villagers' extreme interest in *terras* is the priest's most often-cited reason for desiring to give up his job and leave Fontelas altogether. Padre Gregório sees this excessive concern with land and inheritance as a degenerate form of 'materialism'. He has for many years sought to escape from this 'vicious circle of stingy peasants' by requesting the Bishop to transfer him to Angola or Mozambique as a missionary.

Much of the talk prior to a death centres on the imminent redistribution of the individual's patrimony: this talk usually circulates in subdued, whispered tones. Such gossip is frequently a mixture of fabrication and fascination about other villagers' property rights rather than precise knowledge of the legal facts involved. Indeed many heirs themselves are not always clear as to the specific legal details of inheritance even in their own cases. However, this fabrication about others is of interest in itself: it points to a particularly acute perception of the implications of death in Fontelas. While death clearly brings about the *end* of a living individual, it also marks the *beginning* of a process of property redistribution. It is both of these processes together that give death in Fontelas such a pivotal role within the transfer of patrimony over the generations.

The gossip I allude to here continues throughout the actual days between a villager's death, the accompanying vigil and funeral, and through the days and weeks following the death. It is the anticipation of the social and economic reorganization of kin and property relations which is sparked here, and which is so visible in villagers' gossip. In this sense, my stress on *process* attempts to highlight the time periods prior to and following a death rather than the death itself.

Of particular interest is the level of conversation during the *mortório*, or vigil.<sup>8</sup> Following a few formal statements to the close kin of the deceased and the sprinkling of holy water over the corpse (which lies for viewing in an open coffin), most visitors begin to mingle together in one of the main rooms or hallways of the deceased's house. Conversation here may reach quite loud proportions, and it is not solely limited to comments on the deceased. Occasional laughs and exclamations can be heard, and conversation may wander over the weather, the day's activities, relatives abroad, the news, national politics, or even the state of one's cows and pigs.<sup>9</sup> Thus, conversation throughout these ritualized days oscillates continually between (a) complete

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<sup>8</sup> My fellow anthropologists, I hope, will forgive this heretical abbreviation of mortuary ritual in Fontelas. A detailed description of vigils and funerals is contained in Chapter 7 of my doctoral thesis (*Social Hierarchy in a Northern Portuguese Hamlet, 1870-1978*, London School of Economics, Dept. Anthropology: Ph.D. thesis 1982).

<sup>9</sup> Both William Douglass (*Death in Murélagá: Funerary Ritual in a Spanish Basque Village*, Seattle: University of Washington Press 1969; at pp. 26-28) and Stanley Brandes (*Migration, Kinship and Community: Tradition and Transition in a Spanish Village*, New York: Academic Press 1975; at p.172) have noted similar attitudes towards conversation during vigils in Spanish rural communities, where laughter is not out of place and where a 'stone-faced solemnity' is absent. Douglass interprets this as a sign of a generally 'matter-of-fact' attitude towards death in the Basque village of Murélagá.

'avoidance' of the human tragedy through miscellaneous chat, and (b) total immersion in the wider implications of the death. In the latter case, there appears a dual 'telescopic extension of time' on both sides of the death. Firstly, one set of comments centres upon the past of the deceased (his/her personality, achievements, and former kin). Secondly, another series of comments lingers suspiciously around the future events which will re-order the relations of the deceased's close kin (the partition, the value of the property, and the life-chances of the heirs). The fact that this field of communication spreads so quickly throughout the hamlet is an indication of the power that death holds within the social structure: the implications of a death are profoundly unsettling and disturbing. Let us see now why this is so.

I turn here to a second time period - that immediately following a death - in order to stress a particularly salient feature of this society's kinship system. This is the extremely strong 'descent' tie<sup>10</sup> between parents and children. This tie pervades the entire kinship system in both legal as well as social terms, and it far outweighs the very weak link in Fontelas between husband and wife.

Clearly, the period following a death is a particularly traumatic one for close kin. Many of the songs and verses from the hamlet's folklore point to the lingering feelings of loss when a parent dies:

*Olha tu, já não tens pai,  
Tua mãe também morreu,  
Diz-me com quem vives tu,  
Diz-me com quem vives tu.*

Listen you, who has no father,  
Your mother has also died,  
Tell me with whom you live,  
Tell me with whom you live.

*Eu vivo com um irmão meu,  
Diz-me lá como lhe fazes,  
Para lhe dar de comer,  
Mandengando\* pelas portas.*

I live with a brother of mine,  
Tell me then what you do,  
In order to give him food,  
Begging at people's doors.

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<sup>10</sup> I use the term 'descent' here in a strictly European and *not* an Africanist sense to stress the vertical link between proximal ascendants and descendants. A number of authors (Goody, *op.cit.*; Emmanuel Le Roy Ladurie, 'Family Structures and Inheritance Customs in Sixteenth-century France', in Goody (ed.), *ibid.*; and Pierre Bourdieu, 'Marriage Strategies as Strategies of Social Reproduction', in Robert Forster and Orest Ranum (eds.), *Family and Society: Selections from the Annales*, Baltimore: Johns Hopkins University Press 1976, pp.117-44) also use the term in a similar way to refer to 'household lines' and the perpetuation of household property through time. The term 'lineage' poses the same problem as Goody rightly notes: "'Lineage" is used in the medieval way, very different from current anthropological use' (Goody, *op.cit.*, p.21).

*Mandengando pelas portas,  
Quando não há que fazer,  
Queria ir ao cemitério,  
E eu sozinha tenho medo.*

Begging at people's doors,  
When there is nothing to do,  
I wanted to go to the cemetery,  
And I am frightened all alone.

*Queria ir beijar a campa,  
Queria ir beijar a campa,  
Onde tenho o meu segredo.*

I wanted to kiss the grave,  
I wanted to kiss the grave,  
Where I have my secret.

*Queria ir beijar a campa,  
Queria ir beijar a campa,  
Onde tenho o meu segredo.*

I wanted to kiss the grave,  
I wanted to kiss the grave,  
Where I have my secret.

\*(i.e. mendigando)

The death of a parent implies a major readjustment of the labour and kin relations involved in domestic groups. These relations are particularly affected, for instance, by the drastic re-ordering of residence arrangements following the death of a *second* parent. After the death of a first parent the existing residence arrangements of a sibling group may not be reshuffled, and in many cases a partition of the first parent's property is postponed until the death of the second. As the bulk of a person's patrimony is transmitted 'downwards' to the latter's direct descendants and not 'sideways' to the surviving spouse<sup>11</sup>, it is precisely this descent relation between the children and each of their parents that attains priority. In the song above, the daughter attempts to recover her parents by trying to break through the 'barrier' between the living and the dead (the grave or cemetery). But the song points at a deeper level to the general stress placed in the society upon both the emotional and property elements involved in vertical parent-child ties.

Also noteworthy here is the absence of a stress upon horizontal links of spiritual kinship. The *compadrio* link established at a baptism between the parents of the child and the two sponsors is not significant in Fontelas. The only occasions at which relations between *compadres* (male co-parents) and *comadres* (female co-parents) are activated is during labour exchanges, particularly at harvests. Co-parents merely constitute one further choice of potential cooperators, and they could equally

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<sup>11</sup> Cf Jack Goody, 'Sideways or Downwards? Lateral and Vertical Succession, Inheritance and Descent in Africa and Eurasia', *Man* (n.s.), Vol.V (1970), pp.627-638.

be substituted by friends, neighbours, or distant cousins.<sup>12</sup> Of far greater importance in Fontelas are the vertical relationships established between the baptised child and the godparents: one's *padrinho* (godfather) and *madrinha* (godmother) occupy rather more significant positions. In the event of the deaths of both of a child's parents, in the absence of other close kin the godparents must ideally assume responsibility for the child's upbringing. In general, then, it is the vertical inter-generational tie of the godparents with the godchild that assumes an important role in Fontelas, and not the horizontal intra-generational one between the ritual co-parents. Strong parent-child ties of descent are here 'mimicked' by the vertical tie between the godparent and the godchild.

These points concerning the binding vertical ties of consanguineal and spiritual kinship in Fontelas suggest a particularly keen interest in the continuity of descent relations over time. Let us return to the theme of 'time' here in another form. The long period of time following a death involves quite a different series of events than those observable while an individual is dying. A certain 'process' is nevertheless discernible. This process is highlighted at obvious ritual moments, such as the three customary masses held for the deceased. These are the 'first week's mass' (*missa do sétimo dia*), the 'first month's mass' (*missa do primeiro mês*), and finally the 'first year's mass' (*missa do primeiro ano*). Following the last of these three masses the period of formal religious services terminates, but further masses said for the soul of the deceased may occur afterwards at any time or frequency. A later duty owed to the deceased by his closest relatives is that of All Souls' Day (*Dia dos Fiéis Defuntos*) on 2 November of each year. On this day a special mass is held for all of the deceased of Fontelas: the women of each household then place candles, flowers, and petals in patterned

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<sup>12</sup> A similar lack of stress upon *compadrazgo* ritual kinship has been reported for three northern Spanish communities. Brandes states that 'the *compadrazgo*, as it is known in Latin America and southern Spain, is virtually non-existent in the peasant communities of Castile. In Becedas at least friendship and neighbourship are the main avenues through which non-familial ties are expressed; they are, as it were, functional equivalents of the *compadrazgo*' (*op.cit.*, p.133). Similarly, among the Basques, Douglass has noted that 'there is little emphasis placed upon the tie established between the godparents and the natural parents of the child. The elaborate godparenthood relationship, which exists in other parts of Spain [as Pitt-Rivers has described], is not a feature of the social organization of Murélagua' (*op.cit.*, p.188). Yet more extreme is the Castilian hamlet studied by Susan Tax Freeman, where 'the term *compadre*, stressing the ties between the adults in the contract, is not in use in Valdemora' (*Neighbors: The Social Contract in a Castilian Hamlet*, Chicago: Chicago University Press 1970, p.141).

designs upon their relatives' graves. Following the 'first year's mass' this is the only day of the year when formal religious obligations to the dead are prescribed.

Yet throughout these formal stages of mourning and religious observance, another process also unfolds at another level. I do not mean to underplay the ritual aspects of the post-death period here, but rather to prevent these aspects from dominating our view of long-term processes. In this sense, it is quite as significant to look not only at the specific individual who has died but also at the *structural* position of that individual within a wider fabric of social and property relations. I have in mind the specific concept of 'generational time'<sup>13</sup> in the sense of the reproduction of a social structure over time. Two questions are immediately suggested here. First, why is it that these specific vertical ties are so markedly stressed within the kinship system to the detriment of 'horizontal' ties? Secondly, what particular explanations can be given for the prevalent role of death as the key nexus controlling all major property transfers in Fontelas? I believe that satisfactory answers to both of these questions lie in this case within the 'structural law' of *post-mortem* inheritance, and that a number of clues leads us directly to this conclusion. The gist of the argument runs as follows.

Given Fontelas' specific geographical location and 'peasant' economy, strict limitations must be placed either upon the total resident population of the hamlet or upon the number of heirs to landed patrimony. If all households actually followed to the letter the legal prescription of 'equal division of property between all heirs' the ultimate result would be collective economic suicide. Each heir would end up with only a few millimetres of terrain. As Fátima Brandão has noted for the North-west Minho region: '... even when property could be divided, an actual division was not always possible or advisable owing to economic reasons'.<sup>14</sup> In order to circumvent this, various strategies are effected towards the ultimate goal of *preserving the patrimony intact* as far as this is possible. We will see below that this key goal is quite closely linked to the 'peculiar' features of social structure I have hinted at: late marriage, celibacy, and high ratios of illegitimacy.

But here let us focus for a moment on the role of death. The basic problem is perhaps most easily conceptualised as a dichotomy, or structural opposition if you will, between the two complexes of

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<sup>13</sup> See Carmelo Lisón-Tolosana, *Belmonte de los Caballeros: A Sociological Study of a Spanish Town*, Oxford: Clarendon Press 1966, pp.170-201.

<sup>14</sup> 'Death and the Survival of the Rural Household in a North-western Municipality', in Rui Feijó, Herminio Martins and João de Pina-Cabral (eds.), *Death in Portugal: Studies in Portuguese Anthropology and Modern History*, Oxford: JASO 1983, pp.75-87; at p.83.

*matrimony* and *patrimony*.<sup>15</sup> In order to achieve the goal of preserving landed patrimonies, drastic limitations must be set upon the potential reproductive careers of a household's future heirs. In most cases this limitation is effected by the postponement (or prevention) of the marriages of some of the siblings. One child (either male or female) usually marries at an early age and attains a favoured position both socially within the parental household and often economically as the recipient of a large portion of the natal patrimony. In other words, a process of selection by the parental generation separates a sole, favoured heir from the remaining siblings. These siblings are then informally 'cajoled' into less prestigious paths - either celibacy or late marriage. Indeed, many of these socially 'secondary' siblings never marry at all. The prime goal of preserving the patrimony intact thus drives a wedge between the siblings.

This system of selection creates a constant tension between, on the one hand, *matrimony* and the drive for reproduction, and on the other, *patrimony* and the objective of 'restricted' marriages. In this society it is quite clear that patrimony is the winner and matrimony the loser. Ideally, each landed household must attempt to achieve a balance between an optimum labour supply and a minimization of threats to divide its patrimony. There is one extreme (too many children and multiple marriages) and the other (no children and no marriages). The former will ultimately force partition, while the latter will menace the household with extinction. It is as if every occasion of matrimony in the society provides a future threat to a patrimony. But as long as a few favoured heirs are chosen to carry on each 'family line', then the remaining heirs are superfluous. In fact, the potential marriages of these remaining heirs can come to constitute outright threats to the natal patrimony, as each married sibling will hold a stronger future claim to an equal share of the patrimony. Unmarried siblings, however, do not pose threats to the patrimony as they do not normally produce (legitimate) offspring who will eventually demand provision. This is why the inheritance system is not one of primogeniture or unigeniture. Rather, it is a 'primonuptial' one - *the child who marries first* and remains in the natal household is usually the favoured one, and it is this child who is likely to obtain a central role in both household management and patrimony transmission. It is irrelevant which sibling this is: it may be the eldest, the youngest, or a middle sibling of either sex.

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<sup>15</sup> No suggestion of a feminine/masculine contrast is intended by the terms 'matrimony' and 'patrimony', nor of 'property in the female (or the male) line'. As we will see below, both the kinship and the inheritance systems of Fontelas are strictly bilateral and there is no detectable stress on male primogeniture. For convenience, my use of the term 'heir' henceforward in this paper will refer to *both* male heirs and female heiresses.

But the unmarried siblings do provide crucial sources of labour for their natal households and care for their parents as they age. Here we find the role of death once again prominent. As inheritance in Fontelas is only linked to death and not to marriage, the members of the younger generation must simply *wait* until their parents die in order to acquire their shares of the patrimony. Although this implies a whole series of undesirable conditions for most of the heirs, from another angle it ensures particularly well for the care of the aged parents. The inheritance system thus contains a 'built-in' form of old-age security. Because no transfer of property takes place at any of the marriages of the heirs, the division of the patrimony is as it were 'stored up' until a much later stage in the developmental cycle of the household. Parents in no way 'retire' in Fontelas following any of the marriages of their children, and there is no 'west room' such as that described by Arensberg and Kimball for Ireland<sup>16</sup> into which the elderly couple physically move as they age. On the contrary, in Fontelas the elder generation 'hold the strings' of the patrimony (the farming house, land, and equipment) literally until their dying day. Indeed, as Goody maintains: 'Late transmission retains generational control; early transmission weakens it'.<sup>17</sup>

Here lies the reasoning behind the selection of a favoured heir. By choosing one son or daughter to manage the household as they age, the parents can begin to create the conditions under which this heir can maintain the patrimony *relatively* intact following the parents' deaths. This is why so many villagers marry late, never marry, or settle for celibacy or bastardy. There is only one point (the death of a parent) in the developmental cycle of the household and not two (the marriage of the favoured heir) at which property is redistributed. All of the aspects of matrimony are thus suppressed, limited, and strictly controlled in the name of preserving the natal patrimony. This opposition does not constitute merely an observer's prism - an analytical device - but rather an empirical reality lived by villagers themselves. The tension between matrimony and patrimony indeed pervades the entire society and we will see that its long-term effects are quite wide-ranging.

One clear illustration of this opposition is visible in some of the legal aspects of the strong descent tie between parents and children which I have alluded to earlier. Not only are the ties between parents and children very close socially, but they are also consecrated within the system of inheritance through the sequential order of heirs. Until the substantial legal changes in

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<sup>16</sup> Conrad Arensberg and Solon Kimball, *Family and Community in Ireland*, Cambridge, Massachusetts: Harvard University Press 1940.

<sup>17</sup> *Family and Inheritance*, p.28.

the Portuguese Civil Code in 1977,<sup>18</sup> the order of heirs to the patrimony of an individual dying intestate was as follows (*Código Civil*; art. 2133):

1. Descendants
2. Ascendants
3. Siblings and their descendants
4. Spouse
5. Other collaterals to the 6th degree
6. The State

Note that the surviving spouse figures only *fourth* in line following three other categories of legitimate heirs. The implications of this system are that the surviving spouse is left in a particularly weak position, his/her partner's property being transferred downwards rather than sideways. It is a person's descendants, ascendants and siblings-and-their-descendants who inherit *before* the surviving spouse. In other words, property tends to pass downwards along consanguineal kinship lines and not sideways through affinal or collateral ties formed by marriage. A surviving spouse is thus as it were 'left out on a limb'. Again, we find relations of affinity underplayed in the face of relations of descent.

Even the apparent joining of the property of each spouse at marriage is itself a predominantly 'downward' and descent-oriented procedure. This is why *the* separate partitions of property may theoretically be effected by a couple's children, each following the death of one of the parents. Upon the death of the first parent, one half of the couple's property is inherited immediately and directly by the descendants, while the surviving spouse inherits the other half. The second parent's half of the original patrimony is then divided after his/her death. Of course, the children (or other heirs) may and often do *delay* the partition of the first parent's patrimony until the death of the second parent.

However, if a couple produce no descendants and remain childless (both dying intestate), upon the first partner's death his/her property is not inherited by the surviving spouse. Rather, the person's property reverts to his/her 'family line' following the principle of *paterna paternis/materna maternis* (the father's property to the father's kin/the mother's property to the mother's kin).<sup>19</sup> The bulk of the patrimony of the first spouse returns to

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<sup>18</sup> I refer here to the major alterations in the 1966 Civil Code introduced by the Decreto-Lei No. 496/77 of 25 November 1977. These alterations have considerably improved the formerly weak position of the surviving spouse in inheritance, although the extent to which these legal changes have affected actual inheritance practices *socially* within Fontelas will have to await future study.

<sup>19</sup> See Le Roy Ladurie, *op.cit.*, p.58.

to his or her natal family.<sup>20</sup> In this sense, the interests of *neither* of the natal family groups of two childless spouses are ultimately affected by the marriage: each spouse's patrimony returns to its original line rather than being inherited by the surviving spouse. Although the example of childless couples is an exceptional one, the legal rules of property distribution are clarified by it. Descent lines everywhere predominate in Fontelas over weak marital ties. Precisely as described for the regions of western France, inheritance customs in Fontelas oblige villagers to 'attach only slight importance to the act of marriage which they seem to regard as an ephemeral union of two perishable creatures, each issued from a different line whose own value lies in its permanence.'<sup>21</sup> Indeed, it is the preservation of patrimony that preoccupies villagers in Fontelas, and not the continuity or coherence of matrimony.<sup>22</sup>

It is not my objective here to raise complicated questions concerning the interrelations between administrative law and customary law,<sup>23</sup> but rather merely to stress that the system of inheritance in Fontelas places great emphasis upon death and relations of descent and not upon marriage and relations of affinity. This is why 'matrimony' and 'patrimony' seem so forcefully opposed. I follow Jack Goody here in maintaining that different societies (and varying regions within one society) place

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<sup>20</sup> See David Sabeau, 'Aspects of Kinship Behaviour and Property in Rural Western Europe before 1800', in Goody, *Family and Inheritance*, pp.96-111; at p.105.

<sup>21</sup> Le Roy Ladurie, *op.cit.*, p.56.

<sup>22</sup> Although this paper focuses primarily on *patrimony*, the entire topic of matrimony and 'restricted marriage' deserves equal attention. Some of the major features of this society's limitation of marriage include: the absence of dowries, the 'natolocal' residence of spouses with each of their sets of parents for many years after their marriage, and the absence of a highly structured courtship system. The mean age at first marriage for men in Fontelas from 1870 to 1978 has been 33.2 and for women 31.0 (Parish Register - marriage entries): these figures provide an excellent example of John Hajnal's classic 'European marriage pattern' (see his 'European Marriage Patterns in Perspective', in D.V. Glass and D.E.C. Eversley (eds.), *Population in History: Essays in Historical Demography*, London: Edward Arnold 1965, pp.104-143). Further, an 'alternative' tradition among the poor of consensual unions, illegitimacy, and non-marital sexuality invites further analysis.

<sup>23</sup> See Pierre Bourdieu's excellent essay (*op.cit.*, particularly pp.142-44) for some penetrating insights into the disjunction between abstract legal statutes, regional customary law, and the actual practices and strategies of individuals on the ground.

differing emphases upon key points in the developmental cycle when property passes from one generation to the next. Societies in which marriage constitutes a key moment of property transfer<sup>24</sup> will consequently have radically different features from those in which property is transferred only at death.

This key 'fulcral moment' of property transfer within the social system of Fontelas is clearly death, and our earlier examination of the process of 'dying' leads us directly to this point. This is why I have insisted upon a perspective which focuses not merely on the individual death as a ritual moment, but also upon the whole fabric of social and property relations within which each specific death is embedded. The preponderant role of death in conditioning property transfers is visible both before a death (in forms of gossip about the deceased's property) as well as after a death, through the weeks and months when the patrimony is redistributed. This is why *both* the related processes of dying and of inheriting must be analysed in their respective short-term and long-term dimensions.

But these short periods of time give us only a hint, an inviting clue, to the much longer processes involved in dying and inheriting in Fontelas. Let us turn now to a different dimension, a somewhat more diachronic one, in order to delve deeper into the question. Has matrimony been so strictly controlled in the past in Fontelas, and has the role of death in inheritance always been so strong? In answering this question, we will need to look at a second 'time dimension': that of the long-term effects of *post-mortem* inheritance. Here we will 'telescope' our perception of time even further beyond the short-term periods preceding and following a death. I will begin to focus upon the topic of the reproduction of social groups. In other words, what are the repercussions at a wider level of the transfer of property at death? What effects does this specific mode of inheritance have upon the overall shape of the social structure over time?

### III. *Inheritance and Social Structure*

There are four major ways through which a *partilha* (partition) of property can be effected in Fontelas. Let us look briefly at these before dealing with the main theme of this section: the long-term effects of delayed inheritance. It is crucial that we grasp the specifics of inheritance partitions here before turning to these wider implications.

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<sup>24</sup> See John Davis, *Land and Family in Pisticci*, London: Athlone Press 1973; and Peter Loizos, 'Changes in Property Transfer among Greek Cypriot Villagers', *Man* (n.s.), Vol.X (1975), pp.503-523.

Inheritance partitions in Fontelas follow one of these four procedures:

1. *De Boca* (orally)
2. *Sortes* (lottery)
3. *Escritura* (will or donation)
4. *Justiça* (court division)

An oral partition is by far the most frequent form of division of a patrimony. Upon the death of an individual (assuming for the moment that there are children) one half of the patrimony<sup>25</sup> is partitioned in equal shares among the person's direct descendants. The second half is then divided following the death of the surviving spouse. Although there is a tendency in Fontelas to delay the partition until both parents have died, even then the legal steps involved do not always coincide with actual practice. An oral partition may occur, but the heirs may continue to cultivate a landholding jointly or to reside within the same house. If the parents die intestate, each heir has rights to an equal share of land, movables, livestock, and a portion of the house. There is an *absolute equality* between male and female heirs both in local hamlet custom as well as in the Portuguese Civil Code, and as I have pointed out there is no informal preference within the inheritance system for either sons or daughters.

With respect to land, three main types of distribution occur:

- (a) plots may be partitioned into smaller plots, with boundary stones placed at the borders;
- (b) different heirs may each obtain separate plots of roughly equal values;
- (c) heirs remaining in agricultural activities may borrow, look after, rent, or purchase the plots of their non-resident co-heirs.

If we were to accept the popular view of smallholding regions being subject to ineluctible fragmentation, then type (a) would seem to us to be the logical practice. In fact, precisely the opposite occurs in the hamlet. Except in cases of very large plots of land, (b) and (c) are the most frequent choices, as these lead to a more practical distribution of farming land among those siblings remaining permanently in Fontelas. Obviously, an

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<sup>25</sup> There are a number of couples in Fontelas who have married under the 'separate property' regime (*separação de bens*) rather than the more frequent 'joint property' regime (*comunhão de bens*). The former arrangement is followed principally in cases of remarriage in order to retain separate inheritance rights for a person's children by a former marriage, and it implies a different series of legal options concerning the disposal of property by spouses from those available to spouses married under the 'joint property' regime.

extreme adherence to (a) through continual subdivisions of shrinking plots would lead eventually to utterly miniscule sub-plots. Thus (b) and (c) are resorted to in order to circumvent this.

Oral partition is termed by villagers *de boca* simply because that is precisely how it is done - 'by word of mouth'. Nothing is written down anywhere, and each heir inherits his or her share.<sup>26</sup> Alternatively, in other cases all of the plots of a group of heirs may be listed at the time of inheritance in each of their names. The *partilha* is still nevertheless termed an oral one because formal recourse to assessors or lawyers is not resorted to. This is why the term *amigável* (friendly) is used to refer to this form of division: all of the heirs divide the patrimony *amigavelmente*, or 'in a friendly way'. Each heir registers his/her plots (or new portions of plots) and all remain on good terms. Following the 8 deaths occurring during my fieldwork, 6 oral partitions were effected. *De boca* division is the ideal form of a *partilha* in Fontelas and indeed the most common.

A second form of partition is that involving a 'lottery' between the heirs. This form of inheritance is similar to oral partition except that a mediator, or witness, is called in from outside the household(s) in question in order to assure an equitable division. The word *sorte* (share/lot) refers, first, to the small slips of paper on which the locations of a series of plots of land are written: these slips of paper are placed in a hat and drawn randomly by the heirs. The second meaning of *sorte* refers specifically to plots or sub-plots themselves from this point on. Any plot in the hamlet can thus be termed 'my *sorte*' or 'my brother's *sorte*'. The lottery system implies some degree of caution, otherwise no outside witness would be called in and the heirs could effect the partition orally. This system also implies a certain amount of equally distributed 'luck' in the drawing. The witness is usually a villager of high social standing with a good knowledge of the hamlet's terrain. Along with the heirs, the witness draws up a list of all the deceased's land.<sup>27</sup> It is at this point, shortly prior to the drawing, that

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<sup>26</sup> This is why the town Tax Bureau has such difficulty in keeping track of who owns what in each of the 95 hamlets within its jurisdiction. Plots frequently remain registered under the name of a villager's parent or grandparent. This may temporarily avoid both a minimal inheritance tax as well as a yearly land tax, but it may lead to problems in cases of sale or disputed ownership rights. Such disputes force an heir to obtain a proper legal title to the plot(s) in question.

<sup>27</sup> This lottery form of partition was described to me by a number of villagers, although I did not observe it in practice. I am not sure therefore whether movables are also written on the paper slips; they may have been distributed orally.

the value of the plots must be decided and that disagreements are most likely to occur. A landholding consisting of 20 plots to be divided among 4 heirs may, theoretically, be partitioned into 4 portions of 5 plots each. But subdivisions may also be made, or alternatively, for example, three less fertile grain plots may be put on one slip of paper and one very fertile meadow on another. Each heir thus draws a slip of paper with a series of listed plots whose total value is roughly equal.

The essential point about the lottery is that it is a variant of the 'friendly' oral partition. Although not as straightforward as an oral partition, divisions by lot do not go as far as the courts. This is why the lottery is relatively rare: either the heirs basically agree and opt for an entirely oral partition, or they disagree to such an extent that one (or more) of them demands recourse to legal bodies. A partition by *sortes* is thus a form of compromise between an entirely 'friendly' agreement among co-heirs and disagreements strong enough to commence legal action.

A third type of inheritance involves the making of a will (*testamento*) or a donation (*doação*). Both of these forms of property transfer are termed inheritance 'by writing' (*de escritura*) and both are considered by villagers to be processes quite distinct from purely oral or lottery divisions. A will or a donation both inject a written document into the process of property transfer. According to Portuguese law, a person may only freely dispose of 1/3 of his or her property through a will (this percentage is 1/2 if there is only one child).<sup>28</sup> This portion is termed the 'third' or *terço* in Fontelas. In legal terms this constitutes the testator's 'free quota' or *quota disponível*. The remaining 2/3 constitutes the legitimate inheritance (*legítima*) of the testator's heirs, and cannot be included in a will (Civil Code; art. 2158). If the heirs feel that their *legítima* has been mistakenly or wrongfully encroached on by a will, they may dispute this in court and recover the portions of their patrimony incorrectly included in the will.<sup>29</sup> However, even in cases of legally correct wills, there is a strong feeling in Fontelas that wills subvert the law of equal partition between all siblings. This contrast between the generalized 'ideology' of equal partition and the simultaneous wariness about wills constitutes a fundamental contradiction within this society, and I will return to it below.

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<sup>28</sup> Again, I am describing the functioning of inheritance here *prior* to the 1977 alterations in the Civil Code (see Note 18 above).

<sup>29</sup> The legal procedures glossed over here are obviously more complex than I have made out. Furthermore, knowledge of the legal rules of inheritance is of course only of limited use in following the actual practices and specific 'moves' of individuals on the ground, as Bourdieu's brilliant analysis (*op.cit.*) of marriage and inheritance strategies in a southern French village has shown.

In a will, a testator may bequeath any or all of his/her free quota to anyone - these may be one or more of the legal heirs, a spouse (more rarely), or an unrelated person. A donation (*doação*) is another matter entirely. This is a *pre-mortem* transfer of property and as such the closest thing to a dowry existent in Fontelas. A donation is really a kind of pseudo-will, or an advance on inheritance, as the portion of property disposed of through a donation already dips into the testator's free quota of 1/3 of the patrimony. The donation thus leaves less property to dispose of through a later will. It is virtually the only means available in this society (apart from sales and purchases) to transfer property *inter vivos* and avoid the strict rule of *post-mortem* inheritance.

According to villagers today, donations are not a frequent form of property transfer, although when they do take place they always involve (like wills) a certain degree of public gossip and suspicion. Villagers were very reluctant to discuss donations involving themselves, although they are constantly speculating about the contents of those of others. Such phrases as *deixou-lhe aquela leira* ('he left him that field') or *a tia fez-lhe doação da casa* ('her aunt donated the house to her') are endlessly on people's lips. This gossip about donations abounds at any time in Fontelas, unlike the gossip described earlier which arises immediately prior to a villager's death. This is due, of course, to the timing of *pre-mortem* donations: donations may take place at any point in an individual's life-cycle. A donation is thus quite an 'odd' form of property transfer in Fontelas as it is not strictly tied to death (although the amount of property included in the donation reappears later in the inheritance redistribution). Particular attention is given in Fontelas to both donations and wills because these lend an aura of suspicion and hidden calculation to the *partilha*. Who has obtained what, and through which means? Did a specific person really deserve to inherit through a will? Or did he/she fiddle the documents or 'play up' to the owner purely for personal interest? Nor are parents the only objects of a 'greedy' heir's intentions: an unmarried aunt or uncle is always a potential target. Hidden plans and schemes may lurk beneath the 'honest' exteriors of caring children, as expressed in the phrase:

*O cordeirinho manso,  
Mama a sua mãe e alheia.*

The meek little lamb,  
Sucks from its mother and  
others too.

The inclusion of a will at the time of a *partilha* does occasionally occur in Fontelas, although my impression is that it is an exception among households with descendants and more common in cases of elderly, unmarried individuals. In none of the 8 cases of individuals who died during my fieldwork was a written will drawn up, and none of the heirs involved in the 8 deaths had received a donation. Following my fieldwork, in 1981 one widowed woman (with children) left a will and another unmarried woman (childless) died later in the year also leaving a will. In

general, villagers in Fontelas view the making of a will with great suspicion. This is due not only to a general aversion to institutionalized 'favouritism' but also to a deeply rooted fear of the legal manipulations of selfish heirs and bribed notaries. Conflicts are most likely to arise in cases of wills and not in cases of oral or lottery partitions, although this is not to say that in the latter there is necessarily an absence of disputes. Of course, a will need not be disputed, and the heirs of the remaining 2/3 of the patrimony may divide that 2/3 orally and amicably between themselves. Nevertheless, a suspect will, in writing and legally binding, is more likely to explode latent disagreements and resentments and to shift the whole affair to the town court.

The fourth kind of inheritance procedure in Fontelas involves extreme cases of disputes which force a legal partition in court. Villagers term this process simply one of inheritance 'by law' or 'through justice' (*por justiça*). I observed a number of cases of disputed inheritance in the town court involving individuals from other hamlets in the municipality. When this occurs, the heirs as well as their witnesses may completely sever all social relations. The partition itself may require *in situ* valuations of the plots in question by land assessors, the lawyers involved, and the judge. As legal costs are high, recourse to lawyers and the court is an expensive affair and in villagers' eyes must involve enough property to merit the various expenses incurred. Indeed, actual legal recourse to lawyers and courts is far less common than indirect gossip or verbal threats. There is even a bit of suspicion of the lawyers themselves, whom some villagers view as liars and smooth talkers who manipulate the law for their own and their clients' interests. Nevertheless, in cases of a deadlock in inheritance the court provides a last resort. Although expensive, a legal division (often lasting for years) usually succeeds in resolving the original dispute.<sup>30</sup>

These then are the four principal means of transferring property between the generations in Fontelas. Of particular interest for our argument is the fact that none of the procedures enumerated here involves the transmission of property or the partial division of a patrimony *at the moment of marriage*. The only form of *pre-mortem* transfer is through donation, yet even this relatively rare procedure does not necessarily involve marriage. Anyone (married or not) may be the recipient of a donation and there is no social custom or habit of giving donations at the time of a villager's marriage. Virtually all of the forms a partition can take (oral, lottery, will, and court division)

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<sup>30</sup> Another form of partition involving the town court (although of less relevance to this paper) arises in cases of minor children whose parents die before their having reached majority. In these cases, a mandatory legal partition is effected in court called an *inventário de menores* (probate inventory).

only occur after an individual's death at a point in the developmental cycle of the deceased's household which is entirely unrelated to the timing of the marriages of any of the heirs. Even in the case of wills, although a will may be written many years prior to a villager's death, it cannot be brought into effect legally until the testator has died. The total absence of any form of dowry provisions, as well as the general cultural orientation towards 'restricted marriage', point unmistakably to a mode of inheritance strictly linked to death.

These then are the short-term elements of the process of inheriting in Fontelas; what are its long-term effects? In other words, what features of Fontelas' social structure can be said to be shaped significantly by this specific mode of property transfer? If we maintain that the particular timing of property transfer at death (and not at marriage) is a prime 'structural law' underlying the social system, then a number of concomitant features must be visible as predictable results. This is indeed the case. Let us now turn to our concluding point about dying and inheriting: the long-term reproduction of social-structural features in Fontelas linked to delayed inheritance at death.

Two major features of this form of delayed inheritance are (a) the separation between favoured and secondary heirs, and (b) extremely high ratios of illegitimacy. An analysis of both of these key features clarifies the pivotal role of death in the process of property transmission. Let us conclude by touching briefly on these two points.

Despite the legal rule of equal partition between all siblings described above, we would be very naive indeed if we actually believed that land and other forms of property are actually 'equally' divided. There is in Fontelas a sharp contradiction between this ideal of legal partibility and the real, practical inequality between favoured heirs and their tacitly excluded siblings. This is not to say that equal partitions never occur, but rather that some form of *social* differentiation occurs at an early stage in the developmental cycle of the household. This is an important point because it reveals a *dual* structure within the internal composition of the rural community.<sup>31</sup> This dual structure separates (a) those in favoured positions with access both to land and to social positions of household management, from (b) those disadvantaged villagers dependent either on the wealthy landed households or on their own favoured siblings.<sup>32</sup> All heirs do not normally

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<sup>31</sup> See Brian Juan O'Neill, 'Proprietários, Jornaleiros e Criados Numa Aldeia Transmontana desde 1886', *Estudos Contemporâneos* [Porto], No. 2/3 (1981), pp.31-73; at pp.62-64.

<sup>32</sup> A similar dual structure is visible at the hamlet level between (roughly speaking) the land-rich and the land-poor, and even in the spatial distribution of houses within the hamlet sub-sections. Smaller and poorer households tend to be located on the outskirts, while larger wealthy ones are concentrated in the centre.

stay in Fontelas in agriculture: some emigrate, some marry out, and others remain celibate. But indirectly, one of these heirs manages to consolidate much of the patrimony in the interests of avoiding both excessive partitions of land as well as the dispersal of 'labouring arms'. For instance, each co-heir theoretically inherits a share in the natal house, but such division of the physical structure of a house among six siblings would obviously be impractical.<sup>33</sup>

In most cases one heir remains in a central role within the family line and it is this heir who is informally chosen to manage the household and the landholding over time. This may be either a son or a daughter, or indeed even an adopted servant. Just how this favoured heir is chosen by the parents is an extremely subtle process, and we must bear in mind that there is not always a favoured heir in any given household. Furthermore, this selection process does not apply equally among the poor, where resources are generally scarcer. In landed households, one parent (or both) may will a portion of property to this favoured heir, although this may create friction or overt conflicts among the other co-heirs. But the practical needs of the household necessitate the elimination of competing claims to the house and the landholding from the other co-heirs (patrimony limiting matrimony). The marriages of these co-heirs will clearly add to the force of their claims. But the delay or prevention of their marriages, either by the parents or later by the favoured heir, will tend to diminish their claims and to relegate them to secondary, celibate positions within the household.

It is almost as if the house (or the household) takes on a kind of life of its own, regardless of the specific individuals who occupy positions within it. However, in Fontelas there are no institutionalized means through which the parents or the favoured heir can force other co-heirs to leave the hamlet entirely or relinquish their shares. I have found no contemporary evidence of male primogeniture, and today there are no compensation payments given to co-heirs in order to push them off the landholding. The most a parent can do legally is to favour one of the heirs with 1/3 (the *terço*) of his or her patrimony and a stronger social claim to retain management of the house. So, although excessive partition is generally avoided, some portion of most landholdings is bound to be split eventually.

The major strategy of the favoured heir is thus to postpone the partition, or if this is unavoidable, to buy out the

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<sup>33</sup> Of the total of about 80 inhabitable *houses* in the hamlet (including a few emigrants' houses, but many more older and currently empty ones) only 2 have been partitioned down the middle in recent decades.

shares<sup>34</sup> of the other co-heirs in order to consolidate the total holding. As marriage in Fontelas is generally late, by the time a partition takes place many co-heirs are already well into their 30s or 40s and long established somewhere or other. The varying life circumstances of each of these co-heirs at the time of the partition is crucial. Who has remained in Fontelas? Who has left the hamlet? Who will press for partition and who is likely to claim a share of the patrimony for immediate use? While all co-heirs are in this sense legally equal, socially their positions as claimants to the patrimony at the moment of partition may be distinctly unequal. If we were to emphasise merely the *legal equality* between heirs, the entire complex of *practical inequality* between favoured heirs and secondary co-heirs would be missed. As one woman put it so aptly, the ultimate result of a partition is not always equal: *Uns ficam com as vinhas, e outros com as pipas*. ('Some get the vineyards, and others the wine-barrels.')

One further element of crucial significance here is the link between death and the old-age security of elderly parents. As Goody has noted:

The link between stratification and the economy is by means of the system of inheritance, which organises the transmission of property from generation to generation, at death, at marriage or at some other point in the developmental cycle.<sup>35</sup>

*Post-mortem* transmission implies not only a delay in the acquisition of property by the young, but also a prolongation of the retention of property rights by the old. The fact that a formal

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<sup>34</sup> Sales and purchases of land in Fontelas do not seem to have been a frequent occurrence, although recent emigration may erroneously predispose us to assume that sales have always occurred regularly in the past. Far more common is the avoidance of *de jure* partition simply by maintaining a natal landholding and household intact through *de facto* joint administration of the legally partitioned shares. This does not involve sales of shares but rather pooling, lending, or simply the non-assumption of legal property rights by unmarried siblings.

<sup>35</sup> Jack Goody, *Production and Reproduction*, Cambridge: Cambridge University Press 1976, p.65.

transmission of an intact patrimony<sup>36</sup> does not occur at marriage in Fontelas implies that the parental generation does not in any sense lose control of the holding. Nor do they enter any form of 'retirement' from farming, unless utterly senile. In this sense, there is no legal (or indeed social) transfer of household authority in Fontelas even at the marriage of a favoured heir - this household authority is retained firmly by the parental generation.<sup>37</sup>

This is why the system of inheritance in Fontelas, although partible, actually looks like an impartible one. The structure of inheritance thus demands that one or more of the parents' children (or other kin) look after them with special care as they age. Favoured heirs in particular will also provide for the

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<sup>36</sup> A number of features of inheritance customs in Fontelas are comparable with other European communities where formal unigeniture is practised. There are important differences, however, particularly in the absence of any property transfer at marriage in Fontelas: this transfer at marriage does occur in the Irish, Spanish, and Italian Alpine communities described by Arensberg and Kimball (*op.cit.*), Douglass (*op.cit.*), Carmelo Lison-Tolosana (*Antropologia Cultural de Galicia*, Madrid: Siglo XXI de España Editores 1971), Raul Iturra ('Strategies in the Domestic Organization of Production in Rural Galicia (N.W. Spain)', *Cambridge Anthropology*, Vol. VI (1980), pp.88-129) and Eric Wolf and John Cole (*The Hidden Frontier: Ecology and Ethnicity in an Alpine Valley*, New York: Academic Press 1974). Whether such a formal system of unigeniture or strict single-heir inheritance has existed in Fontelas prior to the mid-nineteenth century is a historical question beyond the scope of this paper.

<sup>37</sup> Indeed, even in the Minho region of North-west Portugal marriage involves quite a different set of elements (see Brandão, *op.cit.*). Brandão notes that in the Minho '... in general, marriage consummated the change in the positions of the two couples in the household, when the newly-weds acquired the leading position' (Brandão, *ibid.*, note 18). In Fontelas, even cases of uxorilocal and virilocal residence do not imply such a change of position for the younger couple. Natolocal residence even further highlights the near-obliteration of the newlyweds' claims to status and the supreme authority of the parental generation. The crucial point of contrast here between the region studied by Brandão and this one, I think, lies in the difference in the *timing* of key property transfers (see also Robert Rowland and Fatima Brandão, 'História da Propriedade e Comunidade Rural: Questões de Método', *Análise Social*, Vol. XVI (1980), pp.173-207). In the Minho region, major transfers of property accompany *marriage* while in this area of Trás-os-Montes such transfers occur only following the *deaths* of the parents.

parents' funeral expenses, look after their graves on All Souls' Day, and have at least the prescribed series of masses said for the parents following their deaths. The parental generation thus retains near-complete control of both land and labour while they remain alive. The selection of a favoured heir further ensures this old-age security. Indeed, one elderly villager quoted the appropriate saying:

|                                  |                                 |
|----------------------------------|---------------------------------|
| <i>Quem é teu herdeiro?</i>      | Who is your heir?               |
| <i>Quem te limpa o traseiro.</i> | The one who cleans your behind. |

Again, the close link between death and the delayed transmission of property is evident. The special care given by the favoured heir to the ageing parents is institutionally cemented within the structural law of inheritance at death.

But one of the most flagrant results of this form of delayed inheritance is the presence of consistently high proportions of illegitimacy in Fontelas over time. A detailed count of all baptisms recorded in the Parish Register<sup>38</sup> for Fontelas since 1870 yields the following information. The overall illegitimacy ratios in Fontelas range in the late nineteenth century from over 1/3 to almost 1/2 of all baptisms. The mean illegitimacy ratio (bastards baptised as a percentage of all children baptised) over the century has been 47.4 per cent: 326 in a total of 688 baptisms. In other words, throughout the last century (1870-1978) virtually one half of all children baptised in Fontelas have been bastards.<sup>39</sup>

In the decade from 1910 to 1919, bastard children accounted for 73.6 per cent of all children baptised. Three out of four children baptised during that decade were illegitimate (there were 19 baptisms of legitimate children but 53 of bastards). From 1920 to 1929 this percentage was 67.9 per cent. Finally, even in the decade from 1950 to 1959, the ratio was 47.5 per

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<sup>38</sup> There is no doubt that these figures in and of themselves, are quite reliable, as each baptismal entry in the Parish Register records the marital status of the mother of a bastard child as 'single' (*solteira*) as well as the standard phrase 'unknown father' (*pai incógnito*).

<sup>39</sup> The overall illegitimacy ratios in the parish as a whole (around 700 inhabitants) for the last three decades of the nineteenth century yield similar figures: 29.2 per cent for 1870-79, 35.2 per cent for the 1880s, and 40.8 per cent for 1890-99. During the first three decades of the twentieth century, the parish ratios were respectively 44.1 per cent, 46.0 per cent, and 42.3 per cent.

cent.<sup>40</sup> Only later in the 1960s and 1970s did illegitimacy suffer a marked decline, with respective ratios of 23.4 per cent and 7.4 per cent. Recent emigration has undoubtedly contributed to this decline in the last two decades, but this emigration in itself does not explain why the ratios have been so consistently high in the past.

Analysis of the occupations of the mothers of bastard children reveals that unmarried women in the social group of *jornaleiros* (day-labourers) have accounted for the majority of bastard births. Almost as consistent has been the tendency among women from the social groups of *lavradores* (ploughers) and *proprietários* (large landowners) to bear legitimate children within legal marriage. Since 1870, 172 bastard children and 87 legitimate ones have been born to *jornaleira* mothers, while in contrast only 10 bastard children yet 102 legitimate ones have been born to *lavradora* mothers. The basic pattern of the illegitimacy figures thus points overwhelmingly to bastard births among the poor, and legitimate ones among the landed. The exclusion of these unmarried mothers from *both* matrimony and patrimony has been quite evident as far back as local records go. Villagers themselves produce apparently contradictory explanations for illegitimacy. The wealthy and the poor view the problem from opposite ends, and both sides systematically blame the other. The poor, especially many single mothers themselves, blame wealthy men for 'exploiting us as servants or day-labourers' and burdening them with bastard children. Yet the wealthy blame the poor simply for being poor - 'how can they help themselves, those poor souls?' Those on the bottom of the social hierarchy seek economic and political causes, while those on the top find moral ones.

My point here is that neither side is correct, but that the entire problem is shaped from the start by a specific mode of

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<sup>40</sup> A far broader survey of the literature on illegitimacy in various disciplines is needed here, considering the near-total absence of analyses of the topic in the major anthropological monographs of European communities. Although I have not yet carried out systematic comparisons of illegitimacy in other rural areas of Portugal and Europe, some suggestive figures are available from the Austrian mountains: Michael Mitterauer ('Marriage Without Co-residence: A Special Type of Historic Family Forms in Rural Carinthia', *Journal of Family History*, Vol. VI (1981), pp.177-181) has reported a 68.1 per cent illegitimacy ratio for 1870-74 in an Eastern Alpine region of Austria. Further research on rural illegitimacy in Portugal is being carried out in the Historical Sociology Unit of the Instituto Gulbenkian de Ciência (Oeiras, Portugal). My brief analysis of illegitimacy here clearly places far greater emphasis on factors involving property and household structure than on religious morality or the even less applicable topic (in this case) of 'honour and shame'.

inheritance. Not all fathers of bastard children are wealthy, nor are all of the relations leading to illegitimate children necessarily exploitative. Many fathers were simply co-heirs left outside (or 'pushed' outside) the main lines of property transmission in their natal households. Yet for poorer women, children are always an asset as their labour is highly valued. Better a bastard child than none at all. We see then that even among the poor, 'descent' and sibling ties are strong while ties of marriage and affinity are comparatively weak. As Le Roy Ladurie has noted for the west of France,<sup>41</sup> the inheritance customs of this region of Portugal 'smile on children, but not on love'.

The ratios of illegitimacy over the decades provide ample evidence of a persistent pattern of the social reproduction<sup>42</sup> of a 'bastard group' over time. The maintenance of a group of day-labouring women (the *jornaleiras*) and their illegitimate children has provided a source of temporary hired labour for wealthy landed households. Many servants in the hamlet's wealthy households have come from this poorer group. But the key point is that the wealthy and the landed have managed to retain this source of wage labour *without* posing threats to the bulk of the hamlet's patrimonies. Note that unless their paternity is legally recognized (a rare occurrence), illegitimate children inherit property only from their mothers. (The inheritance rights of illegitimate children have also undergone legal changes since the 1977 alterations in the Civil Code; see note 18 above.) But the illegitimacy ratios of Fontelas are suggestive: bastard births are so common that the society's general level of tolerance for illegitimacy must be quite high. Even today in Fontelas, over 1/3 of the hamlet's adult women over 21 have borne at least one bastard child at some point in their lives. Bastard children and single mothers although low in social status, are accepted as social individuals and non-marital unions are openly tolerated.

This does not mean that some stigma is not also present. Bastards are called *zorros* or *zorras* (the term suggests both 'fox' and 'rascal') while verses such as that quoted below indicate the 'inheritance' of the low status of poor single mothers:

*Da ovelha ruim,  
Não sai cordeiro bom.*

From a bad ewe,  
A good lamb does not come.

<sup>41</sup> Le Roy Ladurie, *op.cit.*, p.58.

<sup>42</sup> The term 'social reproduction' (like 'strategy') is borrowed here directly from Pierre Bourdieu (*op.cit.*), although comparable uses of the term can also be found in Lisón-Tolosana (*Belmonte*) and Peter Laslett (*Family Life and Illicit Love in Earlier Generations*, Cambridge: Cambridge University Press 1977).

Irregular sex is thus in the long run a mechanism, or a somewhat indirect path, for the satisfaction of sexuality and the reproduction of labour outside the central spheres of marriage and property. A large part of the hamlet population must remain marginal to the delicate balance between prestigious matrimony and undivided patrimony. Few are those who obtain *both* prestigious marriage and a large share of the patrimony, but many are those who obtain neither. The latter are however allowed a considerable amount of choice of sexual partners, much as Georges Duby has noted for medieval France: 'Under this ethic, marriage, I repeat, regulated the sexual impulses, but only in the interest of a patrimony. As long as no inheritance was involved, sexual activity was permitted outside marriage'.<sup>43</sup> Viewed in this way, the 'problem' in Fontelas is thus not illegitimacy but marriage itself.

The link between illegitimacy and inheritance is now clear. I submit that the main reason for these illegitimacy ratios lies in the mode of property transfer I have been describing. As long as a high rate of *illegitimate* fertility exists, a large part of the population is strictly prevented from both marrying and inheriting. They have no easy road to either matrimony or patrimony. This does not mean that illegitimate children do not later marry or inherit at all, but merely that their chances are slim from the start. The needs of the peasant household are so delicately balanced between labour and land in this isolated mountain region, that the inheritance claims of some villagers must be obliterated. These villagers may *produce* within their natal households, but they must not *reproduce* within them. This is why the fundamental concern of maintaining patrimonies 'intact' is not merely one of population control: the lower group of the *jornaleiras* and their bastard children may continually expand in numbers without threatening the basis of the inheritance of patrimony. In other words, the prize of prestigious matrimony is reserved for only a few, and the rest must either emigrate, marry out, or settle for celibacy or bastardy.

The extreme delay in inheriting property thus shapes the structure of this society's peculiar features. The only way in which the society's fundamental contradiction between the legal equality of heirs and the practical inequality between favoured heirs and secondary co-heirs is ever resolved is through the illicit links between these co-heirs and unmarried mothers. A 'bastard class' is the final result, and it is these unmarried male co-heirs who are in fact the 'unknown fathers' of bastard children.

In consequence, there must be an actual approval (however indirect) of illegitimacy and non-marital sexuality for the purpose of keeping patrimonies together. Only a few favoured

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<sup>43</sup> Georges Duby, *Medieval Marriage: Two Models from Twelfth-Century France*, Baltimore: Johns Hopkins University Press 1978, p.7.

heirs - primarily among the wealthy landed groups - ever connect marriage, property, and 'respectable' sexuality. But for the rest, these three are disconnected. If an acceptable level of non-marital sex can exist (although the *jornaleiras* have borne most of the burden) then a substantial number of people can be kept out of the competition for patrimony. But the general limitation of marriage in the society still allows for a few favoured and prestigious marriages. It is in this sense that the two sides of the coin support each other: prestigious marriage could not remain prestigious without the reproduction of a group of people systematically locked out of both matrimony and patrimony. The chosen few hold onto land, while the many excluded lose their grip. Much like the myriad branches of a tree, many more villagers fall 'outwards' from their natal landholdings (and downwards in the social hierarchy) than those who retain control of the central 'trunk' of the patrimony.<sup>44</sup>

I do not wish to be misunderstood in this paper, and to be misjudged for placing undue emphasis on an abstract system of inheritance. Clearly, anthropologists study living people just as much as they analyse the structure of societies. Obviously, villagers in Fontelas live within a system of customary law and strategize their moves in myriad ways. I do not mean to imply that these villagers are passive victims of the 'mindless application of inheritance laws'.<sup>45</sup> As Pierre Bourdieu would maintain,<sup>46</sup> villagers' strategies are conditioned by a whole structure of property relations accumulated over the generations.

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<sup>44</sup> Georges Duby's fascinating essay on medieval marriage in France suggests that the limitation of marriage in the interest of an undivided patrimony may have been a very old European pattern. For twelfth-century northern France, Duby writes that '...the concerted effort of families to deprive most of their sons of legitimate procreation was bound to weaken the dynasty. And yet our text shows just as clearly that this policy attained its main objective, for it did preserve the wealth of the house. In the course of these two centuries covered by Lambert's account, none of the main trunks of these lineages permitted the sprouting of sidebranches, that is, the division of inheritances. At every generation, one male only took over the entire patrimony. This was made possible by the elimination of his brothers, who were shunted toward the clergy or a monastery, toward adventures in the Holy Land or in England, or toward the deadly hazards of military apprenticeship and practice' (Duby, *op.cit.*, p.102).

<sup>45</sup> Davis, *op.cit.*, p.111.

<sup>46</sup> *Op.cit.*; see also his *Outline of a Theory of Practice*, Cambridge: Cambridge University Press 1977.

In this sense, each individual (or each specific death) is merely one among a long chain of successive links, and this is why I have stressed the long-term *processes* of dying and inheriting.

There is an interconnection over time in any society between kinship relations and property relations,<sup>47</sup> although the precise features of this interconnection obviously vary from society to society, from region to region. The goal in mountain regions of Europe seems to be the same nevertheless: the preservation of viable landed patrimonies through the avoidance of partition. As Bourdieu quotes the clever phrase of Marx: 'The beneficiary of the entail, the eldest son, belongs to the land. The land inherits him'.<sup>48</sup> This society achieves this goal through blocking *one* of the possible moments of property transfer (marriage) by delaying this transfer until death.

What I wish to stress here is that Fontelas provides a particularly clear test-case for Goody's recent statements about family and inheritance in rural Europe. These are the extreme results of a system of delayed inheritance: late marriage, celibacy, complex households, natolocal residence, stress on descent over affinity, and illegitimacy. Also present in Fontelas are other related features of this characteristically 'European' family organization, also described by Goody in *Production and Reproduction* (Cambridge 1976): spinsters, bachelors, 'informal marriage' and the occasional concubine, abandoned children, step-parents and step-children, half-siblings, and some female servants suggestive of the Cinderella tale. I have shown that some of these features are closely linked to a specific form of property transmission. These features are clearly shaped by a certain structural logic - this logic in itself depends to a great extent on the transmission of property at death. An understanding of the *timing* of key property transfers in Fontelas is crucial to our analysis of the rest of the society. These two processes of dying and inheriting (stretched out to incorporate the long-term periods preceding and following a death) invite further study. This region of Portugal provides some extremely unusual characteristics which suggest comparison with other regions of mountain Europe. Further research along these lines, linking varying forms of

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<sup>47</sup> On this point, once again Goody makes the key link: 'The manner of splitting property is a manner of splitting people; it creates (or in some cases reflects) a particular constellation of ties and cleavages between husband and wife, parents and children, sibling and sibling, as well as between wider kin' (Goody, *Family and Inheritance*, p.3). Fontelas provides an excellent example of this comment on the relation between kinship ties and property relations - in Fontelas it is the manner of 'splitting property' that clearly conditions strong parent/child ties and contrastingly weak husband/wife ties.

<sup>48</sup> 'Marriage Strategies', p.117.

kinship relations with property relations, will prove particularly stimulating for future interdisciplinary work in anthropology and social history.

Underlying the peculiar characteristics I have described for Fontelas, and providing them with their basic structural shape, is the fact of partible inheritance at death. The transmission of all major patrimony in Fontelas is tied to this one event. This then is the underlying structural law which conditions the basic contours of this rural community. The *ritual* aspects of dying and the *legal* elements of inheriting may only lead us to a partial, skewed view of a long historical process. By isolating these discrete events, we may lose sight of the time dimension through which patrimony is passed from one generation to the next. In Fontelas this is only achieved through dying and inheriting, and it is these two closely linked processes which hold the key to the entire social structure.

BRIAN JUAN O'NEILL

## PORTUGUESE STUDIES IN OXFORD

Oxford's interest in Portuguese goes back a very long way. There were students from Portugal at the University in the Middle Ages, and in the sixteenth century - as Mr McNeill explains below in his account of the Portuguese holdings of the Bodleian Library - the University's lust for Portuguese material was such that it appears to have resorted to force to obtain it.

The modern history of Portuguese at Oxford, however, began exactly fifty years ago, in November 1933, when the University accepted, from the Junta de Educação Nacional, 'the sum of £700 sterling per annum for the maintenance of a Lecturer in Portuguese Studies ... and generally for the furtherance of Portuguese Studies at Oxford'. Professor António Augusto Gonçalves Rodrigues, who arrived in Oxford in the following year, was the first of a series of distinguished Portuguese teachers (now known as *leitores* or *leitoras*) who have continued to work here ever since. The present *leitora*, Miss Maria Teresa Pinto-Pereira, took up office in 1980. The generous financial support of the Portuguese Government, which has greatly increased its initial grant, has continued throughout the period, and is now administered by the Instituto de Cultura e Língua Portuguesa. Since 1968 the University has also maintained a lectureship in Portuguese Studies from its own funds.

Thanks to an *ad hoc* grant from the Gulbenkian Foundation St Antony's College was able to appoint a Portuguese Research Fellow, Dr Manuel V. Cabral, a specialist in rural social structure and change, with wide research interests on questions concerning modernisation, the formation of the working class and the social bases of Fascism. Some of his work is available in English, including papers in the *Journal of Peasant Studies* and elsewhere. The College is seeking to create a permanent Portuguese Fellowship

to enable young Portuguese in the field of social and historical studies to pursue research in this area in Oxford.

In addition to the Portuguese *leitor* and the eventual Portuguese Fellow at St Antony's, three permanent members of Oxford's academic staff research in Portuguese studies : Dr David Goldey (Lincoln), in contemporary politics, Dr Tom Earle (Linacre), in modern Portuguese literature, and Herminio Martins (St Antony's), in sociology and politics.

The study of the Portuguese language and literature is controlled by the Faculty of Medieval and Modern Languages. The place of Portuguese in the faculty is exactly the same as that of the other modern languages taught at Oxford. Undergraduates may read for a Honours degree in Portuguese alone, or Portuguese may be combined with another modern language, or with history, classics or philosophy. The syllabus is flexible, and allows students to concentrate on Portuguese studies, Brazilian studies or both. The faculty offers no subsidiary or part-time courses - though it is possible for undergraduates reading another language to take one optional paper in Portuguese in Finals - and accordingly there are not so many students reading the subject as there are at some other British universities. Last academic year there were 21 students, however, which is the highest total ever. The fact that Portuguese is taken solely as an Honours subject means that a generally high standard is reached.

Post-graduates may work on Portuguese or Brazilian topics for the two research degrees offered by the University, the D.Phil. and the M.Litt. Those interested in post-graduate work in literature have a third option, the M.Phil., which is a taught course, lasting two years, examined by written papers and a short thesis. Portuguese and Brazilian studies can be offered in the M.Phil. in European Literature and in the M.Phil. in Latin American Studies. Courses are also available for post-graduate students interested in Portuguese anthropology and sociology.

Recent doctoral dissertations in Oxford have included José Cutileiro's anthropological study of a rural community in the Alentejo, a version of which was published by Oxford University Press, and João de Pina-Cabral's anthropology dissertation on a community in Northwestern Portugal. Maria Filomena Mónica's sociology dissertation on education in Salazar's Portugal has been published in Portugal, as have V.Pulido Valente's history dissertation on the early years of the Portuguese Republic, and Graça Almeida Rodrigues' history dissertation on 16th-century Portuguese humanism.

In recent years five post-graduates have successfully completed theses on Portuguese or Brazilian literature, three for the D.Phil. and two for the M.Litt. Some of these have been published, as have two D.Phil. theses submitted to the Faculty of Modern History.

In recent years the Oxford University Press - whose first Portuguese imprint was a translation of the *Book of Common Prayer*, published in 1695 - has re-issued Aubrey F.G. Bell, *Portuguese Literature* (first published in 1922), and has published an

edition of *Os Lusíadas* by Professor Frank Pierce in 1973.

The success of Portuguese at Oxford is largely due to the excellence of Oxford libraries. There are three libraries concerned with the subject. The Spanish and Portuguese section of the Modern Languages Faculty Library (librarian: Mr A.Seldon) is intended especially for undergraduate use. Its Portuguese and Brazilian holdings amount to some 2,500 volumes.

Since the nineteenth century the Taylorian library has been the Oxford library principally responsible for literature in the European languages. It now has about 7,500 books in Portuguese, and is actively buying new material. In addition, the library receives 25 current periodicals in the field, and has long runs of a number of important older journals, including *Panorama* (1837-1858), *Revista Universal Lisbonense* (1841-1857), *Ilustração Luso-Brasileira* (1856-1859), *Revista Lusitana* (1887-1943), *Revista Nova* (1901-1902), *A Águia* (1912-1919), *Afinidades* (1942-1946), *Mundo Literário* (1946-1947) etc.

Although the library was founded only in the last century, it has been able to acquire a number of antiquarian books in Portuguese. There are Portuguese books in the Martin and Finch collections, and the library has also bought a number of early editions, especially of Portuguese poetry. There are early editions of Sá de Miranda, António Ferreira, Diogo Bernardes, Andrade Caminha, Camões, Manuel de Melo, Veiga Tagarro and others. The librarian in charge of Hispanic acquisitions is Mr J.Wainwright, to whom we are grateful for this information.

The third and perhaps the most important holding of Portuguese material in Oxford is to be found in the Bodleian, Oxford's largest library, on which a separate note, by a member of the Library's staff, is appended below.

T.F. EARLE  
HERMINIO MARTINS

#### *Bodleian Portuguese Holdings*

The Bodleian Library has been collecting in the field of Portuguese studies since 1600, when we received by donation - only the second donation after Bodley's re-founding of the Library - some 200 volumes from the collection of Fernão Martins Mascarenhas, bishop of Faro from 1594 to 1618 and later Grand Inquisitor of Portugal. The donation came in fact from the Earl of Essex, who had sacked Faro on his expedition against Cadiz in 1596 and removed the books from the bishop's palace. The collection included 15th- and 16th-century imprints from Lisbon and Coimbra, and a manuscript life of John the Baptist in Portuguese by Antonio Pireira - the first manuscript to enter the Bodleian.

Over the next two hundred years there was no deliberate

attempt to acquire Portuguese material, though many works of Hispanic interest were received by donation. One way and another, the Bodleian built up a collection of early Portuguese literature that should not be underestimated: there are extensive holdings of Camões, for example, with editions of the *Lusiads* going back to 1572. But by the first half of the nineteenth century the Library had fallen into the pattern of collecting works principally on the history, rather than the literature, of the Iberian nations. Bandinel, the Librarian from 1813 to 1860, was active throughout his incumbency in acquiring works on Spanish and Portuguese history, though we have the word of George Ticknor that in 1838 the holdings of Spanish literature were 'miserably deficient'.

After the establishment of the Taylor Institution the Bodleian gradually abandoned any attempt to cover literature in European languages, and in the twentieth century has come to regard itself, insofar as Portuguese studies are concerned, simply as the major Oxford library for history and the social sciences. Our aim nowadays is to collect all the major current publications on the history, geography and social conditions of Portugal, Brazil and such of the former Portuguese colonies as are not covered by dependent libraries. (Rhodes House is responsible for all Bodleian collections on Africa south of the Sahara, and much of the coverage of Goa and Timor is shared with the Indian Institute.) In addition, we are still trying to improve our holdings of earlier, out-of-print materials: our recent acquisition of several contemporary pamphlets on the Miguelite Wars is a case in point. And to keep us up to date, we receive most of the current Luso-Brazilian historical journals, published in Portugal and elsewhere.

R.MCNEILL

## COMMENTARY

### LÉVI-STRAUSS AND THE AUSTRO-ASIATICS: ELEMENTARY STRUCTURES UNDER THE MICROSCOPE

In the thirty-odd years since its original publication in 1949, Lévi-Strauss's *Elementary Structures of Kinship* has been the lodestone for kinship studies - the one work, if any can be so identified, towards which both theoretical and ethnographic analyses have been oriented. These comments are no exception, and perhaps none of them will seem original, save in the present ethnographic context. But there may be some value in showing how much (or little) credibility can be found in Lévi-Strauss's observations concerning just a few ethnic groups, most of which have a great deal in common linguistically and sociologically. This is especially significant, since some of his conclusions concerning them are used to support the general theoretical arguments of *Elementary Structures* in a number of places. To base any judgment as to the soundness of the book as a whole on such a tiny number of its ethnographic sources would however be foolhardy and unfair. And this is not simply a critique: these ethnographic examples in *Elementary Structures* have steadily been superseded by subsequent research, and these comments are intended similarly to bring things up to date.

The ethnic groups concerned are certain of the Austro-Asiatic speakers of eastern India, who form a linguistic minority distinct from speakers of Indo-European, Dravidian, or Tibeto-Burman languages. Most are found in southern Bihar and northern Orissa, on the Chotanagpur plateau, but one group, the Khasi, live in Meghalaya. All can advisedly be classed with the 'tribal', rather than the 'caste' area of the Indian social spectrum.

A first oversight on Lévi-Strauss's part is that he does not seem to recognize or appreciate the ethnic and linguistic autonomy of the Austro-Asiatics, and in two places in *Elementary*

*Structures* he alludes or refers to certain of them, quite erroneously, as Dravidian: the Bhuiya and Munda on pp.425-6; and the Munda, Santal, and Mahali on p.417 (where he is repeating Risley's apparent mistake - 1892: xxxvii).<sup>1</sup> The first of these references occurs in that part of Chapter XXVI (on 'Asymmetric Structures' in India) where he is refuting Rivers' suggestion (1907: 622-3) that cross-cousin marriage among Dravidian groups may be the result of dual organization. It is not this, however, but the author's treatment of the two Austro-Asiatic groups referred to in the course of this refutation that must be questioned. Take first his analysis of the Munda kinship system:

Each Munda village is divided into two groups of *khut*, called *paharkhut* and *mundakhut* respectively. The first provides the religious chief, the second the secular chief: the first is regarded as 'older', the other as 'younger'; the first is regarded as superior, and the other as inferior. Both *khut* of a village belong to the same clan or *kili*, have the same totem, and cannot intermarry. Marriage can only take place between *khut* belonging to different villages and clans, and according to the following rules, viz., if a marriage takes place between two corresponding *khut* of two different villages, other marriages of the same type are sanctioned within the limits of the same generation, but a prohibition ensues for the following generations which lasts as long as the first couples live, and even as long as both *khut* preserve the social relationships derived from the intermarriage. Conversely, if a marriage has taken place between *paharkhut* of one village and *mundakhut* of another, this type of marriage is prohibited in the following generation, while that between two *paharkhut*, and that between two *mundakhut*, are allowed (*Elementary Structures*, p.426).

...the simplest interpretation is that of a system originally based on marriage with the father's sister's daughter.... Here...we are faced with a pseudo-dualism, which in this case masks a system of patrilateral marriage (*Elementary Structures*, p.427).

The *khuts* are in fact lineages, and there are often more than just two in any village, despite the implications of the passage to the contrary. While the two mentioned above are the dominant ones, there are apt to be representatives of other lineages and/or clans, affinal and non-related, present also. Nonetheless, the tendency for villages to be associated mainly with one dominant clan has led to a preference for village exogamy (on

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<sup>1</sup> References to *Elementary Structures* in this article are from the 1969 edition published in Boston.

which Lévi-Strauss's model in part depends), clans being strictly exogamous (Roy 1912: 115 ff., 400; Sachchidananda 1957: 8).

What is at issue is the identification of this system with patrilateral cross-cousin marriage. Certainly the two models have in common a rule of 'delay' in repeating alliances in ensuing generations: but the delay is of a different sort in each case. In the theoretical model of patrilateral alliance it is the reciprocity that is delayed, the return of the woman who is to replace that one taken in the previous generation. There is no question of the complete *prohibition* on re-alliance in the following generation, which is the key feature of the actual Munda system, according to Lévi-Strauss's sources. His interpretation is also countered in two respects by recent ethnographic reports. Firstly, both Das and Choudhury state unequivocally that all cousin marriages are banned among the Munda (A.K.Das 1965: 10,12; Choudhury 1977: 62). Secondly, Yamada (1970) reports that the ban on repeating alliances lasts for longer than a single generation (as the above passage hints at one point). A tendency to remember only one's own clan name reduces the usefulness of the clan system as a regulator of marriage, despite this ostensibly being its chief function. Generally, however, people know the villages of their affines and immediate ancestors, and use this knowledge as a basis for deciding questions of marriageability: it is advisable to marry into a village with which there *has* previously been a marriage, but not for at least three generations (Yamada 1970:263, 384-5). This rule is also reported in Hoffman (1930-1941: 2969, 3827), Lévi-Strauss's original source for the above passage.<sup>2</sup> A similar rule occurs among the related Santal (Bouez 1975: 120). It is, I suggest, how the passage should really be interpreted, i.e. as a means of avoiding marriages with unacceptably close relatives, not as a system of patrilateral alliance. A final but by no means minimal point is that both the feasibility and the existence of prescriptive<sup>3</sup> systems of this

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<sup>2</sup> Though it admittedly does not occur in Koppers (1944), probably his immediate source, due to the scarcity of Hoffman's *Encyclopedia*.

<sup>3</sup> Preferences for marriage with the father's sister's daughter are, of course, another matter. From his Figure 80 (*Elementary Structures*, p.727) it is clear that Lévi-Strauss envisages for the Munda what anyone else would call a prescriptive system. It is true that elsewhere he has dismissed the distinction between the two as merely that between rules and behaviour (1966: 17-18); but Needham has frequently shown the fallacy of this view (1973, etc.), just as Korn has shown the extent of the confusion inherent in Lévi-Strauss's reasoning here (1973: 36-8).

sort have been questioned by Needham (1958; 1962: 106-7, 111 ff.).<sup>4,5</sup>

The second point I should like to raise concerns Lévi-Strauss's interpretation of the Bhuiya term *bandhu*, and the imputation of matrilineal alliance to this group.

...the Bhuiya of Orissa, who practise village exogamy, are divided into *kutumb*, or villages where the members are prohibited spouses, and *bandhu*, villages in which marriage can be contracted. Because the social organization of the Bhuiya is harmonic, viz., patrilineal and patrilocal, and *bandhu* designates maternal relatives, there is a double reason to conclude upon a system of generalized exchange (*Elementary Structures*, p.726).

Thus the term is glossed as i) 'marriageable', and ii) 'maternal relatives', in successive sentences. It is not easy to follow the reasoning here, but the implication is presumably that marriageable categories are also maternal ones, and that this suggests a system of repeated alliances of the sort characteristic of a rule of matrilineal marriage, a conclusion further supported by the 'harmonic' regime.

There are two things to quibble about here. First, it is clear from Roy (1935: 134-5), Lévi-Strauss's source for the above passage, that in the present case only the first interpretation of *bandhu* - 'marriageability' - is the correct one. Lévi-Strauss's view that the term means 'maternal relatives' (*Elementary Structures*, p.411 and n.6; p.426 and n.5) is based on a footnote in Held (1935: 71 n.1) in which the latter is quoting Hocart (1924: 103 n. ). Whatever the justice of this gloss as regards the Mahābhārata and the age of Manu, it is not applicable to the Bhuiya, nor to two related groups where the term also occurs, and in the same sense, the Juang and the Kharia (McDougal 1964: 322-8; Roy and Roy 1937: 188-9).<sup>6</sup> Secondly,

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<sup>4</sup> This observation of Needham's has itself generated some controversy though without really being disestablished.

<sup>5</sup> It should be noted that Lévi-Strauss applies his interpretation of the Munda system to other parts of his overall argument (*Elementary Structures*, pp.199-200, 445, 463). I leave it to others, especially Australian specialists, to comment on any changes in the significance of these parts, in the light of what has been shown here.

<sup>6</sup> Like the complementary term *kutumb* ('agnate', 'tabooed in marriage'), meanings of *bandhu* vary considerably both regionally and diachronically (cf., for example, Inden and Nicholas 1977: 15-17, 117-9, nn.1,4,7,8).

ethnography which should have been available to Lévi-Strauss (and in one case clearly was) undermines his conclusion that the Bhuiya have 'generalized exchange'. While Roy states that marriage with cousins is allowed, he implies, solely on the basis of the terminological equivalence of mother's brother and father's sister's husband, that the system here is, or used to be, one of bilateral or 'restricted' exchange, not unilateral or 'generalized' exchange (1935:142-3). Ray Chowdhury goes even further, in that he regards cross-cousin marriage as disapproved, and not at all mandatory or preferential. The fact that marriage into one's mother's clan ('sept' in the original) is banned within at least three, and sometimes as many as five generations, also reduces the possibility of prescriptive matrilateral alliance existing here (1929:102, 106-7).

This is not the only example of the latter sort of error. Earlier Lévi-Strauss had told us that:

Certain tribes still practising matrilateral marriage justify it by disparaging the maternal lineage as too insignificant to merit a special prohibition. Thus the Santal, who incline towards matrilateral alliances, have the proverb: 'No man heeds a cow-track, or regards his mother's sept' (*Elementary Structures*, p.416).

What Risley, his source, actually says, however, is:

The Santals...are said to make up for their sweeping prohibition on the father's side by allowing very near alliances on the mother's side - a fact pointedly exemplified in their proverb...(1892: xlix).

The entire section of which this latter passage is a part is, as Lévi-Strauss realizes, a discussion of *sapinda* rules, and especially the feature widely found in North Indian tribal and caste society whereby the number of generations excluded in marriage is fewer on the maternal side than on the paternal side (*ibid.*:xlix-1), i.e. it deals with the extent of prohibitions, not with the existence of preferences, as one is apt to infer from Lévi-Strauss's words. What is more, the extensive literature on the Santal, admittedly mostly quite recent, shows not only disapproval of cross-cousin marriage, but also restrictions on marriage with maternal kin, contrary to the statements of both Lévi-Strauss and Risley.

Another point of difficulty concerns the Khasi. Lévi-Strauss adduces this group in support of his theory, reasonable enough in itself,<sup>7</sup> that matrilateral alliances do not depend on a particular mode of unilineal descent (*Elementary Structures*, pp.273-4). He chooses them under the belief that while the Khasi proper are

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<sup>7</sup> Cf., for example, Needham 1961; 1962: 64; 1964:237; etc.

firmly matrilineal, one of their sub-groups, the War, are apparently patrilineal. His evidence for this is the supposed existence among both groups of the *seng*, a descent group that is matrilineal in the case of the Khasi, but patrilineal (because of its descent from three *male* mythical ancestors) among the War. The mistakes occurring here were copied from Hodson (1925: 163-4), and are refuted by both recent and previously available ethnography. In fact the *seng* only appears among the War, and as a non-exogamous, cognatic, land-holding descent group; i.e. it is not in either sense unilineal, nor is it involved in the regulation of any sort of alliance system. Moreover, descent *per se* among the War is no less matrilineal than among any other Khasi sub-group (Das Gupta 1961: 152; 1968: 135). As for the Khasi '*seng*', this seems to be something quite different: *ka iing*<sup>8</sup> *seng*, merely a synonym for *ka iing khadduh*, the household of the senior maternal uncle of the matrilineal descent group, and headquarters of its ritual activities (Gurdon 1914: 88). Finally, the restriction against marrying one's mother's brother's daughter in the lifetime of her father (*ibid.*: 78) tells against the Khasi having any prescriptive marriage rules or systems, yet again contrary to Lévi-Strauss's assumptions. Cousin marriage is again not particularly liked here (Roy 1938: 130), probably due at least in part to the fact that over fifty per cent of Khasi are Christian.

Last but not least, we must register the fact that none of the literature on any of the Austro-Asiatic groups with which Lévi-Strauss deals - especially that available to him before the late 1940s - offers any clear evidence of present-day prescriptive marriage rules or systems, nor much on preferences for any particular category of relative as spouse.<sup>9</sup> Yet these are precisely the topics<sup>10</sup> of *Elementary Structures*, and the choice of these groups as examples intended to support its overall argument is unfortunate, since they do not fit his requirements.

Some further remarks can be devoted not to his use of sources, but to his choice of them. Thus his source for saying that the Kharia practise cross-cousin marriage is not the first-hand ethnography of Roy and Roy (1937) or Das (1931), but - of

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<sup>8</sup> Not *iing*, as in the English translation (*Elementary Structures*, p.274).

<sup>9</sup> Some of the Austro-Asiatic groups with which he does *not* deal are another matter, however. Also, cross-cousin marriage may be allowed in those related groups he does use, as among the Kharia, for example (Roy and Roy 1937: 223); but such non-regularized behaviour will hardly have the systemic significance in which Lévi-Strauss is interested.

<sup>10</sup> Speaking advisedly, that is: we have already alluded to Lévi-Strauss's unfortunate attitude to the relationship between prescription and preference (see note 2 above).

all people - Frazer (1919: 126), whose own source, Russell and Hira Lal (1916: 447-8) is not even credited. Even in 1949, when *Elementary Structures* was first published, Risley's work of 1891-2, a gazeteer-cum-encyclopedia rather than a proper ethnography as such, was well out of date, and hardly to be compared with Biswas (1935) or Roy (1912). Lévi-Strauss also places as much weight on Koppers (1944), a secondary source, as on the first-hand reports of Roy (1935) and Hoffman (1930-1941), though the latter is admittedly quite rare; and as much on Hodson (1925) as on Gurdon (1914), on whose original monograph the former based part of his article.

I am in no position to say how far this often cursory misreading and misinterpretation of out-of-date and ill-researched sources is typical of *Elementary Structures* as a whole, nor, in fairness, to what extent it has generally been overtaken by subsequent ethnography. I can only say that the few items it has of interest to my own research into Austro-Asiatic kinship have not improved my confidence in the veracity of its results or the validity of its author's methods; and that they have led me to believe that the monumental and ambitious task of producing an adequate theory of such kinship systems, well-substantiated by good-quality ethnographic examples, still awaits its redactor.

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## INDIGENOUS REPRESENTATIONS OF EMOTIONS:

### THE CHEWONG

Progress has been slow in the cross-cultural study of emotions. From the scientific viewpoint - examining what is 'really' happening in emotional life - it is not clear whether there are universal emotions of a distinct kind, or what should be included in this category. From the indigenous viewpoint - examining how members of societies comprehend the emotions - it is not clear what significance should be attached to the multitude of ways in which the emotions are understood and handled, or how these differences should be explained. And perhaps least progress of all has been made in integrating the scientific and indigenous approaches, to establish, for example, the role played by indigenous formulations in organising and managing emotional life.

Research in Western laboratories by experimental psychologists suggests that the nature of emotions and how they are managed is to a considerable extent *constituted* and regulated by cultural factors. This means that ethnographic investigation of what is probably the most relevant cultural factor, the conventions of emotion-talk, is of some consequence. Investigation of this variety should help elicit the broader characteristics of emotional life, and so pertain to the Lévy-Bruhlian issue of whether or not the idea of psychic unity applies to the emotions.

The first step towards the goal of establishing whether or not certain emotions are culture-specific thus involves obtaining as good an idea as possible of the nature of how emotions are represented. In this paper I concentrate on the representations of one of the handful of preliterate cultures whose psychologically significant language has been well-recorded, namely the Chewong of the Malaysian interior. My intention is to discuss what is involved in characterizing Chewong 'emotion'-talk, and then to indicate how emic findings can help illuminate the nature of

Chewong emotional life. This culture is particularly interesting, I suggest, in that the emotions are largely ignored. Signe Howell, the ethnographer on whom I rely, has arrived at a somewhat different interpretation. Having worked closely together, we have decided that she will reply to this paper: interpretation is so difficult that it seems best for me to concentrate on the 'ignoring emotions' theme and for her to elaborate on the theme that the Chewong 'acknowledge' (Howell 1981:143) psychological states.

### *Characterization*

Chewong or any other ethnography of 'emotion'-talk can be characterized in terms of the following options:

First, and most basically, whether what appears to be emotion-talk is actually about the emotions. Rodney Needham suggests that

it may be that terms for inner states are social more than they are experiential; in other words, that they are useful for the ascription of virtues and demerits to the characters of other individuals rather than as socially contrived instruments by which individuals are enabled to assess their own inner experiences (1981:77).

In deciding what counts as emotion-talk, I include *whatever* means are used for talking about the emotions *so long as* these means are understood, *by participants*, to function (whatever other functions they might have) as ways of discriminating between emotions as *inner experiences*. I take it for granted that emotions are essentially inner experiences.<sup>1</sup> They cannot be reduced to physiological arousal or behaviour (the former being too non-specific, the latter not necessarily occurring). But because emotions are bound up with physiological arousal, and generally involve behaviour, it is possible to infer emotion-talk when it does not operate. As we shall see, 'emotional' behaviour need not imply acknowledgment of emotions. My rule is that representations which appear to us in the West as being about emotions (such as talk of particular forms of behaviour which we associate with anger) do not qualify as emotion-talk in other cultures unless there is evidence that inner experiences are brought to mind.

Second, what is the degree of elaboration of emotion terms? That is, how many emotions are identified?

Third, we have to ascertain how emotions are classified,

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<sup>1</sup> For discussion of the fundamental issues involved in using terms such as 'emotion' and 'behaviour' in comparative enquiry, see Ardener (1973). An important consideration is that the meaning of such indigenous terms has changed with time. Is our objectivity merely a historical illusion?

including psycho-moral considerations to do with how they are evaluated. Which core (positive/negative) emotions are emphasized, and which configurations appear? Is a culture more Apollonian or more Dionysian in orientation?

Fourth, there is the question of which vehicle or mode is used to talk about what could be emotions. Moving from the internal to the external, and illustrating from our own culture, vehicles include direct reference to emotions as inner experiences ('I feel angry'; 'I hide my fear'), use of physiological phenomena ('I tingled with fear'), use of bodily parts including organs ('I vented my spleen'), use of behavioural manifestations ('her smile said it all'), appeal to context of arousal ('you can imagine how I felt when the car skidded'; 'that was joy-making'), and finally use of metaphors and other figures of speech ('love is like a red red rose').

Fifth, attention is directed to the locus of emotions. Again moving from the internal to the external, emotions can be seated in mentalistic, physiological, biological and behavioural domains, or in external agencies and phenomena. Questions of *locus* differ from questions of mode in that whereas any mode can plausibly be held to refer to emotions (defined as inner experiences), this is less likely with regard to *loci*. One would have to demonstrate, for example, that 'emotions' grounded in an organ, say the liver, are grounded in an organ which, according to participants, can experience inner states. Or again, 'the wind is violent' is a case of emotion-talk only if it is held that the *wind* can be angry.

Sixth, what are the contexts of use and the functions of emotion-talk? Options here include use to explain motivation (whether of normal or abnormal behaviour), use in a more exploratory or existential fashion (to make sense of and report to others what is going on in the self), use in remedial activities (as in the discourse of psychotherapy), uses in socialization, the encouragement of good behaviour ('you do not want to feel guilty, do you?'), and in general to manage the self, and use in wholly conventional fashion (as when one feels obliged to say that one was happy at a party). When conventions are involved, it will be noted, 'emotion' terms need not refer to emotions: 'I fear it will not rain', or ethnographic instances of obligatory 'emotional' expressions during rituals.

Finally, we have to establish the nature of emotional display. What are the rules governing display, and what are the consequences? (The latter include punishment, feelings of release, and bringing harm to others.)

Deciding between these possibilities raises awkward problems of interpretation. Bearing in mind Needham's point that 'we tend to interpret exotic psychological observations as the varied expressions of universal inner states...' (1981:76), care is required if we are to characterize accurately alien representations. Degree of elaboration, for example, appears to be an easy matter to settle: one simply counts emotion terms. But that presupposes establishing which terms are understood to refer to emotions. Or

again, one has to decide whether talk in a particular mode, say behaviour, has more to do with behaviour than with what behaviour might imply, at the emotional level, for us in the West. Such problems are discussed in the following examination of Chewong indigenous psychology.

### *Chewong 'Emotion'-Talk*

Among the Chewong 'emotion' terms are poorly elaborated; 'emotions' are in the main viewed negatively; the mode of 'emotion'-talk is organic and the locus is ultimately behaviour; the functions of 'emotion'-talk are to explain and handle behaviour; and many 'rules' function to hold potentially dangerous 'emotional' display firmly in check.

I argue that what appears to be emotion-talk is best characterized in another fashion. This leads to my conclusion that, in the etic frame of reference, the Chewong representations function to dampen and channel emotional arousal.

On any standard, Chewong do not explicitly acknowledge many emotions. Howell's 'vocabulary of inner states' contains only a handful of candidates for registration as emotion words: *chan* ('angry'), *hentugn* ('fearful', 'frightened'), *punmen* ('like [something]'), *meseq* ('jealous'), *lidva* ('ashamed, shy'), *hanrodn* ('proud'), *imeh* ('want'), and *lon* ('want very much') (1981:134). None of the Malay or Malay-derived terms given in this vocabulary list have to do with emotions, although Howell elsewhere refers to *sedap* ('pleasant', 'nice', 'agreeable', 'fine' (1980:207,240)). Malays, it is interesting to note, consider two hundred and thirty or so terms to refer to emotion states (see Boucher 1979:170).

That the Chewong have such an impoverished vocabulary of the 'emotions' clearly suggests that they are not much concerned with the intricacies of emotional life. However, emotions can be talked about in ways other than by use of what I have called 'direct' emotion terms. Together with words which might function as bodily expressions of emotions, for example *abud* ('hot [body]') and *sedeig* ('cool [body]') (Howell 1981:134), there remains the possibility that Chewong employ behavioural, contextual and more metaphorical idioms in much the same way as we do.<sup>2</sup>

As another step, is it possible to argue that the Chewong lack the linguistic means to talk of the emotions as inner experiences? The liver (*rus*)<sup>3</sup> provides the mode and, to some

<sup>2</sup> This possibility is ruled out for bodily expressions ('the lack of...a language of bodily expressions'; Howell 1981:135), and Howell has told me that Chewong never speak of the liver as hot, etc. However, 'homesickness' is spoken of in terms of *moni* ('smell') (Howell 1980:204).

<sup>3</sup> Liver-talk is common in South East Asian societies, as well as elsewhere (see e.g. Burton 1972:150 orig. 1621).

extent, the locus of 'emotion'-talk: and there does not appear to be evidence that what the liver is used to convey has anything to do with inner states. In the absence of evidence it is appropriate and parsimonious to assume that what it refers to should be sought elsewhere - a consideration returned to once I have discussed what liver-talk does not do.

Howell observes that,

The liver, *rus*..., is the seat of both what we call 'thoughts' and 'feelings', and they do not make any conceptual distinction between the two. In fact they have no word for 'think' or 'feel'. Whenever they do express verbally emotional and mental states and changes, this is done through the medium of the liver. Thus they may say, 'my liver is good' (I am feeling fine), or 'my liver was tiny' (I was very ashamed), or 'my liver forgot' (1981:139).

She comes to the conclusion that 'in their concept of the liver as the seat of all consciousness they have a means, albeit a limited one, for describing their inner states' (*ibid.*: 143). As Howell elsewhere writes, *rus* 'has super-physiological attributes, being the seat of individual consciousness, and the medium via which emotional and mental states are expressed' (1980:208).

I am not convinced that liver-talk performs this descriptive function. First, why should 'my liver is good' be glossed as 'I am *feeling* fine' when the Chewong have no word for 'feel'? Does not this linguistic incapacity suggest that 'my liver is good' is a perfectly adequate translation, not standing in need of further interpretation? 'Good' and other such terms, as I hope to show later, make more sense if they are regarded in terms of appropriate (or deviant) behaviour. Second, states of the liver, an organ which most people do not experience, provides a curious idiom for talking about inner experiences. If the Chewong were to use the vehicles of 'heart' or 'bowels' (cf. Onians 1973), one would be more inclined to accept that inner experiences were implied: these organs are physiologically significant. That they use the liver idiom, which is not experienced physiologically and so does not enter into emotions, surely implies a high degree of dissociation from the emotions. Third, it is not as though the liver provides a metaphorical way of talking about emotions which can be otherwise conceptualised as inner states (as when we are able to unpack 'I vented my spleen' as 'I released anger'). For, as Howell affirms, 'psychological states are *only* expressed via the medium of the liver...' (1981:139, my emphasis). The dissociation is complete. And fourth, why should the Chewong want to talk of inner states - excepting, according to Howell (*ibid.*: 141), 'fear' and 'shyness' - when, as the ethnographer emphasises, Chewong culture adopts the philosophy of suppressing the self (*ibid.*:141)? At least, that is, to the extent of not articulating inner states. Given the fact that 'thoughts' and 'feelings' are not distinguished, what applies to the former presumably applies

to the latter: and in the former 'thinking' ('saying it in the liver', *bad lam rus*) and expression of thoughts is not distinguished (1980:260).

To repeat an important point: representations are only about the emotions if culture provides ways of talking about the emotions as inner experiences. From the etic point of view, liver-talk *could* be 'about' emotions (a consideration returned to later). But we are not at present concerned with this framework; our concern is to characterize the emic perspective. And there are as yet no clear signs that this framework supports interpretation in terms of inner experiences.

Significantly, the Chewong can speak of the 'true person', an inner person in that the *ruway* lies within the body (*bajo ruway*, the *ruway*'s 'cloak'), and a 'personage' which 'may be translated as consciousness' (Howell 1981:138; 1980:179). Although such a conceptual vehicle could be used to talk of the emotions as states of consciousness, *ruway* is ignored: 'Disease is defined as loss of *ruway*, but psychological states are...never [expressed] by reference to the *ruway*' (1981:139).<sup>4</sup>

That 'psychological' states are 'only' expressed via the *rus* - which, as should be apparent, is much less suitable than *ruway* for expressing inner experiences - supports my contention that liver-talk is not about emotions. The inner self does not appear to exist in so far as the emotions are concerned. As I shall attempt to argue, after paving the way by introducing Chewong 'rules', this interpretation receives broader support in that rules encourage the dampening of emotional arousal: in particular by discouraging expression and examination of anti-social and distressful 'emotions', and in general by directing Chewong attention to a behavioural rather than to a mentalistic locus.

#### *Rules and Context*

Rules are of fundamental importance to the Chewong. Their everyday life is 'largely structured by the all-pervasive presence of them' (Howell 1981:135). To illustrate how they operate, *tola* means that if somebody shows disrespect towards certain categories of affines by behaving too openly, the individual becomes ill; *maro* entails that if somebody is 'stingy', not offering food to a visitor, the individual suffers dizziness; *punen* (of seven varieties) can mean the connexion between 'speaking badly' (either speaking of an anticipated feast, etc., or exclaiming when an accident occurs) and suffering; *mali* has to do with the unpleasant consequences of whistling or swinging legs (etc.) in an extravagant manner; *taladn* operates in similar fashion, but when somebody

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<sup>4</sup> Which makes one wonder why Howell translates *ruway* as 'consciousness' - all the more so in that the *rus*, as we have seen, is 'the seat of all consciousness'.

laughs at animals; Tanko, a supernatural being, punishes those who transgress rules governing sexual intercourse; and *pantang* rules 'specify restrictions on behaviour once events (major life-crises) have occurred which are outside the control of man' (*ibid.*:137; see also 1980:241-280).

Other than *pantang*, which is protective in function, rules are preventive. They 'indicate the sort of *behaviour* which must not be indulged in lest specific consequences come about' (1981: 137, my emphasis). What all the rules have in common is that deviant behaviour results in punishment. They all bear on behaviour, whether it be showing disrespect, not sharing food, acting extravagantly (swinging legs), having illicit sex, or, in the more general sense of behaviour as 'Manner of conducting oneself' (*Shorter Oxford English Dictionary*), laughing, crying, shouting, 'speaking badly', and whistling.

Rules focus on external activities, on the individual as publicly envisaged and as socially accountable. Chewong rules remind us of Skinner-derived behavioural therapy. The 'black box' is left intact in the understanding and management of anti-social behaviour. Concerning the activation of rules, *tola* applies when someone shows disrespect (it is not couched in terms of being too intimate); *maro* operates if one does not give food (it is not attributed to inner desires to keep for oneself); one type of *punen* is called into action if one behaves badly in connexion with accidents; and *mali* operates because somebody whistles (not, apparently, because that person is, in our terms, happy). As for the consequences of rule-infringement, attention is again directed away from emotions:

...rather than *describe* their thoughts and feelings as we would at such times, they describe the cause of the disease or mishap, i.e. they refer to the particular rule that has been broken.... (Howell 1981:140, my emphasis).

We in the West find it most natural to introduce emotions when the rational, socialized, self goes awry to break rules and conventions. How can the irrational be explained by reference to the rational? As the philosopher Richard Peters puts it, 'the phrases in which the term "emotion" and its derivatives are not only natural but almost indispensable are when we speak of judgments being disturbed, clouded, or warped by emotion....' (1962: 119).<sup>5</sup> As well as using emotion-talk to explain deviation, we also use it when suffering from the consequences of rule-trans-

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<sup>5</sup> See also Coulter: 'It is most routinely the case that emotion-concepts function in accounts that explain action that is in some way considered untoward or problematic within a situation, or that explain the absence of some otherwise obligatory or preferred course of action' (1979:132).

gression (think of 'guilt' and 'shame'). That the Chewong appear to resist these apparently natural tendencies, rules being called into play by anti-social behaviour and resulting in punishment and the description of the particular rule that has been broken, shows the extent to which Chewong modes of thought are orientated away from the emotions.

Whilst Howell in the main emphasizes the behavioural nature of rules (they 'prohibit most kinds of extravagant behaviour'; they 'inform the individual how to conduct himself or herself' [1980:241-2]), she also argues that they function to acknowledge emotions implicitly. We are told, for example, that,

...the sorts of things that are forbidden by the rules are such as *we would regard* as expressions of feelings. One must never laugh uproariously, cry, shout, whistle or behave extravagantly in any manner. By just invoking the word for the rule/repercussion a whole gamut of emotions is thus implicitly referred to (1981:141; my emphasis).

And on occasion Howell is prepared to go further, writing that 'rules provide them with an idiom for *explaining* their inner states whenever these change at times of stress - such as during disease or "bad luck"...' (*ibid.*:140, my emphasis).

What encourages Howell to interpret rules in terms of their being called into operation by and acknowledging inner states? One reason is that it is difficult for a Westerner not to assume that crying, laughing, etc., register acknowledged inner states. A related reason is that 'the nature and indeed...very existence [of rules] demonstrates that emotions, far from being foreign to them, are of such importance as to require close control' (*ibid.*: 135; cf. 'That they experience them or at any rate are aware of their existence, must be assumed because of their being specifically forbidden' [1980:285]).

Postponing for the moment further discussion of these incitements, the fact remains that there is no good ethnographic evidence to suggest that it is emotions, rather than behaviour, which are specifically forbidden. I agree with Howell that rules are about behaviour; that they provide 'an *externalized* idiom for... controlling and suppressing the self'; that they 'constitute the dominant restraint upon Chewong behaviour' (1981:142 my emphasis; 1980:241). I do not agree that rules should be seen as involving 'control of the emotions' (1980:242). One kind of *punen*, for example, is supposedly instigated when someone 'explicitly wants' something; it is better understood, as Howell also indicates, in terms of the principle that 'not to share is anti-social' - all the more so in that *punen* can operate in the absence of desire (1981:136; 1980:252). It is significant that the ethnographer herself appears to have reservations about introducing emotions. Thus when she observes, in an extract already given, that the things which are forbidden by rules 'are such as we would regard as expressions of feelings', the implication is that Chewong do not regard deviant behaviour in the same light.

*Liver and Rules*

My earlier interpretation, that liver-talk does not appear to be about emotions, can now be supported. The two idioms (rules and liver) are closely related. Given that the Chewong have without a shadow of doubt been taught not to display whatever 'emotions' they might have (excepting 'fear' and 'shyness'), it is hardly feasible that they should want to acknowledge that they break rules because they have fallen foul of anti-social emotions. But participants require ways of relating rule transgression to what is occurring in their lives. Bearing in mind the behavioural nature of rules, and bearing in mind that the liver does not seem to provide an emotionalistic idiom, it appears that liver-talk should be understood as performing this 'relating' function.

First, liver-talk provides an account of the behaviours which initiate rule sequences. Thus the entire *maro* sequence runs: a visitor from another settlement comes to one's house; he is not given food; one's liver is *kenjed* (glossed as 'stingy' and *simply* meaning that one is behaving anti-socially);<sup>6</sup> *maro* is activated; one is punished (1981:135). Second, and this is an associated function, the liver is seen as providing rule-enforcing agencies with the information they require. Thus 'when someone breaks a certain rule the retribution of which is an attack by a tiger, the tiger sees the offence in its liver, *endagn lam rus*, and hence knows who and where to attack' (1980:207). It is also probable that liver-talk provides a way of praising culturally valued behaviour, so encouraging conformity (as in 'his liver is good').

When Howell translates liver-talk into mentalistic terms ('angry', 'jealous', etc., of her vocabulary of inner states), she presumably has relied on contextual evidence. 'His liver is tiny' has to do with 'shame' because the person in question has been caught out in public. I have argued that there is no reason to regard liver-talk as being about anything more than what is occurring in the public world of social activity. What so readily appears to be discourse of inner experiences is actually monitoring and regulating social behaviour. What matters is the social, not the experiential; is behaving appropriately, not attending to (controlling, satisfying, exploring, communicating) the 'emotions' themselves.

Although it is extraordinarily difficult to establish the extent to which the Chewong (or any other people) acknowledge the emotions, Howell's ethnographic material is sufficiently detailed for me to be able to argue that the Chewong are more behaviouristic than she on occasion suggests. There is little or no

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<sup>6</sup> And perhaps alluding to those prior social events which explain why one is stingy, and to the fact that one is stingy in the sense of having this personality trait (one tends to behave in stingy fashion). I should point out that deviant behaviour can also be attributed to the heat of human blood and bodies (Howell, 1980:228).

evidence that they *know* emotions, let alone knowing differences between, for example, 'anger' and 'irritation'. The Chewong do not pay conceptual attention to the emotions (excepting, according to Howell, 'fear' and 'shyness'). Certainly the Chewong stand in stark contrast to societies such as the Taita of Kenya where the 'black box' is broken into, where anti-social behaviour is attributed to 'anger' and then managed by a form of 'psychotherapy' (see Harris 1978 ; for another example of a mentalistic traditional society, see Briggs 1970). That Chewong do not associate 'emotions' with the inner conscious self, do not talk of 'emotions' when they suffer, do not appear to manage 'emotions' by means of indigenous psychotherapies, and so on, together with the facts that behaviour and *morality* are emphasised (recall *tola* [disrespect], or 'my liver is good' and see Howell 1980:52), provide good reasons for concluding that 'emotion'-talk is used in and *belongs to* social contexts.

### *The Etic Perspective: Some Psychological Considerations*

The Chewong jolt our view of ourselves. Most of us know only too well what it is to be ravaged with jealousy, blinded by rage, or paralysed by fear. Seemingly driven to regard the emotions as inner states, the significance of many of our activities is bound up with how we consider that our emotions should be managed in the best possible manner (Should I postpone getting married until I am sure of my feelings? What shall I do to relax tonight? How can I get rid of my anger?). And we tend to feel that the better we understand our emotions, the better we can manage or come to terms with them (this is most clearly exemplified by the psychotherapeutic tradition). How then is it possible for the Chewong not to register what we take to be so self-evident, namely the existence of a wide range of emotional experiences? How do they explain deviant behaviour, and how is it possible for Chewong to handle their emotions when they do not know what they are handling?

A radical answer to these questions would be to argue that emotional experiences, as we understand them, are absent. So I ask: what are the *actual* psychological consequences of Chewong indigenous psychology? More exactly, what are the psychological consequences of not attributing deviant behaviour to emotions, of not conceptualizing suffering in terms of emotions, and in general of not representing the emotions?

Explanation has to be of what is the case: that, 'One of the most striking features of Chewong life is the lack of emotional displays among adults' (Howell 1980:54). This consequence, however, can be explained in three ways from the etic viewpoint. At one pole there is Howell's 'suppression' argument, that 'most emotions commonly acknowledged in the West are suppressed by the Chewong' (1980:285). Chewong experience much the same emotions as we do, but cultural punishment models or rules prevent their display (Chewong legends spell out the consequences of rule-

breaking; 'Feelings such as envy, desire, hostility and so on, that might otherwise be provoked at various times are therefore avoided...' (*ibid.*:141). At the other pole there is the theory which holds that Chewong culture does not allow for the constitution of the great majority of emotions. Accordingly, Chewong emotional experience is very different from our own. And between these two poles is the theory, which I adopt, namely that Chewong experience certain emotions in ways approximating to how we do but in greatly dampened fashion.

Concerning the first approach, Howell points out, in connexion with the activation of rules, that 'the type of situations singled out as most likely to provoke emotional responses are familiar to the Westerner' (1981:141; *cf.* '...a close examination of the sorts of behaviour prohibited in the rules, reveals remarkable emphasis upon the control of emotions' [1980:242]). For reasons already discussed, I do not think that these considerations permit us to follow Howell's mentalistic interpretation of 'emotion'-talk. But there is a considerable amount of evidence from experimental psychology that certain situations are likely to result in emotional arousal and that certain forms of behaviour are likely to be bound up with these perturbations (see Berkowitz 1971; Ekman 1977; Leventhal 1980).

If indeed Chewong experience 'anger', etc., should we not agree with Howell's claim that rules function to prevent the expression of emotions? This claim is weakened in that preventing the expression of aroused emotions is almost certain to result in cathartic display (see Heelas, in press). So far as I can establish, Chewong culture does not provide for such display. More fundamentally, the suppression argument is rendered redundant if Schachter's theory (pole two) is valid. *Suppression* gives way to *denial* and *redirection*. Instead of rules implicitly acknowledging emotions by controlling them, rules and liver-talk ignore emotions and so prevent them from being constituted.

The work of the psychologist Schachter (1971) and others shows that emotions to a large extent depend upon cognitive or cultural models, and that in the absence of such models physiological arousal (however initiated) results only in 'as if' feelings (1971:3).<sup>7</sup> Chewong culture, I have argued, does not provide models for the great majority of emotions: which means, according to Schachter's theory, that these emotions are absent. Howell, it is interesting to note, moves some way towards this position:

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<sup>7</sup> The position about to be outlined is also supported by the research of London and Nisbett (1974), for example, and by social learning theorists (see Bandura 1977). See also Peters 1974:402; Hampshire 1959:13; Mischel 1977:21. Anthropologists who have adopted this position include Geertz (1966) and Ortner (1978).

Where we differ is that whereas we examine and talk about these [emotional responses], the Chewong play them down. The rules discourage discussion and *differentiation* of emotions (1981:141; my emphasis).

Robarchek's explanation of Semai rules (very similar to those of Chewong culture) is of a fully-fledged Schachterian order. *Pehunan*, for example, is taken to involve 'frustration', a much less specific or cognitively guided state than a particular emotion (1978:767; 1979). Many psychologists have argued that frustration results in anger. That this does not occur in the Semai setting (where anger is tabooed) is because frustration can in fact be guided into other emotions. The *pehunan* rule provides a fear model, a model which goes deeper than simply preventing angry display (by pointing to those punishments which follow rule-transgressions) in that it transforms potential anger into real fear. In Robarchek's words, 'in those instances where frustration does occur, the resultant emotion in the frustrated party is not anger but is rather fear of the danger to which he has become vulnerable' (*ibid.*:769).

This kind of explanation is plausible. It derives from reasonably well-established psychological theory, and it suits the ethnographic facts. Thus it suits the emphasis on fear, explains why there are no signs of the Chewong and Semai suffering from repression/suppression, and explains why at least the Chewong are not motivated to engage in behaviours which are not forbidden by rules (they do not kill because they do not feel angry). What is particularly interesting is that the Semai (and the Chewong) have themselves arrived at an implicit formulation of Schachter's theory: to speak of a desire (read frustration) 'makes it explicit and increases the speaker's vulnerability of *pehunan* if the desire is not fulfilled' (Robarchek 1978:768). In emic terms, to talk is to increase vulnerability. In etic terms, it is to lapse into using explicit models, and so help constitute what should be denied.

Schachter's theory makes it very unlikely that rules simply work at the level of preventing emotional display. But the evidence cited earlier also makes it unlikely that emotions are entirely absent in the absence of appropriate 'models for'. Hence my adoption of a middle position: putting it crudely, Chewong biology ensures that they experience something approaching what we in the West experience; Chewong culture ensures that their experiences are considerably dampened.

Coulter is close, although rather Schachterian, to this position:

*A sensation or feeling-state* could arise and be avowed intelligibly in ways that are unoccasioned by the social and historical circumstances of a meaningful environment whereas, by contrast, the appropriate application of *affect-concepts* to describe someone's state depends upon specific arrays of meaningful circumstances. What

distinguishes grief from remorse and disappointment from shame is not a determinate inner feeling but responses, actions, appraisals and situations in the social world (1979:127).

On the one hand biology provides sensations, feeling-states, experience of frustration, or what Leventhal calls 'primary emotional experiences' (i.e. '...emotions of anger, grief, disgust, fear, shame, joy, interest, should retain a common *core* in experience as they are based on innate motor scripts' (1980:192; my emphasis). On the other hand culture provides - or does not provide - the means whereby these basic and ill-defined experiences are more refined, differentiated, and filled-out.<sup>8</sup> Fully-fledged emotional experiences 'are not mere eruptions independent of appraisals and judgments, beliefs and conceptualizations' (Coulter 1979:131), but the eruptions are significant enough for us to hold that the Chewong experience what they do not conceptualize.

### *Conclusion*

As well as discussing what is involved in interpreting the 'emotion'-talk of another culture, I have attempted to address the issue of psychic unity. In the absence of hard data (such as a psychologist *might* be able to provide if he went into the field and, for example, tried to collect physiological data), it seems reasonable to suppose that the Chewong experience dampened versions of a number of emotions,<sup>9</sup> but are unlikely to experience those more meaning-dependent and infused emotions of the variety

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<sup>8</sup> The position I have adopted relies on the validity of the distinction between 'feeling-states' etc., and emotions themselves. Running between nature and culture, the most likely hypothesis is that there is a spectrum between the biological and the meaning-dependent: a spectrum whose poles have been characterized in terms of such contrasts as 'primary' and 'enriched' (Leventhal 1980:192), 'physiological arousal' and 'emotion' (Schachter 1971), and 'somatic experience' and 'cognitive experience' (Leff 1977: 324). Evidence supporting the validity of such contrasts (and by implication the spectrum hypothesis), but not their exact form (recall that Schachter and Leventhal do not agree), is provided by experiments showing that frustration can result in various emotions (Berkowitz 1980:345-6) and by the fact that psychologists working with various theories have found it necessary to draw broadly similar distinctions.

<sup>9</sup> Perhaps of the kind mentioned by Leventhal (1980); cf. Ekman's list - the 'emotions' of happiness, surprise, fear, anger, disgust, sadness, and, probably, interest and shame (1977:72).

mentioned by Coulter. It would be rash to deny basic (biological) psychic unity; it would be only marginally less rash to deny psychic disunity with regard to the role exercised by meanings in the education of emotional life. Emotions exist between what is constant (biology) and what is variable (culture). They are not simply endogenous (given by nature) and they are not simply exogenous (given by culture).

The Chewong live *in* their 'behavioural environment' (Hallowell 1971:87), an environment which includes rules and associated superhuman agencies. These function to minimize frustration ('desires' must be gratified), and to channel emotional arousal away from anti-social display (negative and positive learning models make it less likely that Chewong experience anything approaching our 'anger', more likely that they experience something akin to our 'fear'). Dampening and channelling is further facilitated by general lack of acknowledgment of the emotions. In the words of Robert Burton, in *The Anatomy of Melancholy*,

...so we, as long as we are ruled by reason [read rules], correct our inordinate appetite [read satisfy 'desires'], and conform ourselves to God's word [read supernatural agencies], are as so many saints : but if we give reins to lust, anger, ambition, pride, and follow our own ways, we degenerate into beasts, transform ourselves... provoke God to anger, and heap upon us this of melancholy, and all kinds of incurable diseases, as a just and deserved punishment of our sins (1972:137).

I earlier raised a number of questions, to do with how the Chewong manage to virtually ignore the emotions. Needham asks us to consider,

...the imaginary example of an encounter with humanoids from outer space. We should not presume that such beings possessed any of the inner states that we attribute to human beings, and we should have no real need to do so in order to describe their behaviour or gauge their capacities or predict their future actions (1981:73).

The Chewong are Ryleians, not aliens. But they demonstrate that it is possible to organize social and individual life with minimal reference to emotions. In particular they show that it is not necessary to enter the 'black box' in order to manage the emotions. Instead of handling anti-social emotions in terms of indigenous techniques involving what we would call psychotherapy, Chewong culture facilitates behaviour therapy (or, more exactly, therapy as envisaged by such social learning theorists as Bandura [1977]). Providing a naturalistic 'experiment', Chewong non-aggression (Howell 1980:50-56) and generally low-keyed emotional life shows the success of their management strategy.

And given the dampening of emotional life, it is easy to understand why there is no 'need' for participants to struggle to

develop emotion-language. Furthermore, whether it be socialization, explaining deviant behaviour, or handling suffering, Chewong have an effective idiom: children are told to share their food by adults saying *punen*; deviant behaviour is attributed to liver-states which in turn imply, it seems, personality traits and perhaps prior social experiences; and suffering is conceptualized in terms of rule-transgression (see Howell 1980:244; 1981:141; for details of socialization). Finally, socially acceptable behaviour is also encouraged without requiring emotional encouragement (as when we say, 'it will stop you feeling guilty'): 'When offering food to a guest, he will be urged to eat by the host's exclamations of *maro!*' (*ibid.*:141). Emotion-talk is not needed when a society focuses on externals, on behaviour and the external agencies which operate the rules.

It will be apparent that I have become increasingly speculative, in particular with regard to the psychic unity issue. I have relied on the approach of combining experimental evidence and ethnographic conditions.<sup>10</sup> On the experimental side, there is doubt as to whether explicit emotion terms are necessary to constitute appropriate emotions. As Leff, in discussing Schachter, puts it: 'A problematical question is whether a particular experience is available to someone who does not have the word to define it' (1973:300). Less direct means (such as models conveying 'anger' through behaviour alone) might well result in the experience of 'anger'. Another problem, but this time on the endogenous side, is that there is 'considerable uncertainty' as to what cues have to be present to naturally elicit core emotions (see Berkowitz and Le Page 1971:351), or, more generally, as to the range and 'strength' of such emotions. And on the ethnographic side, it is not clear whether the Chewong have direct ways of talking about emotions: Howell, in personal communication, tells me that the Chewong do say, 'I *am* angry'.

It follows that it is difficult to know exactly what significance to attach to the facts that superhuman beings attack people (Howell 1980:218) and that there do not appear to be any positive models for 'fear' or 'shyness'. I still maintain, however, that there is sufficient evidence - both ethnographic *and* psychological, however unfashionable it might be to infer from the latter - to suppose that Chewong do not live with the same emotional repertoire as we do. Leff, it seems to me, is justified to claim

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<sup>10</sup> See Heelas (in press). Psychological findings, obtained from experiments where conditions and consequences can be much more exactly specified and measured than in the field, must surely be more important to the social anthropologist than is perhaps often acknowledged. Granted that there are more variables in field circumstances, and that it is even more difficult to 'examine' emotions as inner experiences, experimental research (combined with investigation of ethnographic circumstances) provides a way forward along the path of reasonable speculation.

that despite the difficulties, 'there is a strong link between the availability of appropriate words for the various emotions and the ease with which people distinguish between their experiences' (1973:304), and that 'somatic' experiences are a dampened version of 'cognitive' varieties (1977:324). Who would want to maintain that meanings do not change experiences, including those of an emotional variety? Who can deny that emotions are in part (whatever the extent) constituted by meanings? So, given even a minimal difference between Chewong and our own modes of thought, who would want to deny that psychic disunity pertains between these cultures?

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## REVIEW ARTICLE

### WHITHER CHINA? FEI HSIAO-T'UNG AND ANTHROPOLOGY IN CONTEMPORARY CHINA

R.David ARKUSH, *Fei Xiaotong and Sociology in Revolutionary China*, Cambridge, Mass. and London: Harvard University Press 1981 [Harvard East Asian Monographs 98]. xviii, 322pp., Bibliography, Glossary, Index. £10.50.

FEI HSIAO TUNG, *Toward a People's Anthropology*, Beijing: New World Press 1981. vi, 111pp., Photographs, Map, Appendices, Index. No price given.

With British and French anthropology reportedly in the doldrums,<sup>1</sup> the publication of Arkush's book is particularly apposite. Through the biography of its subject, Fei Hsiao-t'ung, it illustrates the dismantling of the entire discipline in post-revolutionary China. While Western anthropologists bemoan the lack of direction they perceive in the current post-structuralist interregnum, at least they are able to probe new avenues at will. Our colleagues in China can however, only despair. From 1952, when all university anthropology and sociology departments were closed by the Government, until the time of the publication of Arkush's book in 1981, the academic disciplines of anthropology and sociology had officially ceased to exist. (That there ever was, or ever could be, an independent discipline of anthropology in the People's Republic is a question I will return to below.)

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<sup>1</sup> See 'Commentaries' by Collinson and Picone in *JASO*, Vol.XIII (1982) pp.287-299; also Kuper's observation that the 1980s are a 'pinched, anxious, sceptical, uncertain' period for anthropologists in A.Kuper, *Anthropology and Anthropologists: The Modern British School*, London: Routledge & Kegan Paul 1983, p.x.

As is evidenced by the title of this book, Arkush weaves together two themes. On the surface we find an orthodox biography of one of the major figures in Chinese anthropology. This is only part of the story however, for through Fei's biography the reader is also provided with a general introduction to the destruction of the social sciences in modern China.

As biography, Arkush's book has much to offer and is interesting on two levels. Specifically, it is a stimulating biography of Fei in which his many roles are examined. To those of us who know Fei only through his academic publications, Arkush's book is revelatory, and the prominence that is given to Fei's non-academic roles does much to put his academic work in perspective. Here a new side of Fei is presented: we are informed of his work as an activist and as a popular writer; of his work in the 'Third Stream' of Chinese intellectuals (who sought, through the Democratic League, a middle ground between the KMT and the Communists in the years before the consolidation of Communist power); of his work as an important populist and cultural broker (through his frequent appearances at mass meetings in factories, schools, and government assemblies) in the formative years of the People's Republic. Through this detailed examination of Fei's career, Arkush illuminates dimensions of Fei's life previously hidden from Western scholars, which, once revealed, do much to clarify incidents in his professional life. Taking one episode as an example, Arkush's treatment does much to resolve the confusion over Fei's confrontation in the pages of *Encounter* with Wittfogel and the 'Cold Warriors' in 1955.

On a more ambitious level the examination of Fei's career is used to provide the reader with an introduction to the intellectual history of twentieth-century China. In this sense the author employs Fei's life history to adumbrate an entire class during a pivotal period of Chinese history. Arkush maintains,

Just as Fei thought Chinese society could be illuminated by microscopic studies of carefully chosen single communities, so too it is my hope that a study of a single influential example like him can contribute to an understanding of what it meant to be a Westernized intellectual at the time of one of the greatest revolutions in human history.

In the details of his and his relatives' lives can be glimpsed a process of significance for China's modern history: the transformation of traditional local gentry into urban, Westernized, modern intellectuals separated from the rural society which is most of China.

This representation of Fei as microcosm is both ambitious and problematic. Through Fei's biography we see most graphically the rapid and sometimes violent transformation of Chinese society with which the Chinese intelligentsia had to contend. In Fei's biography we have detailed before us the example of one intellectual's accommodation to revolutionary change in China. Insofar as any one life *can* be taken as representative of an entire class in the re-making

of China, this book is a contribution to that debate.

As biography, this book has much to commend it. Arkush brings to bear a depth of research of impressive proportions. Where else, for example, could we learn that 22 of the 36 faculty members at Fei's first university (Soochow) in 1925 were Chinese (six of whom had been educated abroad), with three Ph.D.s and 15 M.A.s on a staff teaching 341 students; that 14 of the 16 Christian Colleges in China offered courses in sociology that year; or that in the year of Fei's admission to Yenching University (1930) its Sociology Department library received 20 English-language journals and 70 Chinese-language journals, and moreover contained 2000 books? Arkush is equally conversant with the personal details of Fei's life which he deploys to great effect. I found, for example, that Arkush's explanation of the events that led to Fei's fieldwork in Kaihsienkung greatly enhanced my understanding of his *Peasant Life in China* (London 1939). Similarly, the excellent and extensive (43pp.) annotated bibliography of Fei's Published works will be a boon to the growing army of Fei-watchers.

This is not to say that there are not occasional lapses in the text - for example identifying Fei as among the first fieldworkers in China (which does scant justice to the contribution of D.H. Kulp's classic *Country Life in South China* (New York 1925) and ignores entirely such important antecedents as Leong and Tao's *Village and Town Life in China* (London 1915) and A.H. Smith's *Village Life in China* (New York 1899)), or the omission from the Index of Boorman, Hawtin, Sanchez and Wong (all cited on page 323 of the text) - but, happily, most such lapses are innocuous. A minority, however, suggests a general lack of familiarity with anthropological literature - for example his reference to 'Malinowski's *kulu*' (*sic*) on page 144 - and this gives rise to more important reservations about the book.<sup>2</sup> It is difficult to countenance the narrow scope of the biography and the resulting imbalance in it. While Arkush successfully follows Fei's accommodation to revolutionary change in China, this represents only one aspect of his career. While it tells us much about the pertinent circumstances of Fei's life and of the events that shaped his career, it is curiously imbalanced in that it pays remarkably little attention to Fei's own contribution to the discipline of anthropology. That he did play a major role in the evolution of the discipline (both in China and the West) cannot be denied.<sup>3</sup> But Arkush confines himself to the con-

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<sup>2</sup>The high standard of production generally maintained in the Harvard East Asian monograph series makes it unlikely that this is merely a type-setter's error.

<sup>3</sup> See Malinowski's enthusiastic Preface to Fei's *Peasant Life in China*, in which he credits Fei with opening the Aladdin's cave of non-tribal societies to anthropological study. Even allowing for what Arkush has called 'prefatory hyperbole', it is clear that Malinowski saw Fei's work as pushing back the accepted boundaries

sideration of Fei's effect on the social sciences in *China*. In the light of Fei's major contribution to the discipline as a whole, I find it disappointing that Arkush limits his assessment in this way. Of course it is always difficult to reconcile a reader's expectations with an author's intentions, but it seems that this oversight detracts from an otherwise remarkable piece of scholarship.

Imbalance is apparent in the text in other forms as well. The concentration on Fei's early years is almost complete: of the 286 pages of text the first 257 pages only take us up to 1957 - leaving Arkush to race through the past 26 years in the final 29 pages (first in an Epilogue, then in a Postscript). While the reasons for these addenda may be admirable (to be generous one could see them as a conscientious attempt to bring the text up to date), the execution leaves much to be desired. The reasons for this are clear: Fei's disappearance from public and academic life from 1957 to 1979 denied Arkush the documentary material on which this biography is based. But this merely illustrates another shortcoming in the text: Arkush's overwhelming reliance on Fei's own publications. Arkush uses Fei's prolific output to provide the basis of this biography to the virtual exclusion of other source-materials. This hamstringing the author and forces him to terminate Fei's biography prematurely at the time of the anti-rightist campaigns that followed the Hundred Flowers.<sup>4</sup> But although Fei ceased publishing

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of the discipline:

The anthropology of the future [he wrote in 1939] will be... as interested in the Hindu as in the Tasmanian, in the Chinese peasants as the Australian aborigines, in the West Indian negro as in the Melanesian Trobriander, in the detribalized African of Haarlem as in the Pygmy of Perak.

A position accepted as commonplace now, but bold words in 1939.

Nor was Malinowski the only Western anthropologist to recognize the importance of Fei's work. Maurice Freedman went so far as to refer to the 'Chinese phase in social anthropology' wherein the new directions and priorities in anthropology were credited directly to Fei's pioneering effort 'that pushed the frontiers of anthropology outward from savagery to civilization' (M.Freedman, 'A Chinese Phase in Social Anthropology', *British Journal of Sociology*, Vol. XIV (1963), p.1). Freedman commented elsewhere on the 'general reverence' anthropologists still hold for Fei. 'To this day', Freedman wrote, 'Fei is a piece of furniture in the minds of Anglo-American liberals' (M.Freedman, 'Sinology and the Social Sciences: Some Reflections on the Social Anthropology of China', *Ethnos*, Vol. XL (1975), pp.194-211; at p.197). Nor is Fei's importance restricted to the past of the discipline: his fieldwork monographs remain among the best ethnographies of Chinese society generally.

<sup>4</sup> Fei was among the most prominent targets of the anti-rightist backlash. Indeed, one commentator identifies the 'first and decisive blast' in the Government's anti-rightist campaign as an editorial in the influential *People's Daily* which took as its title a satirical version of the title of an earlier article by Fei (see

at that time, he has continued to play a role in post-Maoist China worthy of examination. This is particularly true for the period since 1972. Other authors have found ways of penetrating the wall of silence surrounding this period and have valuable things to say about Fei's place in it.<sup>5</sup> Even if Arkush's reliance on Fei's publications alone denied him direct access to this period of Fei's life, his lack of interest in these secondary sources seems perplexing and undermines the value of his text.

While it is clear that Arkush intended his work to be a biography of Fei through which the experiences of a generation could be examined, his book is also of interest for the light it throws on the dissolution of academic sociology in China. Here the imbalance in the text is rendered advantageous, and the section on the Hundred Flowers period nicely complements Wong's admirable study of the rise and fall of sociology in China. The confrontation between sociologists and the Government in the Hundred Flowers period is deftly handled, and Arkush shows clearly the twin dilemmas with which sociologists had to contend. On the one hand a shift towards a 'socialist' rather than an 'academic' sociology was imperative if the discipline was to survive in any form.<sup>6</sup> But at the same time such a shift resulted in a repudiation of their new work in some Western circles (e.g. Wittfogel) and their isolation from the mainstream of sociology. On the other hand we note the escalating conflict at this time between the sociologists (fighting a rearguard action against the contraction of their discipline since

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A.King, 'The Development and Death of Chinese Academic Sociology', *Modern Asian Studies*, Vol.XII (1978), pp.37-58). There was a vigorous campaign against Fei in the popular press (Arkush refers to 'perhaps a hundred' books and articles criticizing Fei) which published articles with such titles as 'The Reactionary Nature of the Functionalist Social Anthropology Imported by Fei Xiaotong and Others', or his friend Lin Yueh-hwa's article entitled 'The Sinner and Ugly Fei Xiaotong'. Fei's vilification was complete: he had the dubious honour of being personally savaged by Chairman Mao Tse-tung himself in a passage now immortalized in Mao's *Selected Works*.

<sup>5</sup> See G.Cooper, 'An Interview with Chinese Anthropologists', *Current Anthropology*, Vol.XIV (1973), pp.480-481; A.R.Sanchez and S.L.Wong, 'On "An Interview with Chinese Anthropologist"', *China Quarterly*, Number 60 (1974), pp.775-790; and King, *op.cit.* Wong's excellent *Sociology and Socialism in Contemporary China* (London 1979) Arkush notes on page 323 as having been unavailable to him.

<sup>6</sup> On this distinction between 'academic' and 'socialist' sociology see Wong, *op.cit.*, and King, *op.cit.*

1952), who saw the re-establishment of the discipline in academic institutions as an intellectual issue, and the Chinese leadership, which saw it as a political issue. For the authorities, sociology was anti-socialist and anti-Marxist, and, consequently activities directed towards the reinstatement of sociology departments was politically suspect. Once perceived on a political/ideological plane the dismantling of the discipline was inevitable. As King put it in 1978, 'Today in Communist China sociology...is completely dead'.<sup>7</sup>

That there could ever be an independent discipline of sociology or anthropology in China - both in light of the present Government and with reference to the intellectual history of China - is a question worth pursuing. Up until now I have deferred to the convention of Hsu (and implicit throughout Arkush) that the terms 'anthropology' and 'sociology' be used interchangeably.<sup>8</sup> There is, however, *prima facie* evidence to suggest that the equivalence between 'anthropology' and 'sociology' may not be absolute. Certainly two different terms for anthropology (*ren lei hsueh*) and sociology (*she hui hsueh*) exist in Chinese. These terms derive from different roots which, in fact, closely parallel the difference in usage of their English counterparts. It will be noticed that the core elements of the two terms differ. The *ren lei* in *ren lei hsueh* ('anthropology') may be glossed as 'human beings' or 'mankind' from the two characters 人 (*ren*, 'man') and 類 (*lei*, 'a class, species, kind').<sup>9</sup> The core of *she hui hsueh* ('sociology') is derived from the unrelated characters 社會 (*she hui*) which can be variously translated as 'society, community, social order, an organized society'. *Hui* (會) alone can be glossed in terms of an association of people: 'to meet, to co-operate; a society, guild, association'. The first character of the compound (*she* 社) is more difficult to translate as its meanings vary widely depending on context. In this instance it derives from a term relating to a village or hamlet in which the inhabitants share a common altar to the earth god, and can be loosely translated as 'a company or society'.

It can be noted, therefore, that in the Chinese terminology

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<sup>7</sup> *Ibid.*, p.57.

<sup>8</sup> 'Few serious Chinese scholars,' Hsu wrote in 1944, 'today maintain the distinction between the once separate disciplines' (cited, approvingly, by M.Freedman ('Sociology in and out of China', *British Journal of Sociology*, Vol.XIII (1962), pp. 106-116; at p.106). This view is accepted also by Sanchez and Wong (*op.cit.*, fn.4). Wong (*Sociology and Socialism*, *op.cit.*, p.79) similarly: '...for many Chinese students of society, sociology and anthropology were inextricably enmeshed.'

<sup>9</sup> For these and other terms cited below, see R.H.Mathews, *Mathew's Chinese-English Dictionary*, Cambridge, Mass.: Harvard University Press 1969.

there is the same vulgar bifurcation of subject-matter implicit in our own usage of the relevant terms: thus anthropology is the 'whole science of man..., study of man as an animal' and sociology the 'science of the development and nature and laws of human society' (as defined in the *Concise Oxford Dictionary*). It could be argued that in the formative years of the social sciences in China, as in the West, the term 'anthropology' was laden with connotations of the primitive and the exotic, whereas sociology gained respectability through its study not of 'man the animal' but rather 'the science of human society'. Certainly the pejorative sense implicit in the term 'anthropology' was readily apparent in the 1930s, and it is easy to impute a Chinese antipathy to the application of the term to the study of their own society.<sup>10</sup> This, perhaps, explains why there were several sociology departments in Chinese universities in the 1930s, but not a single anthropology department. Indeed, it is only in the post-colonial world that the attention of Western anthropologists has shifted in a large way to the study of modern, industrial societies. Certainly at the time Hsu was writing the anthropological world was largely confined to that of 'primitive man'. The notion that Chinese society could be studied by that sort of anthropology would have been anathema to the Chinese (whose own perception of the self was no less strong than the ancient Greeks' antipathy to 'barbarians'), and so it was that a blurring of the disciplinary boundaries came about.<sup>11</sup>

That the distinction has since remained blurred in China may be related to the subsequent history of the country. The years of occupation, civil war and revolution effectively stymied the growth of the discipline; it was the lack of contact with other intellectual and academic traditions that has rendered invisible the demonstrably *methodological* (rather than *topical*) differences which are now seen to be the primary distinguishing features between the studies of anthropology and sociology in the West. Thus while Hsu's statement may have been valid at one time it now represents a bias outdistanced

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<sup>10</sup> Wong, taking the opposite tack, suggests that 'sociology' was a mode of discourse particularly attractive to the Chinese in the 1930s because of its pretence of being *scientific*.

<sup>11</sup> In Fei's reminiscences about the early history of sociology in China he refers indirectly to the prejudice against anthropology. He reports that the work he and some of his colleagues were doing in the 1930s did not conform to British and American conceptions of sociology: '...some friends and I smuggled in something else under the name sociology' (cited in Wong, *op.cit.*, p.79). But they nevertheless sought to call what they were doing 'sociology'. The reason he gives for this deception illuminates the intellectual climate of the time. 'But at that time anthropology and ethnology were not regarded as respectable academic labels, so very often they [Fei and his colleagues] had to take shelter in the departments of sociology' (*ibid.*).

by events in the growth of the discipline. As the Chinese have been cut off from the significant events in the evolution of the social sciences for three decades they seem to be unaware of the vast changes that have occurred. Specifically, they still labour under a bias no longer relevant to the subject but which continues nevertheless to colour the perception and reception of anthropology as an academic discipline in China.

But if anthropology in China was first subsumed and later eliminated because of its association with sociology, is there anything we can say of the future? While anthropology in China has been still-born and academic sociology has been pronounced as 'on the verge of extinction'<sup>12</sup> or even, as King claims, 'completely dead', there may be some cause for cautious optimism about the future. We may, at last, be witnessing the creation of an *indigenous* Chinese anthropology. Events of the past five years are encouraging. The early reliance on an alien intellectual tradition was effectively terminated after the closure of the university departments in 1952, and the subsequent repudiation of the Western (i.e. non-Marxist) tradition is complete. However throughout the period from the Hundred Flowers to the present there has been a small nucleus of researchers doing work which in some ways does approximate that of anthropologists. This is the work of the staffs of the various institutes which study the Chinese minority populations and who define their work as 'ethnology'.<sup>13</sup> It is anomalous that Chinese national minority work continued throughout the period of suspension of the social sciences in China. Some Chinese anthropologists and sociologists (including Fei) were shifted into minority work after the closure of the university departments. This has caused some Western observers to see this as evidence of applied anthropology in China, but such a conclusion is probably naive. From the little we know about the Central Institute of Nationalities and the various regional National Minorities Institutes the picture is not clear.<sup>14</sup> Preliminary analysis indicates

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<sup>12</sup> Cooper, *op.cit.*, p.481.

<sup>13</sup> The etymology of the Chinese term for 'ethnology' (*ren chung hsueh*) is interesting and again betrays a certain ethnocentrism which would prohibit its application to Han Chinese populations. The term is composed of the characters for 'man' (*ren* 人) and 'seed, grain; a kind or sort' (*chung* 種). The use of this second character is highly suggestive. It appears in such compounds as 'classify, type' (種別) and 'variety, type, species, genus' (種類), as well as in the compound which is translated as 'clan, race, or tribe' (種方). Wong states that contemporary Chinese use the term idiosyncratically and define ethnology as 'the study of (Chinese) minority peoples' (*op.cit.*, p.78).

<sup>14</sup> Cf. *ibid.*, pp. 80ff; and see G.Gjessing, 'Chinese Anthropology and New China's Policy toward Her Nationalities', *Acta Sociologica*, Vol.II (1957), pp.45-67.

that these institutes function in a way not unlike the early activities of the Bureau of Indian Affairs in the United States - that is, as an organ of the Government in which research priorities are subordinate to the Government line. Such an interpretation is consistent with the drastic reversals in their activities in the past two decades. But whether their activities are in a form properly identifiable as anthropology as such is another matter. Gjessing, reporting on his visit in 1954 to the Central Nationalities Institute, took the view that it was the only place where 'academical tuition in anthropology' (*sic*) was given.<sup>15</sup> Whatever can be said of his view at that time, it is doubtful whether it could be maintained today: the work of the Institute's staff now only vaguely resembles that of Western anthropologists. Examination of their research methodology is informative:

...team work and not individual enquiry was the rule. The collection of information, preparation of the preliminary drafts, discussions, and then revisions for selective publication were all conducted collectively.<sup>16</sup>

Such ethnography by committee seems to be the antithesis of the participant observation which is the corner-stone of Western anthropology. Moreover, the inclusion of ideologically-sound cadres on the research teams doubtless influences research priorities and conclusions. Thus Wong notes that national minority research is used in China as ammunition to prove the validity of Marx's pronouncements on socialist development and revolution.<sup>17</sup>

Nevertheless, while the work of these Institutes may be politically motivated and circumscribed they do perform a valuable function. Since the 1950s, under Fei's leadership, the National Minorities Institute has engaged in activities which now fall under the rubric of 'urgent anthropology' in the West (i.e. the 'rescue' of data about China's minority populations before it is 'irretrievably lost during rapid social changes').<sup>18</sup>

Whether the work of the National Minorities institutes will evolve into a recognizable anthropology remains to be seen. Certainly the foundations are there, but a reconciliation between Government policy and academic affairs will have to be effected before such a development can occur. At present, with the Chinese government committed to a Soviet-style *etnografia*, the outcome is in doubt.<sup>19</sup>

<sup>15</sup> *Ibid.*, p.59.

<sup>16</sup> Wong, *op.cit.*, p.89.

<sup>17</sup> *Ibid.*, p.86.

<sup>18</sup> Fei, cited *ibid.*

<sup>19</sup> The Soviet approximation of 'anthropology' is ideologically circumscribed. Gellner defines their *etnografia* as the study of 'pre-capitalist social structures' (E.Gellner, 'The Soviet and the Savage', *Current Anthropology*, Vol.XVI (1975), pp.595-617; at p. 595; cf. T.Dragadze, 'Anthropological Field Work in the USSR',

Western access to the work of the National Minorities institutes is restricted, and publications by their staffs seem to be directed to internal consumption, making it difficult to assess their continuing role. The past five years have, however, seen a slight easing of the climate and tantalizing morsels of their work are now beginning to surface in the West. One of the most significant of these is the publication (in English) of a collection of recent essays by Fei. Not only does this mark Fei's rehabilitation but it provides us with an invaluable window on the activities of the National Minorities institutes and, indirectly, on the current climate of anthropological research.

The six essays in this volume may well define the place of anthropology in China. In the first essay (which provides the title of the volume and which is the re-publication of Fei's address on his receipt of the Malinowski Memorial Medal in Denver in 1980), Fei makes clear to his Western audience the independent status of the 'new' anthropology in China. In this essay he expands on his earlier rejoinder to Cooper (*op.cit.*) and points out again the incompatibility between Western conceptions of anthropology and his own activities in China. It is clear that 'the things that anthropologists do' takes second place to his 'revolutionary work' in China. The other essays also indicate the research priorities of the National Minorities institutes. Essay 3, 'On the Social Transformation of China's National Minorities', purports to explain why tribal groups in China remained 'primitive'. The explanations put forward are mono-causal and seem crude to the Western ear. We are told, for example, that the reason the Lisu people did not progress is because successive Han Chinese governments prohibited them the use of iron. The essay is reminiscent of nothing so much as the 9-fold evolutionist scheme from savagery to civilization so long repudiated in the West but kept alive by Marxist 'modes of production' research.

The intrusion of ideology occurs in other places as well. We note, for example, that the essay entitles 'China's National Minorities - An Introductory Survey' contains the text of a poem by Mao to explain the plight of China's minorities. Whether such obeisance is obligatory or merely prudent, it seems inappropriate to Western social scientists. The final essay in this volume, 'Revisiting the Mountains of the Yao People', provides numerous examples of the confusion between ideology and research. We note, for example, the (obligatory?) castigation of Lin Piao and the Gang of Four and we are told of the damage their policies did to the region. Fei's recapitulation of the history of the Yao people also has a strange ring to it:

Final victory [from oppression] came only after the Chinese

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JASO, Vol.IX (1978), pp.61-70. As long as such ideological restraints are in operation the interface between Western anthropology and Chinese ethnology (or Soviet *etnografia*) will remain imperfect.

Communist Party united all the nationalities in China in a concerted effort to topple the three mountains crushing the Chinese people - feudalism, imperialism and bureaucrat capitalism. In the case of the Yao's, liberation came in the shape of the People's Liberation Army.

At times this essay reads like the text for an advertisement for either the Chinese Tourist or the Chinese Development Boards. Of his first night back among the Yao after a 43-year absence Fei writes,

After a brief rest, I pushed open the windows and was greeted by the sight of a galaxy of lights sparkling on both sides of the Jinxiu River. I asked myself if I was really in the Dayao Mountains. Just then my host turned on the lights in the room and said, 'We can't use all the power our hydro-electric station produces. We need faster development.'

After treating his readers to a description of the marvels of the Jinxiu Central Power Station (with its two 1600-Kilowatt generators) Fei tells us of the changes this has wrought for the Yao:

Forty-three years ago, when night fell over the mountains... I used a flashlight and this was always a signal for local urchins to rally around and follow me about. But now every home had electric lighting, every production brigade showed movies and every commune possessed a TV set. A project was even under discussion to publicize the use of electric stoves as a way to save on firewood.

Riveting stuff, perhaps, but inadequate for an anthropological audience.

In his Preface Arkush talks of the three different roles that Fei plays in modern Chinese intellectual history: as social scientist, political journalist and cultural intermediary. If the selection of the essays in his *Toward a People's Anthropology* can be taken as an indicator, Fei's rehabilitation is incomplete. Only his roles as political journalist and cultural intermediary have been reconstituted. It remains to be seen whether he will regain his stature in the international academic community. As long as the requirements of Marxism-Leninism and the Thoughts of Mao Tse-tung remain paramount and 'all other theoretical systems which have the capacity to interpret social realities are repudiated as reactionary or revisionist',<sup>20</sup> the prospect must remain in doubt. If Fei's current essays are, in fact, indicative of the current state of affairs in China then we see an anthropology reduced to providing 'proof' of Marxist axioms. In this light it would, perhaps, be no more contentious to observe that the Soviet and the Chinese rejec-

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<sup>20</sup> King, *op.cit.*, p.58.

tion of Western anthropology on the grounds that it was a 'handmaiden' to colonialism in the West is mirrored by the use of anthropology as a handmaiden to Marxism-Leninism in the East. It would seem that we have some way to go before we discover a common ground for discourse.

STEVEN SEIDENBERG

## DISINHERITING THE TZOTZIL:

### NEUTRALITY AND IDEOLOGICAL EFFECT IN ETHNOGRAPHIC DISCOURSE

Despite a wide divergence between the methodological practices of North American and European anthropology and the difference in the problems they take as constituting their principal interests, some of the effects of their discourse are remarkably similar. The point is reminiscent of the ideological effects of neo-Marxist texts in radically undermining the foundation and nature of indigenous cultures - caused by their uncritical use of a language, politically circumscribed, permeated by ethical nuances which continue to stem from an essentially bourgeois or western epistemology. Similar considerations lead us to scrutinise more closely certain unsettling characteristics of North American ethnography, which through its own particular practices give almost identical effects. The presuppositions which run through their methodological practices are almost never articulated, nor even thought problematic. In contrast with neo-Marxism, American ethnography is notable for its complete blindness to the presuppositions and effects of its theoretical practice. In the United States, philosophy, politics and anthropology are never mixed. Ignoring its own practice, ideologically impregnated according to the precepts of a distinct cultural logic, it is unable to detect other logical structures in alien societies. Its own discourse, perceived as scientific, rigorous and exacting, employing a neutral language, more often than not effects an absorption of indigenous categories according to the tenets inherent within itself.

The problems, within the limitations of our phrasing of them, will be pursued through a series of texts originating from the work of the rather ambitious Harvard Chiapas Project under the auspices of Evon Vogt.

Neither Vogt nor any of his associates have ever articulated

the theoretical method from which they proceed. Presumably, they have thought it has not been worth the effort since its tenets have appeared elsewhere defining the 'scientific method' and the status of the anthropologist as voyeur (such as Parsons), or perhaps they themselves are largely unaware of the practices which they execute in deriving primary information and its subsequent articulation in textual discourse. It is a truth not easily disputed that we show a greater interest in results than in the practices, which we follow almost automatically at a sub-conscious level. We are able to perceive some of the presuppositions which inform these texts as they appear in (a) explicit but abstract statements, i.e. as heuristic props to the principal arguments, divorced from further theoretical consideration, and (b) implicitly within the organisation and presentation of the 'ethnographic data'.

The principal document informing us of category (a)-type intent is Vogt's 'The Genetic Model and Maya Cultural Development' (1971). In this paper which has had a widespread influence on many of his colleagues, Vogt assumes that a population must successfully adapt itself to the 'ecological niche' in which it finds itself before it can prosper and expand.

In the genetic model as applied to human populations, we assume that a small proto-group succeeds in adapting itself efficiently to a certain ecological niche and in developing certain basic systemic patterns which constitute the basic aspects of the proto-culture. If the adaptation proves to be efficient the population expands and the group begins to radiate from this point of dispersal. As members split off from the proto-group and move into neighbouring ecological niches, they make appropriate adaptations to these new situations and begin to differentiate - that is there are adaptive variations from the proto-type over time as the members of the genetic unit spread from the dispersal area. (Vogt 1971:11-12)

We cannot be sure from this statement which aspects of the proto-group emerge as adaptations to the environmental pre-conditions and which, if any, grow wild as weeds. On the level of collective representations, Vogt does not state whether his 'basic systemic patterns' encode relations between the group and its ecology and as such represent symbolic articulations of an external logic decreed by the laws of natural survival, whether they pre-exist the group's efficient adaptation, and therefore, I would understand, the social organisations which accomplish that adaptation, or simply co-exist independently. He goes on to suggest that three factors can lead to variation in language, physical type and systemic patterns. The first of these is, again, change of ecological settings, but the other possibilities are the experiences of external culture contact and internal biological, linguistic and cultural drifts that take place over time within

the different genetic units.

Thus it would appear that Vogt gives primary significance to an initial ecological context demanding specific and efficient adaptation on behalf of the proto-group. He does not state explicitly if adaptation is achieved specifically by the patterns of social organisation which emerge, but he does note that exceptions to the general dispersed settlement patterns among the Maya have emerged only in areas where special topographical conditions exist, such as the mountainous constrictions around Lake Atitlan or population pressure exerted by neighbouring groups in Northern Yucatan. Once the initial and successfully accomplished adaptation has taken place, internal factors within the basic cultural configuration can diversify and lead to elaboration and innovation. Before considering their view on ideology or collective representations, we can achieve a greater clarity on the relation between environment and social organisation through the work of Vogt's colleague, G.A. Collier, *Fields of the Tzotzil: The Ecological Basis of Tradition in Highland Chiapas* (1975).

Collier, while not apparently holding that kinship as a whole is an adaptive response to local environmental conditions, does believe that certain functions of kinship evolve as responses to external situations. Among the general functions of kinship is the direct relation it often imposes between succession of property rights and biological descent. In small communities with abundant land and natural resources, conflict will be limited, but as the population increases and land becomes harder to obtain, conflicting claims will often enhance descent-based systems, clearly defining the legitimate lines of succession. As an inverse corollary, populations that derive their solidarity from shared resources would experience a decline in this solidarity if the resource base increased, creating a large excess. Similarly, with high population density, the resulting fragmentation of the land could cause the decline of such groups. Applying this hypothesis to several Maya communities - Chichicastenango, Santiago, Chimaltenango, Apas, and Chan kom, where swidden agriculture dominates the economy, and Chamula and Amatenango, in which wage labour and craft specialisation are of more importance - Collier finds strong principles of patrilineal descent in the former, while bilateral descent characterises the latter two. Thus the functional connection between kinship and land use is substantiated. He concludes:

...in the Maya culture area, patrilineal emphasis in social organisation is strong where land is highly valued as a resource and is available in amounts great enough so that it can serve as a mechanism through inheritance, for binding together the affairs of a man and his heirs (Collier 1975:76).

A second adaptive function of the family is in stabilising the relation between socially valued resources and roles within

it. In this sense the family as the elementary unit of production maintains traditional relations of production with the environment and reproduces the ascriptive values associated with the process, despite the constant flow of maturing adults. The stability of these systems depends on the balance that a population achieves with its resources. These may be undermined, however, by demographic increase, either internally, or by pressure exerted from externally expanding groups on limited available resources. As we have seen, demographic increases and the resultant shortage of land in Chamula and Amatenango have resulted, according to Collier, in a shift from patrilineal to bilateral kinships. In summary Collier writes:

Kinship is a traditional behaviour that is not a regressive survival from the past but rather a dynamic adaptive response to placement in a setting, a locale with limited potential for human use (Collier 1975:107).

Generally, where land is abundant and a free good, swidden farmers do not have a descent organisation, but where land is scarce and a valued commodity, descent emerges to systematise rights to land. When, however, landownings are overtly used and excessively fashioned by inheritance, farming tends to give way to other occupations and land ceases to motivate descent based kinship (Collier 1975:206).

How collective representations and the wider ideological system enter all this is not clear. We are, however, given one clue in Vogt's (1971) suggestion as to the origin of the pyramid in Mesoamerica. He assumes that as among the contemporary Tzotzils, the early Maya believed that the mountains and hills were the dwelling-places of ancestral spirits and gods. With their later migrations to the lowlands, a religious problem would have ensued because their gods would now be homeless - hence the beginning of the pyramid constructions as metaphoric mountains in which the gods could adapt and live. Thus, just as the modern Tzotzils, like the ancient Maya, adapt their social organisation to fit into the environment, the divine order of beings also adapted their residence in accord with external factors. Ideological constructions appear to follow the same logic as social organisation. Whether man or divinity, the environment takes precedence; man follows in his utilitarian activities and the gods are dragged behind.

Regarding the implicit indications which would substantiate such a theoretical methodology, we must observe the organisation of the texts of these authors. This is complicated by the minute dissection they have achieved of indigenous cultural categories, packaged up, one might say, into Western standardised portions which are then examined and scrutinized. However, it does seem that Vogt himself has been designated to write the syntheses of the work and here we shall discuss the presentation

of his *Zinacantan: A Maya Community in the Highlands of Chiapas* (1969).

This work is divided into two parts. The bulk of the volume is dedicated to recording the ethnographic data as a description of the life of the Tzotzil community. Tagged at the end are a few chapters which then discuss in very general terms the relationship between various parts of the society, projecting present tendencies into the future. Below I have arranged an abstract of the contents of this volume following the same sequence as it appears in Vogt 1969. The accompanying notes to the right of each entry are inserted as commentary.

1a. Physiography, soils, geology, flora and fauna

It is curious how every ethnography begins with the unethnographic. Physical descriptions are made from Western standards, pointing out those things of interest to ourselves (fertility of soil, rainfall, economic geology) using our classifications (zoological, botanical, geological). There is nothing indigenous in this and the discourse here is completely separate from the rest in anthropology. In some works, however, the remainder can be a continuation of the naturalism that informs the text at its very inception. If this is a background, and introduction to the basic problems of survival, a context in which the ethnographic description begins or which circumscribes it, then it is just that, from the tainted vision of the Western observer.

1b. History

Here opens the second distinct discourse of the text. History always has a different epistemology to that preceding and proceeding this chapter. It must be a hypothetical reconstruction and the logic which informs the venture can be naturalist or cultural. It can never refer to a past reality and contains within it the ideological pretensions of the method. If it is to give depth to a contemporary culture it may be a backward projection of that culture already mediated through Western eyes, thus its remoteness is twice distanced.

If it is indispensable to the analyst then it is as a means of projecting his methodological tenets onto historical cultures as a means of their utility and justification, i.e. to show the same factors occurring in the past as during the present, historical adaptation. Thus history can be the hinge on which the naturalisation, already described for the physical environment, opens out into the domain of cultural history and from there into culture generally. Note physiography and ecology are included in the same section as history.

2. Subsistence Activities. The third discourse is again different from the former and is concerned with description and technique, what an older academicism termed 'material culture'. These activities are all utilisations of nature and are traditionally cited as traits that mark the separation of man from his animal relations, and the foundations of culture. They are in fact all activities that mark the transformation of a rude and savage nature by conscious activity, and in this context the importance of these practices are noted and made spatially prior to any other elements that make up a society. These are direct relations determined largely by the environmental pre-conditions. Note the continuation of discourse one into the realms of discourse three and the subsequent naturalisation.
  - Housing, manufacture durability and cost
  - Clothing
  - Agricultural activities
  - Trade and other economic activities
  
3. Social Structure
  - Groupings
  - Settlement patterns
  - Life cycle
  - relationship system
  - cargo system
  - political systemFrom subsistence activities, nature enters into society and ensures its efficient adaptation and economic survival. All these systems have a logic and economy of function which matches them perfectly, harmonising and covering the contradictions of dysfunctional components. In the last analysis it follows the natural logic which

had its initial moments in the perception of nature by man and his grasping of 'essential' relations.

4. Religious beliefs and rituals

Once the naturalist interpretation of a society has been completed religion and ritual remain as the residual categories of the indigenous symbolic logic (what the British evolutionists termed 'survivals'). They are either subsumed as positive expressions of natural relations constituted outside of themselves or as curious and arbitrary. They are the remains of a much larger classification which the anthropologist takes as his domain of inquiry.

5. Some principles and processes

This discourse is that of anthropology, i.e. it is said to be external and different from discourses two to four. External because it derives from a foreign culture to that of the subject of the text; different not only through this, but in its higher abstractive and interpretive abilities. In fact, here begins the articulated explication of the relations and juxtapositions implicit within the previous discourses. Just as at the beginning it was the past that justified the present (and, therefore, the method of the theoretical practice in interpreting it), the conclusion further extends the competence of that method in predicting the future course of natural events and the probable responsive adaptations necessary.

Taken as a whole, the text presented in this way suggests an evolution. Through the textual evolution, we derive a revelation of the society, i.e. that to which the text is said to pertain. This is successive only in as much as the text itself is successive; cumulative only in so far as the text is cumulative, and rational only to the extent of the rationality of the text. Now, the text is none of these 'things', if we take them as signifying pure categories. Rather it is a pastiche, being successive only in as much as it composes and strings classes together, drawn as we have seen from different discourses each based on widely

different tenets; cumulative only in the enforced relations between these classes and the conclusion which bourgeois rationality necessitates; logical only in terms of the implicit and covert rationality of this theoretical practice. What we derive then is not a picture of Tzotzil culture but an interpretive portrait of the interaction between a particular theoretical practice and an alien culture, and the effective consequences for both. I shall return to this later. For the moment I suggest that the organisation of the text displays an evolution from the material to the abstract (the most abstract appearing as general analytic principles and historical projections); from the determinate and determining environmental necessity to luxurious speculation (as in 'anthropological discourse'); and from nature to culture (where cultural expression is again more pronounced in bourgeois practices). This evolution further discloses the oppositions implicit within the text between the Tzotzil and American bourgeois culture, pronouncing the latter in a position of superiority and domination, being a product of successive historical adaptations.

From explicit and implicit sources it appears that the Harvard scholars hold to an ecological model, imbued with determinate efficacy, at least in the first instances of a society's life and during its most elementary periods of subsistence before it achieves efficient adaptation of its social organisation. From the time that it develops surplus resources from the land under its exploitation, its base will have undergone sufficient development to allow the elaboration and diversification of its internal institutions, collective representations, and divine repertoire. These will, however, always bear some relation to their ecological circumstances, even if not direct, and again at the primordial level the assumption would seem to be that the ideological or religious aspirations are directly influenced by the environment. The idea that they are mediated by forms of social organization does not come until later, when certain religious and ritual observances are thought as expressions of divine disfavour towards transgressions of normative behaviour.

We shall elaborate further on the theoretical practices of these authors as we widen the discussion, but our principal interest is in tracing the effects of this naturalised kind of discourse in its presentation and understanding of Tzotzil culture, and its implications for Western civilization in general.

Notable throughout the works of these authors is the emphasis placed on descriptive ethnography and on economy of interpretation. At the beginning of *Zinacantan* Vogt (1969) writes that it is a descriptive account and whatever interpretation it comes to disclose is incidental and unavoidable. Again in Collier's work the emphasis is placed on the description of patterns of land-use from the possibilities inherent in the environmental backdrop. He concentrates on physical features ignoring almost anything that is cultural. It is a curious feature of his study (Collier 1975) that despite his concern with demonstrating the adaptive and functional fit between kin-

ship organisation and patterns of resource availability, he fails to give account of the system of kinship itself. Everything is from the point of view of the environment; all he has told us about kinship is that succession is tied to descent and the solidarity of the kin group is a function of its efficiency and ability in allocating scarce resources. This is perhaps among the most impoverished reductions of kinship existing in the ethnographic literature and it tells us nothing of the particular form and relational system characteristic of Tzotzil culture. Clearly, the problem in proceeding with the analysis of kinship in this vein is that the environmental pre-conditions or other natural facts have little or no further heuristic utility in disclosing the nature of particular relations and behavioural patterns within the family group. On the other hand, structural, semantic, and social anthropology (i.e. those approaches that begin with culture), take consideration of Collier's conclusions at the very beginning, and embrace the finer and elaborate particularities of kinship within a wider system of symbolic classifications and causalities. Here, however, the emphasis is on descriptive intangibles rather than physical and material manifestations. Collier feels at ease only in describing the obvious: that part of nature which presents itself to our perception and can therefore be transcribed textually in the terms of its ordinary presentation. This he presumes is the only way of avoiding ideological or subjective interpretations to intervene in the description of alien social systems. In other words the text must parallel the phenomena under consideration at the most elementary, closest, and most obvious level, so it itself can always be questioned by reference to the object-relations which it signifies. This same line of reasoning I suspect to be the *modus operandi* of an earlier paper by Collier, *Categorías del color en Zinacantan* (1966), which summarises the results of perceptual tests on a sample of Tzotzils. From this he summarises relations between the range of their colour perceptions and linguistic classifications as compared with North Americans. Between the two ethnic groups there are marked differences in the names they give to abstract colours, but similarities exist as well demonstrating basic cognitive correspondences. In this paper Collier again describes natural phenomena and explores the relation between them and linguistic categories through 'controlled' experiments. He finds that even though linguistic classification can vary between cultures, they remain fundamental as encodings of naturally occurring phenomena and processes. The particular view of language hinted at here will become important later on in the discussion.

As further examples of the importance given to description, we can point to the work of Silver (1966) on shamanism and Cancian (1965) on the religious cargo system of Zinacantan. Both authors exhibit the same caution in stipulating relations other than those at the most superficial and apparent level. Thus the greater part of Cancian's work is a description of the organisation and the attributes of the religious cargo system and the

behaviour of its incumbents. Thus, for these authors a descriptive chronicle seems to be considered as the best way in attaining scientific documentation while minimizing individual bias and subjective values. From this body of data relations can be abstracted that are self-evident functions, and thus the mechanism of the society derived. In keeping with the economy of interpretation, relations governing the structural ensemble are basic, necessary and often elementary and self-explanatory. In all cases they are conceived as the means of survival for that society. This of course brings us at last to that old and faithful ghost of ethnographic interpretation, functionalism.

In no instance have we begun with categories and positions common in the interpretive classifications of ethnographic literature. Rather we have begun with a corpus of work and located within it certain propositions that indicate its methodological orientation. Ecological determinism, materialism, and functionalism can assume many forms. We have wanted to discuss only one manifestation of these and so trace their particular effects in this context. A brief discussion of the functionalism of these authors is necessary precisely because functionalism outside of its theoretical practice is nothing but a formal category which has assumed more than enough ideological connotations to make it meaninglessly abstract.

Functional utility begins with nature. We have seen how the form of social organisation, kinship and settlement patterns must follow the rationale of its ecological setting to adapt efficiently. Once the initial adaptation has been accomplished successfully the society can undergo development of its internal institutions. It is assumed throughout the texts of these authors on the Tzotzil that subsequent development and diversification proceed to widen the ecological base of their subsistence and to reconcile contradictions and anomalies pre-existing within institutions, which are expressed through dysfunctional forms of behaviour. Relations between institutions, which are extrapolated as the essential mechanisms maintaining the society, are conceived as functionally adapted reinforcements and counterbalances to areas of stress. Thus Cancian considers the functions of the religious cargo system as defining the limits of community membership by participation, reinforcement of commitment to common values, reduction of potential conflict and support of traditional kinship patterns. In *Zinacanteco Divination* (1974) Vogt sees shamanistic healing ceremonies as being expressions of the public redressing of socially generated conflicts between individuals. According to Vogt, the shaman '...is saying that the person has been socially disruptive and requires re-socialization by the lengthy procedures that are incorporated in a Zinacanteco ritual of affliction' (1974:205).

The entirety of Tzotzil is reduced to mutually reinforcing adaptive mechanisms following a natural logic independent of themselves and man. Vogt taints this functionalist conception with his theory of replication, but in keeping with the tradition of this work replication is apprehended only at the behavioural

and organisational levels. The linguistic affiliation of words indicates the cases of replication, which are seen at a behavioural level; but again language is understood as nothing but an encodation of experience and is not credited with applying a logic of its own to the situation. Vogt's theory of replication seems to be quite shallow and does nothing more than state thematic and organisational correspondences as they occur throughout the culture. The question he asks through this is, astonishingly, 'what themes are repeated in rituals that are to be found elsewhere in the culture?' (Vogt, 1965:385). He does indeed find correspondences, notably the idea of 'embracing' which occurs at a number of different levels, and the replication of the organisation of the social and cultural levels which show parallel development towards increasing size and complexity. I have set the two parallel systems out side by side so that relations become clear.

| Social Organisation   | Ceremonial Organisation   |
|---|---|
| a) Patrilocal extended family occupying a house compound.                                 | Ceremonies involving the family and no more than one <i>h'ilol</i> (shaman), e.g. curing ceremonies, new house dedication ceremonies. |
| b) The <i>sna</i> composed of one of more localised patrilineages genealogically related. | <i>K'in krus</i> ceremonies twice a year that involve all the families and <i>h'ilolét</i> who live in the <i>sna</i> .               |
| c) Water-hole groups composed of two or more <i>snas</i> .                                | <i>K'in krus</i> ceremonies performed semi-annually, preceding those above.   |
| d) <i>Paraje</i> composed of one or more water-hole groups.                               | Two ritual ceremonies performed at the beginning and end of the year, by all <i>h'ilolétik</i> of the <i>paraje</i> .                 |

With this information, however, all Vogt accomplishes is to note that the ceremonial order expresses and reinforces the social order. The *k'in krus* ceremonies at the level of the *sna* compound serve as symbolic expressions of rights of land ownership. The *k'in krus* ceremonies of the water-hole group express rights and obligations to the water-hole from which they derive their water. Finally, the ritual ceremonies of the *paraje* are a collective affair which symbolize solidarity. One is puzzled as to the nature of the 'theory of replication' since it does not

claim to disclose organisational replications (as these are already apparent at the morphological level to which most of these writers restrict themselves). However, what is clear under this label, whatever its worth, is that Vogt finds functional relations following rules of an identical nature. Replication is nothing other than a statement of observed coincidence between institutions, implying no explanation or relations other than those of the functionalist model.

A substantial theoretical practice has been isolated from the writings of these authors who presumed neutrality. Their empiricism, an ill-conceived and passively accepted standard of scientific enquiry, effected a substitution of explanation by description. They assumed that a division of the society under investigation into individual units and the assignment of a 'social scientist' to report on each would lead to deeper insights and a greater understanding of the society as a whole. The resulting specialisation should not be underrated. The study undertaken by an academic institution profited by dividing the society among its post-graduate students who were working towards their professional qualifications. Not surprisingly, as will be demonstrated, the work that came out of this practice told us more about the organisation and pre-suppositions of Harvard anthropology than it did about the Tzotzils.

Beginning with the empirical assumption on the possibility of the neutrality of discourse, we shall make explicit the effects of these arguments on Tzotzil culture, and the consequences they hold for our own. Language can never be neutral in an absolute sense, since it exists and presents itself to us already constituted. In this the language itself has a history of development through which it has evolved together with its connotative aspects. Etymological analysis teaches just this; the differentiation and specialisation of a language, the accumulated nuances and meanings that retain their quality form part of the sense of a word, even after its original significance has long gone and no appropriate context exists for the use of the word in any original sense. Language is not a static representational system from which we can choose constituent elements and combine them to form new meanings and expressions with perfect liberty. It follows rules and traditions as well as embodying a culturally constituted logic of classification through which it expresses relationships between objects and classes. On a different level we have a further ideological formation which consists in the incidental connotations of words, the nuances which they come to possess in association, and the values that these represent and which are most apparent in the written text. Language in general, as in this particular case textual discourse, can never be assumed to be the neutral medium of expression which empiricism so desires. For Vogt, Collier, and their colleagues, their texts express real things, processes, and relations external to them because they are amenable to their senses, and the same can be recorded because they can be recreated in their relational positions through accurate and faithful description. The 'reality' of the text is no more

in doubt than that of their senses, and the sense of both is common. One cannot help but recall the most fundamental dictum of inquiry, that the 'signifier' and the 'signified' do not get confused. This is precisely what has happened throughout the texts of these authors. They have equated the two. Just as Vogt has perceived a replication between social organisation and the ceremonial order in Zinacantan, he also has assumed a correspondence between language and that which it comes to designate in his own method. This is an interesting correspondence because we can assume the same process is at work in both cases. Vogt reduces the ceremonial order to simple symbolic expressions of social organisation, in the same way as linguistic denominations are made expressions of external phenomena. In both cases the logic is not seen as already constituted either within Zinacantan ritual language or within his own textual discourse, but as external and having a common source. The logic they conceive is natural. It is a primary characteristic and necessity for any discourse, and resides outside that discourse itself. The neutrality and, incidentally, the worth of the discourse is dependent on its correspondence to an object reality outside of itself. If it clearly lacks any semblance then, and only then, is the presence of an ideological mask suspected and the discourse suspended in favour of another.

Returning to the problematic of logic, we can suppose that for these authors the same logic is the foundation of all meaningful discourse and the basis of all communicative systems, regardless of cultural origin and expression (i.e. underlying different languages and between language and other symbolic representations). This logic would then be transcultural and transcendental. Implicit in this formulation is that language serves to verbalise and articulate external relations which must be grasped if a population is to adapt and survive the enmity of nature. This, of course, was Malinowski's view of both culture and language, and he succeeded in proving the rationality of different indigenous societies only by reducing them to the terms of his own discourse. Vogt *et al.* adopt the same assumptions and produce identical effects.

Following the assumption that logic is an attribute characteristic of human thought and classification Vogt *et al.* begin to disassemble Tzotzil culture at the very inception of their project. The rigour of their dissection is best illustrated by the titles and subject-matter of the doctoral theses and published books which the project begat. The list which follows was obtained from Vogt's record of manuscripts and publications of the Harvard Chiapas Project included in his 1969 publication, which is not exhaustive, and includes: *Childbirth in Zinacantan* ; *Economic Role of Women in San Felipe* ; *Infant Development in the Zinacanteco Indians of Southern Mexico* ; *Color Categories in Zinacantan* ; *The Economics of Divorce and Re-marriage in Zinacantan* ; *Women in Ritual in Zinacantan* ; *Ritual Objects, Their Pre-Fiesta Logistics* ; *Clothing Norms in Zinacantan and Chamula* ; *Tzotzil Ethno-anatomy* ; etc. The categories that they reflect are surely

distinct from any logic, natural or other, except that which characterises Western social sciences. The participants of this project have produced a massive fragmentation of even the traditional divisions within the discipline of cultural ethnography. By projecting these miniscule areas, each one as the domain of their legitimate study on Tzotzil culture, they have created a barbarous disfiguration of indigenous categories which they have simply ignored. Even though each investigator has produced a 'little theory', in Cancian's words, to account for his own slice of the Tzotzil, it is remarkable how alike they all are. The only way of explaining this is by the common theoretical practice which they follow and its constituent and culturally based logic that they deny. The logical foundations of Tzotzil classifications are presumed before the ethnographic work is even begun, and indigenous categories are subsumed and identified with the logic of the theoretical practice itself. Any translation between one society and another, despite wide differences, is a technical problem and not one of semantics. It can be assisted by the observation of behaviour and organisation of activities which are concrete exemplifications of the work of the logic contained in the language, and which accordingly are clarificatory. Ritual, religious, or symbolic representations also represent expressions of basic relations and serve to strengthen the solidarity and adaptive capacity of the institutions to which they are related. They obey the same logic insofar as they have adapted to local circumstances, clearly seen to be the case once the anthropologist has penetrated the 'forest of symbols' and ritual expressions which convert the inner-most statements of necessary relations and observances.

Vogt perhaps demonstrates this well in his paper on *Zinacanteco Divination* 1974 which we have already mentioned. Here the rites and incantations practised by the shaman in a curing ceremony are thought to be outward expressions of a deeper function which is ideologically presented through the ritual medium. The causes of illness, although phrased as 'soul loss' inflicted as punishment by the ancestral gods or witchcraft (Silver also mentions natural causes as a third source in *Enfermedad y Curacion*, 1966), is seen either as a castigation for anti-social behaviour as in the first case (Vogt 1974), or an expression of tension between kin and resultant accusations of malice (Silver 1966). In both instances the function of the shaman is to arbitrate and redress the situation judicially. In explaining what he calls the ambiguous nature of the shaman's performance, Vogt reveals his bias to his own disadvantage:

...since his final decision in the diagnosis is based neither upon the sophisticated technology nor the tested principles of modern science and since he does not possess the legal power of a judge backed by a police force, and the threat of fines or a jail sentence, it is crucial that his divinatory procedures have an ambiguity about them which allows him a measure of latitude in his decision and

reinforce his judgement with supernatural sanctions.  
(Vogt 1974:204)

So does Vogt dispose of ritual behaviour and symbolic associations if they are inexplicable to his understanding. 'Culture' is derided throughout in favour of natural logic.

Since it is our opinion that a crucial and important part of any viable anthropology is the study and elicitation of indigenous symbolic classificatory systems, we shall examine the characteristics of that which presents itself in Vogt's theoretical practice.

Most clearly it is a logic of adaptive response. The social organisation of the culture must adapt to the local environment in an efficient manner, and the ideological representations must ideally express these relations and serve to ease tensions where these are produced in the process through adaptive responses. The whole structural *ensemble* of the society is rationally ordered, mutually complementary and self-adjustable to the pre-conditions to which it must adapt. What motivates the positive responses to environmental necessities is the instinct of survival. In the first instance it is the primordial instinct of 'populations' to maintain themselves at whatever cost, thus they mould their institutions accordingly so as to guarantee their subsistence within a given ecological niche. At a more abstract level the survival imperative is built into their social institutions. Its guiding logic works towards harmonising and complementing these institutions and governs the relations between them. The logic of relations, which it is man's fortune to grasp, is therefore utilitarian. It is self-interest necessary for his survival and reproduction, these same principles being at the foundation of both his own actions and those of the organisations which compose his society. Here is the re-vindication of utilitarianism; that man's over-riding concern is with his own survival and that his actions are directed by this most basic of instincts, therefore reflect a particular rationality formed by his perception of the logical relations existing between himself and the environment in which he is situated. Again, not how language is naturalised in this conceptualisation becoming nothing more than the coherent articulation of already existent natural relations.

This view of humanity implies a familiar nature. It is a charter for aggression, assertion and individuality: aggression to defend oneself from a savage and threatening nature; assertion in its utilisation and defence; individuality because this is accomplished for one's own survival or that of the immediate family group. In fact all the values that are most endeared to the bourgeois imagination we find imputed to other indigenous cultures, perhaps not surprising since the methodological tenets presume that societies share a common heritage. The Hobbesian conception of man thus comes to take its place alongside the doctrine of utilitarianism and the theory of adaptation and survival of the fittest as begat by Darwin and Huxley. Finally,

given this particular view of human nature, rationalised according to the laws of a daunting logic, emerge the functions of social institutions once they have evolved from the primordial state. These very values that are said to be necessary in ensuring man's subsistence are potentially disruptive at the social level. The individual values implied by this view of man must be assumed by the social institutions themselves in promoting a collective response to the problems of human existence and survival. So it passes that social institutions emerge to soften and diminish the basic attributes of individual human nature to make society possible. They continue to function as a system of checks and balances guaranteeing the survival of the group through their physical and moral restraints on the individual. For Vogt this is precisely the function he attributes to the role of the shaman within Tzotzil society, and Cancian, writing of the religious cargo system of the same community, observes that it allows the expression of basic values inherent in human nature through socially prescribed roles and behaviour. He writes: 'It stipulates the rules under which a man may enhance his public image, and thus helps to minimise potentially disruptive innovation and competition'. (Cancian 1965:135)

We have traced a set of presuppositions which have their origin in the very basis of bourgeois society. They constitute the charter of its morality and values and justify its preoccupations. They present themselves as rationally derived natural propensities which form the very foundation of human nature, and from this claim their universality. Presumed, in the very construction of the theoretical practice which masquerades as neutral, they are found time again in alien cultural traditions, thus justifying methodological utility and the consequences this holds in our perception of bourgeois society. The rationality of the bourgeois imagination is one based on nature, and through the extension of its ideological modes of discourse it is attributed to other cultures; thus uniting the world according to tried and tested bourgeois precepts, and placing its cultural achievements at the pinnacle. The ethnographic work of Vogt *et al.* is an exemplification above all of how a methodology, imputed with scientific pretensions, serves as an ideology, transforming whatever it touches into the parallel experiences and values of those of its own society, in the process of which it justifies its own existence while leaving only a charred remain of the object of its scrutiny. I believe this is no exaggeration given the effects of this discourse on the Tzotzil and the reduction or destruction of their cultural concepts and indigenous logical categories. The example of Vogt's treatment of shamanism has already been cited. Collier by phrasing his investigation of land in Western terms has produced nothing but a technical account of land usage as viewed by him. The tendencies he finds inherent in the process he describes are projected as a social forecast of successive changes given the continuation of population growth and resulting pressure on land. In economic forecasts it is usual to interject that certain conditions are accepted as 'givens', so too with Collier and Cancian; but

what they accept as given is the rationality and the resulting picture that it gives of the specific cultural logic at work. Basically, that which is 'given' is unknown or at best mediated by a bourgeois ideology, while whatever resulting predictions are effected are untrustworthy. A notable consequence of this method is to circumvent whatever is culturally unique. In the case of Collier, we are never told how the Tzotzil visualise, regard and value their land, though we glean from scattered comments made by other authors that there is an indigenous conception of sacred geography where mountains, caves, and water-holes are thought as sacred and are associated with particular beliefs and supernatural persons. Libations are made to the fields during cultivation, and differential values are ascribed to different kinds of land and natural phenomena. One cannot help wondering if these are all part of an involved symbolic classification which perhaps might regard the environment as a metaphor of religious expression as is the case among the Huicholes of northwestern Mexico. In similar fashion, the discussions on Tzotzil architecture have been limited, with minor exceptions, to technical accounts. Vogt (1969) describes their appearance, means of construction, durability and cost, while Warfield's (1966) account can be no liberal stretch of the imagination be seen as having any anthropological inspiration. Nevertheless the values and symbolic associations of architectural practices are of importance, just as those are of the land, particularly if external agencies govern development in indigenous areas (and it seems that these reports are meant only for technical assistance to such agents). In both instances anthropological practice has not been to articulate specific cultural sensitivities but to undermine them. Again, despite scattered references to linguistic oppositions between words used to designate the space occupied by houses and the space dominated by wilderness, house-dedication ceremonies and even evidence that the names of parts of the house are coincidental with those used to designate parts of the human body and the sacred mountains (Vogt 1969) has not been pursued.

Obviously, despite its repression, the indigenous logic of classification continues to attempt its emancipation from the ideological discourse to which it has been reduced. This is not only evident in the examples we have quoted but it has been fully recognised in another series of texts written by different members of the same project. Blaffer's *The Black Man of Zinacantan* (1972), Gossen's study of *Time and Space in Chamula Oral Tradition* (1977) and the various publications of Robert Laughlin (e.g. 'El simbolo de la flor en la religión de Zinacantan', 1962), indicate how, through their careful explications of other parts of culture, the other members of the project have distorted indigenous classifications. Vogt himself apparently has come to understand the impoverishing effects of his theoretical practice on the Tzotzil and has since written a second monograph, *Tortillas for the Gods* (1978) which deals more fully with indigenous classificatory principles.

Concluding on a positive note of anticipation, the

ethnography of the Tzotzil still awaits to be written and in his venture the investigator will find rich information derived from some of the work of the Harvard Chiapas Project, but much of it will perhaps not be of use until supplemented by further fieldwork of a better-informed nature. Until that time the Tzotzil carry their cultural disinheritance heavily upon them in the eyes of others.

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## BOOK REVIEWS

MICHAEL HERZFELD, *Ours Once More: Folklore, Ideology, and the Making of Modern Greece*, Austin: University of Texas Press 1982. xii, 144pp., Map, Appendices, Bibliography, Index. \$17.50.

Ethnicity, ethnic identity, national identity, nationalism - this cluster of terms marks out a field which is becoming of increasing interest to many anthropologists, especially Europeanists, perhaps because it lies at the crossroads of a variety of quite divergent anthropological approaches and concerns. For those sometimes miscast as 'idealists' (i.e. whose interests are with the ideas, notions, concepts, categories which represent, inform, and recreate the social) the concept of ethnicity, whose empirical referents are so notoriously hard to pin down, presents a perfect occasion for the exploration and demonstration of the social construction of social reality and of the entities which it calls into play. For those whose demand is a break with the ethnographic description of small-scale communities and 'partial' societies in favour of a confrontation with the broader economic, social and political histories of the nation-states in which such communities are contained, the study of nationalism seems designed to fit the bill. And for those whose scholarship must intervene in the world of contemporary politics and events, an engagement with the aspirations of emergent (or suppressed) 'nations' plunges them, verbally, into the thick of things. Nationalism is with us, and its standard-bearer is 'culture': the Scots, Welsh, Irish, Bretons, Basques, Occitans, Corsicans are having their day, and the weapon can be a folk-weave blanket as easily as a bomb. It is curious that the study of ethnicity and nationalism in Greece has received so little attention from anthropologists, for there their combination has been, against the odds, triumphant and enduring. We owe to Michael Herzfeld the partial rectification of this omission, and his approach (he calls it 'semiotic') is a historical and systematic analysis of the ideology of Greece's folklorists who, from the War of Independence, have contributed to the making of modern Greece.

A few weeks ago a perfectly respectable centre-left newspaper (*Ethnos*) included the following remarks in its editorial:

In this land of ours, throughout the centuries, the foundations of Democracy were firmly laid by people determined to establish the right of the majority to manage freely their thought and activity. For the

first time in the 5th century B.C., Democracy shone and the basic principles governing society were established. Those principles were: equality of rights, equality of political rights, equality of speech and freedom of speech. Specifically equality of political rights meant non-division of people to privileged and non-privileged classes; equality of rights meant that all people were equal before the law; equality of speech meant that all people had the right to speak and no government could take that right away; freedom of speech meant freedom of the press which was an irreplaceable instrument of Democracy ... These four basic principles which have remained unchanged throughout the centuries should govern today all democratic regimes.

One might pass over a few factual errors. If it is 5th-century Athens which is being talked about, then a substantial proportion of the population (perhaps nearly half) were slaves who had no rights at all, who were defined, in fact, as not human; another substantial proportion of the population were resident aliens and their descendants, who were protected, but who had no political rights; and then there were women (and children) who equally were defined as legally and politically incompetent. This leaves perhaps 20% of the total population to exercise their equality in what was a rigidly divided society. But more interesting than these quibbles is the total collapse effected by this passage of both time and space. Democratic Athens was one city (albeit an extremely important one) amongst hundreds of autonomous cities, whose constitutions were as often oligarchic as 'democratic'. Actually, the editorial does not mention Athens. It talks of 'this land of ours'; for, ideologically, the nation state of Modern Greece, which did not exist before 1832, is projected backwards in time and selectively collapsed with the particular city-state whose fame is universal. It is this spatial collapse, or at least indeterminacy, which also allows the statements that democracy has existed 'throughout the centuries' and that its basic principles have 'remained unchanged throughout the centuries'. The implication is that we are talking about Greece where, in the 5th century, in Athens, these principles were founded. But is it meant that they actively persisted throughout the Macedonian Empire, the Roman Empire, the Byzantine Empire, the Ottoman Empire, or indeed the recent Greek monarchy or the Colonels' junta? These questions are side-stepped by a shift in focus from the *institutions* of classical Greece (Athens?) to a set of moral principles, of *values*, which, it could be argued, might have continued to exist 'in this land of ours' regardless of whatever were the actual political realities of the successive stages of Greece's history; but they are also side-stepped by a shift in focus, or rather a blurring of focus, which merges with surprising immediacy classical Athens (Greece?) with 'all democratic regimes' today, such that, through the medium of these values, classical

Athens becomes not only the modern Greek nation state, but also free democratic Europe.

*Ethnos'* editorial is not a piece of Machiavellian propaganda (nor are its aims anything but laudable). Rather, the successive collapses of an idealized classical past with the modern Greek state and of the eternal values of 'Hellenism' with European civilization as a whole, is a quite innocent habit of mind. How this was achieved, and why it had to be achieved, is a long story. Herzfeld's book tells one fascinating part.

The Greeks rose in revolt against Turkish rule in 1821. But as Herzfeld says:

Their goal was far more ambitious than freedom alone, for they proclaimed the resurrection of an ancient vision in which liberty was but a single component. That vision was Hellas - the achievements of the ancient Greeks in knowledge, morality, and art, summed up in one evocative word. What was more, the new Greek revolutionaries went one step further than their forbears had ever managed to do: they proposed to embody their entire vision in a unified, independent polity. This unique nation-state would represent the ultimate achievement of the Hellenic ideal and, as such, would lead all Europe to the highest levels of culture yet known.

The problem was that this vision was shared by a handful of Greek intellectuals and revolutionary leaders on the one hand, and by foreign classically trained Philhellenes on the other. Whether it was subscribed to, or understood by, or worse yet, at all evidenced in the bulk of the Greek population, the *laos*, who, united by their common Orthodox faith and their hatred for the Muslim Turks, fought the war, is rather another matter. It was not one, however, which could be left unsettled.

As Herzfeld stresses, there are two Greek views of Greece: one outwardly directed and constructed from the accumulated materials of European classical scholarship; the other inward-looking, intensely self-critical, and uncomfortably aware of Greece as being on the margins of Europe. These two views are contradictory, but they are not always mutually exclusive. Which is expressed is often a question of context and company. But Greece's pride in its nationhood, literally its place in the world, and, importantly, the early support it enjoyed as a fledgling nation, perforce involved the cultivation of the first image. In post-Renaissance Europe, substantially cut off from the realities of a Greece under Ottoman domination, a vision of Hellas had grown. In a sense, this vision was sold back to the Greeks. Nor could they afford to refuse to buy; but the price was a demonstration that they were in any case its natural heirs. Cultural continuity was an imperative.

But cultural continuity, as Herzfeld rightly states, cannot usefully be regarded as a question of pure fact. At the least,

it requires a screening, a judgement, and an assembly of what, from the present context, is to be deemed relevant. It is on this enterprise which Greece's folklorists embarked: a study which was, in its own terms, both 'archaeological and patriotic'. And Herzfeld traces with admirable clarity and scholarship the development and intricacies of their thought from the early philologists and historians to the great Politis, who, in 1884, finally gave to the discipline the name by which it has ever since been known in Greece: '*Laografia*', the study of the *laos*, the 'people'.

This 'patriotic and archaeological' enterprise was not, however, without its internal dissidents and external challengers. The two views of Greece, already mentioned, produced two strains within Greek folklore studies. The first attempted to discover through the songs, folklore and customs of Modern Greece the substance of Ancient Hellas itself, even if that meant decrying and carefully removing the patina of two thousand years of 'foreign' influence and subjugation. The second strain, more liberal, less elitist, rather than advocating a purification of culture (and language), desired to see in the customs and manners of Greece as it actually was a continuity of the same Hellenic genius which had inspired the glories of the past. Both, however, were equally patriotic, and faced a common enemy in the person of the Austrian historian and polemicist, Jakob Philipp Fallmerayer, whose name is still anathema in Greece. For Fallmerayer, from 1830 onwards, in an argument based partly on linguistic and partly on historical evidence, proposed that in the course of the Byzantine period the original population of Greece had been entirely over-run and that Greece's present population was not Greek at all, but Albanian and Slav. The reaction was furious, and Fallmerayer was probably single-handedly responsible for the proliferation of Greek folklore studies.

Fallmerayer's attack also provoked counter-arguments which shared the same premise: the identity of 'race' and 'culture' (a common enough conflation at the time). Greece's folklorists turned to the definition and discovery of 'national consciousness' and 'national character' - a character which was quintessentially 'European' - 'virile' as opposed to the degenerate Asiatic, 'individualist' as opposed to the 'communistic' Slav. But if the search for 'national character' was common in the Europe of the time, the repatriation of Philhellenism to Greece itself created a particular oddity, which Herzfeld nicely labels 'ecumenical ethnocentrism'. Greece was more European than Europe (and Fallmerayer was called a Slav): 'If renascent Greece represented the ultimate vindication and triumph of European culture, who but the Greeks should be best qualified to interpret the larger entity? This ... reverses the broadly phenomenological subjectivism of European philosophers by granting the Greeks an intuitive understanding of culture at large.'

Herzfeld's book, as he states himself, is essentially 'an anthropology of anthropologists'; but, in my opinion, its importance is much greater than any study of a group of mere academics and

intellectuals. In many respects it resembles Chapman's excellent *The Gaelic Vision in Scottish Culture*. But if Scottish nationalism and Scottish culture seem only now about to reap the rewards of a limited political success, Greek nationalism and culture followed in the wake of political independence and the establishment of a nation state. Their triumph has been genuinely to bring into being out of remarkably diverse material and within what was a remarkable diverse country a cohesive sense of identity based on the appropriation of the past. *Laographia* as an academic discipline in Greece has changed and is changing; but the legacy of its early years is now part of everyday life.

ROGER JUST

JEANNE FAVRET-SAADA, *Deadly Words: Witchcraft in the Bocage* (trans. C.Cullen), Cambridge etc.: Cambridge University Press and Paris: Maison des Sciences de l'Homme 1980. vi, 273pp., Index, Diagrams. £5.95.

This is an interesting, sophisticated and intense book. It is also in some ways a very good book. The 'Bocage' referred to in the title is the 'countryside of Western France marked by intermingling patches of woodland and heath, small fields, tall hedges and orchards'; the exact location is never revealed, and nothing so empirically vulgar as a map is provided, but we seem to be in or close to the Department of Orne, in the south of Normandy. *Deadly Words* is a book about witchcraft in this area, as experienced by the ethnographer, Mme Favret-Saada, between 1969 and 1975.

The work is an attempt to deal seriously and credibly with a phenomenon, and a discourse, that are for various reasons well insulated from positivist enquiry, or from the simple question-and-answer techniques that are often the first bonds between ethnographer and informant. Witchcraft in the *bocage* arises as an explanation for a string of misfortunes that seem grouped in such a way that 'chance' can be ruled out. The misfortunes cited are those that we might expect - dried-up cows, non-laying hens, death and disease among livestock and personnel, and so on. The discourse of witchcraft is entered when an individual decides that he is bewitched. This decision is not taken entirely spontaneously or independently, but is rather provoked, in the most oblique of ways, by a friend or acquaintance here called the 'annunciator', who asks some question like 'do you think someone is doing it?' From there, the bewitched can begin to formulate suspicions, according to local circumstances and jealousies. Those who believe

themselves to be bewitched can react with a series of defensive measures, designed to seal themselves off from magical attack (avoidance of social, body or eye contact with the presumed witch, liberal sprinkling of holy salt or holy water at strategic places round the farm, and so on). If, however, the defence does not work, if the misfortunes continue, then an 'unwitcher' is called. The unwitcher is an acknowledged expert who identifies the witch, suggests defensive measures that have not been taken, and, in cases of dire need (impending death of the bewitched, for example), goes onto the offensive, that is, 'returns evil for evil', attacks the witch with a force stronger than his own, and thus forces him into retreat or death. All this is done covertly, and at a distance; there is no open accusation, and overt strife is avoided. The only signs of the success of the unwitcher are the distress of the presumed witch (should any such distress be perceptible), and the recovery of the bewitched.

There are no self-avowed, self-confessed witches in the Normandy *bocage* (or, if there are, this book is not about them). There are only those who think themselves bewitched, their unwitchers, and those who are accused by the bewitched and unwitcher of witchcraft. Since accusation is never publicly made, the accused play little part in the discourse of witchcraft, except unwittingly to show signs that can be interpreted as weakness or defeat. The unwitcher is the one that, for financial gain or for reputation, attacks with magic, and it is she (or less often, it seems, he) who orchestrates and induces many of the events of bewitchment and cure. In any external judgement of the events, it might seem reasonable to call the unwitcher 'a witch', and this is indeed what frequently happens whenever witchcraft events are reported in the local press; internally, however, the unwitcher is not a witch, but his contrary, forced nevertheless to fight the witch on his own ground. In a world where there are bewitched but no witches, anyone who knows him/herself, by implication or inference, to be the target of unwitching, will regard this as witchcraft, and treat it accordingly; the author seems to suggest, however, that in order to deny any imputation of witchcraft (that is, in order to deny that one could ever be the legitimate target of unwitching activity), one looks elsewhere than the unwitcher, or than the bewitched on whose behalf s/he is acting, for a witch to accuse. And so, backwards and sideways, the logic of accusation traces its rather unpleasant path.

That is to put it very briefly, and the author would probably deprecate such bluntness; I felt, however, that some summary was necessary before I went on to praise and criticism. First, then, the praise. It is clear that witchcraft in the *bocage* is an extremely difficult phenomenon to investigate, and equally clear that the author of this book has pursued her fieldwork with an admirable insight and tenacity. She argues, convincingly, that there is no place in the discourse of witchcraft for neutral information, of the type that the ethnographer might wish to gather and take home. Witchcraft announces itself, and discusses itself, in a discourse of allusion, or even of silence, and information

is only divulged to those that are assumed to be prepared to take an active place themselves in the witchcraft process. The ethnographer describes how she was drawn, in spite of herself, and rather by accident, into the fringe of the witchcraft process, and how it was only thus that she was able properly to investigate it. Combining the roles of bewitcher/unwitcher/witch (for she seems to have been on the verge of taking up all of these statuses), and of ethnographic reporter, was plainly not easy, and Mme Favret-Saada's account of the moral and intellectual strains imposed on her is eloquent. The local press, eager for supernatural titillation, called her 'the witch of the CNRS'. The peasants wanted to pay her for unwitching services that she did not at first know she had even performed. Her colleagues in the CNRS wanted to know if what she was doing was science.

The peasant of modern France lives, like ourselves, in a theoretical universe where witchcraft has only a very suspect status. The law, the medical profession, the social services, the church, academic folklore studies and everyday common sense all, in different ways, deny it the possibility of existence. In many studies of folklore, witchcraft is treated as a survival, a relic from a more primitive logic, whose phenomena exist not as a system, but as a random collection of curiosities (charms, spells and so on). As such, the science of folklore is quite rightly criticised here for its elementary failure to take seriously what the people under investigation take seriously, and to place the items that it 'collects' in the context in which they can be understood. The medical and psychiatric professions are also criticised here for the way in which they deal with witchcraft. Indeed, the approach of these two professions to witchcraft is, at least as described here, almost hilarious, for it consists largely of the application of empirically immune and semi-superstitious categories such as 'psychosis' and 'collective delirium'. The irony of the juxtaposition of this hermetic ideological discourse with that of witchcraft, equally hermetic, equally ideological, is not lost on Mme Favret-Saada, although she has a lot less fun with it than she might. The attitude of the church to witchcraft is more equivocal, but in general the priests seem to find that the diocesan authorities are happier if cases of presumed witchcraft are referred to the correct positivist authorities (doctor, vet, psychiatrist and so on). It is also interesting that the people in the very middle of the witchcraft events, the villagers themselves, will also invariably argue, at least on first asking, that witchcraft is only something that 'used to be done', or that only 'backward or demented' people would believe in, or that it is only believed in elsewhere. Faced with this stark positivist denial at the centre of its field of operations, it is not surprising that witchcraft is allusive and secretive, and that its investigation would pose an ethnographic problem. We are shown, in this book, through a few very detailed case histories, and through minute nuances of interpretation and utterance, how a series of witchcraft events is generated; we are shown how those who, believing themselves to be 'locked in' to

impotence and disaster through the spells of the witch, come to be locked in to the discourse of witchcraft, and to become at once its manipulators and its victims, with often unpleasant consequences.

It has long been a problem for folklorists, and more recently for anthropologists, that many of the folkloric phenomena that they study seem to be on the point of disappearance, but can yet remain at this point of disappearance for generations. In what I found to be one of the most satisfying insights in the book, Mme Favret-Saada says:

Folklorists might ask themselves what their informants mean when they talk in the past: 'in the old days' people believed in spells and said that...etc. Is this not because the unwitcher - and not the folklorist, for example - is the only person to whom the bewitched can talk in the present? The folklorist takes this use of the past as the basis for firm statements about the forthcoming disappearance of witchcraft (and this has been going on for more than a century) without thinking that a *discourse* on the past is perhaps not quite the same as a past *event* [p.6, note 1, original emphasis].

This comes in a footnote, and as such is not untypical. The text has an extensive apparatus of footnotes (mercifully easy to consult) and appendices, without which it would at times be almost unintelligible. Which brings me to the criticism.

This work is not particularly easy to read. The translation seems to be good, but for various reasons the text is sometimes both elusive and indigestible. Firstly, it is written in a style that has become common enough in post-structural intellectual France, but whose rhetorical flourishes and daring denials of positivist authority tend to seem merely self-indulgent when translated into English (although they have their English imitators, to be sure). Secondly, because the work is a systematic criticism of the 'positivist' approach to witchcraft, the author seems to have sought a 'non-positivist' style, and this sometimes seems to provoke a deliberate retreat into obscurity, and a frequent refusal to say with simplicity and clarity those things that can be so said. Thirdly, this is not an ethnography of a village, or of an area, but of witchcraft. The ethnographer lived a long way from the areas which she studied (fifty miles from her main case study, for example, although she had ethnographically sound reasons for having done so), and her case studies took her over a wide geographical area. There is no attempt to give any systematic economic, social or political background either to the area, or to the particular villages concerned. If we come across farming practices at all, it is only in the form of bewitched livestock. The communities in which the case studies are set are hardly described, and we get only a very little idea of who or what are the people involved. As a

consequence, the whole work has a curiously insubstantial feel, with its only structure a thread of witchcraft, connecting a few bewitched farmsteads set in a landscape of magic salt. It is one thing to argue that witchcraft is not necessarily socio-economically determined. It is quite another to ignore socio-economic factors altogether. We are not, either, and this is a crucial failing, given any real idea of what importance witchcraft has to the society under study. It must surely be allowed that many of the peasants that said variants of 'we don't believe that now, it's all rubbish', meant what they said. The main case study concerns a heavily indebted alcoholic whose marriage remains unconsummated after many years and whom the ethnographer first meets in a psychiatric hospital. If his neighbours said 'only alcoholics and demented people believe in witchcraft nowadays', they may just have been right. We are not told sufficient to allow us to judge. The author expresses the hope that she will not be subjected to the cheap and easy criticism that she went to the *bocage* to study witchcraft in order to work out her own fantasies. I would not make such a criticism. When we consider, however, the deliberately elusive style, the very few (although intensive) case studies, the inevitably exiguous and ambiguous nature of the witchcraft evidence, and the lack of any broader social picture in which the witchcraft could be placed, then I think that if the reader has suspicions of fantasy then the ethnographer has only herself to blame. Mme Favret-Saada says of herself: 'she has chosen to investigate contemporary witchcraft in the Bocage of Western France' (p.3). Why? We are not told. For a book so self-assertive in its subjectivity, this is a curious omission.

This work is self-avowedly iconoclastic and innovative. I feel, however, that some of its icons are long broken, and some at least of its innovations already made. Mme Favret-Saada finds her own theoretical virtue by distancing herself from traditional approaches, but the traditional approaches that she criticises are in several cases almost archaic. In folklore studies, for example, she cites as typical the views of Sebillot (born 1843), and of Van Gennep (born 1873); in a more obviously anthropological tradition, she criticises Lévy-Bruhl (born 1857); in order to situate the ethnographic tradition, she tells of her reaction to the work of Spencer and Gillen (born 1860 and 1856). She cites Evans-Pritchard's *Witchcraft, Oracles and Magic among the Azande* (1937) as a crucial text, but then takes no notice of any work done on witchcraft in this country since. In particular, she makes no mention of the ASA volume *Witchcraft Confessions and Accusations* (1970), edited by Mary Douglas. This work was intended explicitly as 'Witchcraft, Oracles and Magic revisited', and it foreshadows many of the insights and suggestions made by Mme Favret-Saada.

Parts of *Deadly Words* read like a rather exciting and mysterious 'who-done-it?', as we go in pursuit of the witch and the unwitching; it is something of a disappointment, in a way, to discover that it was the discourse what done it. This is,

perhaps, less startling as a *dénouement* than it was even only a few years ago, and it risks becoming a banality. It has been argued in the pages of this journal (see M.Crick, 'Two Styles in the Study of Witchcraft', *JASO*, Vol.IV, 1973) that witchcraft should be regarded as a discourse with its own internal rationality, and its own capacity to define reality. Such is very much Mme Favret-Saada's view, and I think that in general she has more allies than she is prepared to acknowledge. Part of the problem seems to be that she is in retreat from a positivism even more dogmatically inept than that which sometimes prevailed in anthropology in this country. She says:

But of all the snares which might imperil our work, there are two we had learnt to avoid like the plague: that of agreeing to 'participate' in the native discourse, and that of succumbing to the temptations of subjectivism.

The British tradition of participant observation was an altogether more complicated practice, it seems, and it certainly did not provoke its ethnographers into extremes of participation and subjectivity by forbidding these altogether. I remain unsure whether this research dogma was really one forced on Mme Favret-Saada by the CNRS in the late 1960s and through the 1970s, or whether it is a dogma that she keeps on the shelf next to Sebillot, Lévy-Bruhl, Van Gennep, and Spencer and Gillen.

In spite of its faults, however, this is an impressive and subtle book. We are promised a sequel, although what exactly this will contain remains unclear. *Deadly Words* is a valuable and original contribution to European anthropology. With the right sequel, it could be even more so.

MALCOLM CHAPMAN

PETER METCALF, *A Borneo Journey into Death: Berawan Eschatology from its Rituals*, Philadelphia: University of Pennsylvania Press 1982. xxi, 264pp., Bibliography, Index, Plates, Maps, Figures. £17.25.

When a Berawan dies, the body is dressed lavishly and enthroned. Communal festivities are held for some days. Then it is inserted into a jar or coffin, taken to a graveyard, and there entombed.. For a privileged few, a second 'funeral' is held a few years later. Again there is a series of public festivals, now centred around the exhumed remains of the dead. The celebrations close with the placing of the bones in a mausoleum, often of elaborate

design. This skeleton of Berawan mortuary rituals, the core of their ceremonial life, is fully fleshed out in Metcalf's ethnography, in which he skilfully extricates the eschatology of this Borneese people. Building on Hertz's insights but letting the ethnography command what analysis is needed, he reveals, in his cautious step-by-step interpretations, what we can, and cannot, say about Berawan notions of death, the soul, spirits, and ancestors. Stimulated, not embarrassed, by uncertainty and vagueness, he delineates precisely the definiteness (or lack of it) in different domains of Berawan ideology. To pervert one of his comments into one on his own work, 'the power of the performance to move the audience is only enhanced by this indeterminateness'. Berawan ideology, we learn, is orderly in just those areas that are ritually elaborated. The Berawan say little about the close, but problematic, relation between the living body and the soul, and are inconsistent about the location of the land of the dead. About the process of death, however, they are explicit and exact: at first the soul is detached from the body with which, though, it shares a common destiny; as the last remnants of the corpse decompose, the soul fades into pure spirit and so joins the mainly anonymous category of the ancestors. Along the way, Metcalf makes interesting (though sometimes inconclusive) observations about food, hair, noise, and the mockery of animals within *rites de passage*, and about *rites de passage* themselves - the process of 'secondary funeral' reverses the direction of Van Genep's classical tripartite division.

Metcalf, co-author of the praised 'Celebrations of Death', gives his account with lucidity and much care: rather a more apposite word than a clever turn of phrase; detail does not mean drabness. Eschewing wide generalisations, he notes the extension of certain beliefs and the individuality of others. We know on what grounds his interpretations stand. He is explicit, if not fulsome, about the nature of his fieldwork, about the way he elicited information, and (occasionally) about the very questions he asked. The anthropologist thus becomes one of the book's central characters: an attractive procedure since it, in turn, highlights the personality of several Berawan. The author does not let us forget the status of ethnography as cross-cultural artefact, his assiduous, discursive analyses laying bare his method of approach - a rare, but welcomed, uncovering in the genre.

His closing vignette serves as vivid reminder that though people may play their lives out according to rules, they are not compelled by them: at one funeral, a prominent lady unexpectedly sings a song of praise, not the usual dirge, to her deceased friend. The audience cries helplessly.

P.E.DE JOSSELIN DE JONG and ERIK SCHWIMMER, (eds.), *Symbolic Anthropology in the Netherlands*, The Hague: Martinus Nijhoff 1982 [Verhandelingen van het Koninklijk Instituut voor Taal-, Land- en Volkenkunde 95]. vi, 221pp. Dfl.50.00.

Nearly all social anthropologists can be presumed to be more or less familiar with the work of Lévi-Strauss and of Victor Turner. These men are leaders in the work on symbolic anthropology and come from 'the major world schools' of anthropology located in France and in the United States. Fewer, perhaps, are aware of the kind of work which is being done in places such as Denmark, Brazil, the Soviet Union, and the Netherlands. Such is the way that one of the editors of *Symbolic Anthropology in the Netherlands*, Schwimmer, sees the institutional aspect of symbolic anthropology, and this led him to plan collections of work being done in these places (one of which was to be this volume). In the event the project unfortunately proved 'too difficult', but the Koninklijk Instituut voor Taal-, Land- en Volkenkunde in Leiden came to the rescue of what was to be the Dutch collection which now takes its place among the distinguished *Verhandelingen* of the Institute as the 95th volume in the series.

The text consists of an 'Introduction' written by Schwimmer and of the twelve other articles in the book, all of which are more or less theoretical; one is by de Josselin de Jong, the senior editor. Most of the chapters are about 20 pages long; the shortest - 'The Social Meaning of White, Red and Black Among the *Ahonian* of Ivory Coast (West Africa)' by Visser - runs to just 9 pages, which may strike professional critics as somewhat brief given the enormous amount which has already been written about these colours. (Visser's 'Bibliography', incidentally, refers to very little of the literature which we could have reasonably expected to be cited in a paper on this topic.) The book has no index, but as we know books published for the KITLV by Nijhoffs are always very well produced and this volume is no exception. (Proof-reading has not, however, always been as careful as it might.)

Moyer - a Canadian who studied at Leiden - explains that 'Dutch structuralism' (*Nederlands structuralisme*) is an expression which is rarely used; instead people more usually refer to the *Leidse richting* which means something like 'the Leiden way of doing it'. So we should not perhaps be misled by the title of the collection into thinking that it is a comprehensive exhibition of Dutch symbolic anthropology. The book indeed is heavily biased in favour of the *Leidse richting*.

Moyer further contends that there are not 'any major theoretical differences between the Leiden and the Paris approaches'; rather, the *Leidse richting* prefers to concentrate almost exclusively on Indonesian material and not to seek out cultural universals (although these 'undoubtedly exist') as the Parisians have done because they feel that it is more practical to begin with a 'clearly defined' field of anthropological study (*studieveld*).

In fact, a recent symposium held in Leiden on this topic showed that it would be going a little far to say that any '*studievelde*' has yet been clearly defined, but this is perhaps of little importance because 'the concept...is broadly consistent with the structuralist method' according to Schwimmer.

Later, Schwimmer shows that there is really no method which could satisfactorily be shown to be *the* structuralist method and, indeed, the papers in this book follow a Leiden tradition of eclecticism and of balance by refusing to identify with any foreign school. Another feature which unites the papers in this book (in Schwimmer's view) is that 'by and large' they try to resolve some of the 'contradictions' between the phenomenological approach of Turner and the structural (rationalistic) approach of Lévi-Strauss. This can only be welcomed, of course, for the best social anthropology has always been done by those who have denied themselves nothing (so far as they could) in coming to terms with exotic ways of life, and this includes the insights, if such they are found to be, of different theoretical approaches. But readers of *JASO* will undoubtedly want to know how this helps, first, to make the questions which the papers confront resolvable and how it can help, second, to make specific social facts intelligible (or more so). That is, how do these papers contribute to advance in social anthropology? This question is particularly germane for Schwimmer takes the view that the 'smaller anthropological centres...offer solutions' which have been 'underestimated' in the centres where 'the basic theories' have been formulated.

'Advance' can conveniently be considered from the point of view of ethnography and from the point of view of theory, not because the two are practically separable but because they are distinctions which are often made and with which (it is judged) Schwimmer might agree (though he refers to ethnography and to the semiotic). Each will be dealt with in turn.

The papers deal with some interesting ethnographical topics, such as the meaning of some notions of the Khroumirian concerning the symbolism of the body and the way the status of blacksmiths (*verrhE*) is symbolised in Kapsiki notions of what is edible and what is not and by whom. The ideologies considered range from the Kwakiutl to the Biak-Numforese (Irian), and from Scotland to the Cameroons.

The papers are not all equally concerned with ethnography, so that in some papers the ethnographical content is detailed and precise, while in others it is sketchier, but this is often because the papers in question are more roundly theoretical. The question whether ethnography adduced by an author can support the conclusions to which that author comes is not one we have space to address here. The last paper in the collection deserves special mention though: 'Spirit and Matter in the Cosmology of Chassidic Judaism', by Meijers and Tennekes, contains well-ordered data which is detailed and clear and which admirably supports the authors' contention that Douglas's views about 'the relationship between cosmology and social structure' advanced in *Natural Symbols* are, if not wrong, then exceedingly problematical. Any

comparativist, though, will find something to interest him in this book.

Whether he will be similarly interested by the theoretical content of the papers is another matter. We cannot here go into the relative merits of the approach of Lévi-Strauss as compared with that of Turner (or, any other) or into the different theoretical conclusions to which different authors come. We can, however, make a number of general points about this aspect of the papers.

It is not always clear, first, what is indigenous ideology and what has been imputed to social facts by authors on the basis of some theoretical predisposition. Perhaps the contributors do not see this as worrying for Schwimmer argues that social anthropology is in certain cases deductive and that in some conditions deductive argumentation is 'indispensable', while van Loopik (in 'Social Structure in Mangili') writes that he takes the position of Pouwer, 'that a model by definition never is part of the data, and that there is no difference between a paradigm extracted from the data and a model superimposed on the data....' Readers better versed in making methodological arguments will have to judge these views, but it can perhaps be remarked that, *pace* Pouwer, there clearly is a difference between the two. The former can help us advance because it allows us to make discoveries; the latter cannot for it necessarily leads to the constant application of particular theories to data. In the study of Balinese ideology, for example, the imputation of theory to the four great classes (*warna*) into which all Balinese fall would probably result in the distortion of a hierarchical system which cannot regularly be correlated with theoretical views about the nature of inequality. The point is this: by applying 'theoretical models' to these facts of Balinese life, the *warna* represent a system of inequality; induction leads to quite another view, and one which appears to be of some theoretical interest. Why accept the former when the latter has so much more basis in fact and when it is more interesting?

Theoretical predisposition, second, seems to have blinded authors to indigenous ideology: Schefold, for instance, in the only paper in which some influence of Marx can be discerned, does not stop to ask what it is about cash which makes it a suitable item of exchange for corn in the ideology of the Khmir but unsuitable as such for milk. The meaning of 'cash' is on the face of it, after all, as variable from ideology to ideology as are corn and milk, and attitudes to it are equally variable. Such variability renders it wholly unlikely that it can serve as an analytical concept. (The same may be said of 'power'.)

Theory, next, seems to have blinded some to logic. Let us take van Loopik's paper again. We cannot here go into any great detail; one example must suffice. Unity, he says, reigns in nature. Culture arises through the incest prohibition so that society is created through separation. An awareness of own and other however exists in nature. But this implies separation in nature, so that a fundamental point in van Loopik's argument is

flawed. There are other points of similar logical inconsistency. Such flaws can be discerned in *Le Totémisme aujourd'hui* also, and neither can be accepted as explanations of the way that society emerged. As it happens Andrew Lang (for one) had a far more plausible view, that society arose not through separation but through alliance in search of life and of peace. Society may involve the recognition of self and other (as Rousseau recognised) but it cannot be said to rest on it nor can it be said that other institutions derive from it causally. (There is a strong streak of outmoded causal reasoning in many of the papers, perhaps because many of the authors think that it can explain things.)

A final general comment: theoretically, there is (it is very much regretted) nothing which I have found useful in trying to understand Balinese ideology. Indeed, one finds oneself being increasingly baffled that in a book in which in the 'Introduction' we are enjoined to 'an unrelenting preoccupation with the mysteries of phenomenal reality' the main preoccupation of the papers appears to be to move away from that reality as described in social facts to a world (departments of social anthropology, of all places) where what is taken to be of interest is theorising. This, though, often relies so heavily upon terms which are definitional that it is open to endless objection, so that definition of concepts and of terms and not phenomenal reality and its mysteries become the focus for attention.

This interest in theory is well illustrated by de Rijter in 'Lévi-Strauss and Symbolic Anthropology'. Discussing Lévi-Strauss's views of indigenous ideology - which it may surprise some to know hold that it is always important, be it 'true' or 'false' - de Rijter writes that 'the crucial question' is how an analyst knows when he has reached 'real reality', i.e., when his analysis can stop. So the crucial question is what understanding might consist in - or look like, since it so often seems to need diagrams. But how can this question be answered without reference to particular puzzles which need resolution and which (perhaps) require different kinds of resolution?

Understanding will be enhanced, de Rijter thinks, if we 'return to the pre-*Mythologiques* phase of structuralism' for this was when 'a double aim still appeared to full advantage in the actual research process: on the one hand the description and uncovering of various patterns of socio-cultural phenomena and on the other hand the demonstration that these systems of signification are expressions of the human mind adapted to specific environments.' But this means that we have to collect and to analyse social facts; a return to what de Rijter admires, that is, would be best achieved by contributing to 'the actual research process', rather than theorising about it.

Moyer writes that 'the continuation of progress in the analysis of myths of social rationalization would best be served by a combined approach by anthropologists, historians, historiographers, and literary scholars', and this, we might add, naturally goes for all branches of social anthropology. Indeed, nothing which might be useful in analysis is likely to be knowingly over-

looked by anyone really trying to make sense of social facts, especially if the analyst has collected them himself.

Oxford is not mentioned in the text. One could concoct likely reasons and possible motives for this but it would be impertinent to attribute these to the contributors to the book. Still, the omission is odd and can be put down only to a regrettable ignorance, regrettable because what is theorised about in this book is most often what is, after many years, still being put to use in Oxford and in other places by people who were educated there in making sense of social facts, as used to be done in Leiden. It is up to the reader to decide which he considers more likely to contribute to real understanding and to substantial theoretical progress.

ANDREW DUFF-COOPER

EDMUND LEACH, *Social Anthropology*, New York and Oxford: Oxford University Press and Glasgow: Fontana 1982 [Masterguides, gen. ed. F. Kermode]. 224pp., Bibliography, Index. £9.50 (cloth), £2.50 (paper).

Edmund Leach has produced a revelatory introduction to the interests and aims of a working social anthropologist. And yet, since this book was published it has met with a constantly critical response, especially from social anthropologists. The received wisdom appears to be that the book is too idiosyncratic to be a useful introduction to the general subject areas and methodological issues of the discipline. Hopefully, neither students nor interested general readers will feel disinclined to read Leach's volume. To that end this review will briefly indicate some of the book's main achievements and theses.

The first chapter demonstrates the diversity of approaches within the broadly defined subject of 'anthropology'. These differences, Leach claims, reflect two polarities: first, the contrasting problematics of social anthropology (mainly British) and cultural anthropology (mainly American); second, the dichotomy between empiricists, who see anthropology as a natural science akin to zoology, and rational idealists, who seek to uncover the unconscious structures of the human mind. Of course, these contrasts relate to enormous areas of comparative ethnography and theoretical discourse; and it is one of the achievements of the author that he has condensed his vast erudition in these areas to compact proportions. He succinctly reconstructs the intellectual history of such influential credos as 'functionalism', 'structural functionalism' and 'structuralism'. Having done this, and having declared

the functionalist and structuralist strands in his own work, he considers what sorts of epistemological corollaries follow from his conspectus.

To the present reviewer's mind, the most interesting of the personal dogmas which Leach deduces is that social anthropology belongs to the humanities and not the natural sciences. This assertion is nothing new. But it rests upon Leach's considerable grasp of the Popperian view of the contents of scientific knowledge. Social anthropological knowledge, Leach argues, is not scientific (in Popper's sense). It has to do with interpretative understanding. Anthropologists seek to gain insight into the varying forms and underlying principles of social behaviour, rather than to assert falsifiable propositions about conjectured universally-valid truths. This is a fundamental point; for it is important that working social anthropologists as well as general readers should reflect on just what anthropological understanding is.

Leach touches on important questions about the nature of power, asymmetry and exchange in human relationships throughout this volume. For example, his concern with analysing the hierarchical dimension of most human relationships (Chapter 2, pp.58-60) appears again when he later discusses the relation between power and hierarchy (Chapter 5, pp.156-159). He relates these questions to his central thesis which he states in the fifth chapter; namely

that it is only when we come to understand that relationships between man and man and man and god are, at least in a metaphorical sense, the equivalent of economic indebtedness, that we can really appreciate how this transformation of economics into ideology through the mediation of kinship actually occurs.

In support of this fascinating thesis comparative ethnographic materials on marriage exchanges are assembled in the sixth chapter.

In the sixth chapter Leach also expresses scepticism about the universal applicability of technical categories, such as 'family' and 'marriage', which social anthropologists customarily employ. This point about categorical uncertainty and ambiguity echoes a similar concern which has been persistently expressed by Needham (see, e.g., *Belief, Language and Experience* and the papers on 'inner states' and 'religion' in *Circumstantial Deliveries*). The important point which such scepticism raises is whether social anthropologists, as the translators of cultures, can feel confident about the general denotative force of categories which have been formed within a particular society with particular institutional arrangements. This perspective, hopefully, should prompt greater sensitivity both to the observer's own concepts (which he or she uses before, during and after conducting field research) and to the actors' own vernacular categories (which may denote inner states, attitudes and institutions for

which there are no corresponding terms in our Western vocabularies).

One might conclude by saying that most general introductions to the discipline are inevitably idiosyncratic in some way; moreover, they invariably have to skip over large areas of interest. So Leach's volume need not appear any different on these counts. His book is an honest and pleasurable introduction to the 'study of man' (as the etymology of 'anthropology' implies). In debunking the sexism inscribed in the English language which anthropologists use, and in teasing out the common themes in their sundry approaches, Leach has helped to communicate the sorts of values and humanistic concerns which anthropological understanding is based on and engenders.

SCOTT K. PHILLIPS

BABIKR BEDRI, *Memoirs*, Volume II (translated and edited by Yusuf Bedri and Peter Hogg), London: Ithaca Press 1980. 367pp., Index. £11.50.

The remarkable Shaykh Babikr Bedri died at the age of 94 years in 1954. He wrote three volumes of memoirs, published in Arabic, of which two have been translated into English. The first covers that period of his life which ended with the final suppression of the Mahdist revolt, a movement in which he had been an inconspicuous activist. He became comfortably well-off from his dealings in trade, at this time, but at the end of it, he spent nearly two years in Egypt as a prisoner of war. This second volume covers the 31 years of the middle period of his life from 1898, years during which he won renown as an educationist, until his retirement from Government service in 1929, at the age of 68 years - although he continued his educational activities long after that.

If this English translation faithfully reflects the prose of the Arabic original, it is commendably clear, and the descriptive passages are all the more effective because his style of writing is to the point, without any signs of being contrivedly elaborate. Thus, when he happened to spend a night in a village where the plague was raging, he relates how a man, who was assisting in burying the dead, collapsed over the grave: 'He was carried to his house and shortly afterwards we heard that he had died. So the burier was buried.' - a more telling way of conveying the effects of the plague than any amount of lurid description. Or, again, when a married daughter tells him that she knows she is going to die but she wanted to tell him how much she loved him. He took her in his arms and told her to repeat 'I ask pardon of God', which he did '...until her voice died away and I laid her

down. So she departed this life.' - nothing more.

Misfortune of one sort or another dogged him throughout his life. On a tour of inspection of schools, at the age of 61 years, he happened to be without transport at a certain place, so he borrowed a camel. It was a most wayward beast, which, just after he had mounted it, ran away with him. His attempts to control it merely led to it turning rapidly in circles, until he was thrown out of the saddle. The Shaykh fell on to a rock, on his back, severely damaging his spinal column. Alone with only an unreliable policeman as a companion, he lay where he fell, unable to move, wondering what to do. After a while he caught sight of a few people some distance away, and dispatched the policeman to bring them to him. It almost goes without saying that he knew one of them well, for he made a point of getting to know people wherever he went, and to do them good turns whenever he could. He was taken immediately to the nearest hamlet. While there he was given a 'fire bath' (a trough heated with burning wood, the embers of which are removed before the patient is laid on the hot earth, spread all over with leaves, and then to be covered with sand up to his chin), but he had to be removed from the bath in great haste because some still smouldering embers burned through his cloak. Weakened further, unable to eat, and feeling that his pulse-rate was dropping, he requested an elderly woman, who had been tending him, to send for her daughters to entertain him, 'to liven up my circulation'. It alarmed him that the woman, her husband, and others thought his reference to entertainment meant sex, but after he fulsomely explained what he had in mind, the two young girls were fetched. Five diversionary days in their company and he was well enough to be carried on the long journey to Haifa. There he was treated by a doctor, who forced his displaced vertebra back into its place. About a week later he was back at work. In less than a year he was beset by another major misfortune. To his brother Yusaf he had always been devoted, and when, on a tour of inspection again, he heard that his brother was in financial straits, he decided on a detour to visit him. Before reaching his brother's village, he was given the news that Yusaf had shot himself that very morning. It lit a 'flame of grief' in his heart. Within a month of this disaster, the son-in-law he favoured most died at the age of 25 years. The Shaykh faced all these, and other reverses stoically, producing an appropriate epigrammatic sentiment for each one: 'The wise man is he who draws value from misfortunes, not he who is fettered by them.'

Bedri's energy was prodigious, and when an Inspector of Schools - the first Sudanese to occupy the post - the amount of travelling he did, over a wide area, is truly amazing. Out of choice, he eschewed the more comfortable and quicker forms of transport, preferring to travel by donkey - he composed a charming poem for the donkey that was 'very dear' to him. He admits that he was able to save a little in adopting this form of transport, to help him pay off his chronic debts (accumulated in giving loans and other sorts of help to friends and relatives), but donkey travel also allowed him to indulge in lengthy sessions of

conversation with notables and others wherever he went, from whom he learned 'the history of their tribes', and for this reason his works are still invaluable ethnographically for any anthropologist working in the area. The other aspect of this mode of travel is that he was not ambitious in terms of acceding to power; for the first step along this path is to control one's relations with others, free of any dependence on them for a night's lodgings and a meal, a control made quite impossible by slow-moving donkey travel, stopping in practically every village and staying in most.

The Shaykh's ambition was to carry education to as many places as possible. His enthusiasm was not shared by all Sudanese, many of whom, especially the local religious notables, who, on the whole, were opposed to anything other than Quranic education. He was equally determined that some amount of elementary secular education should be given to children as well. Much of his time he spent in persuading local people of this necessity, successfully on the whole. To do this, he adopted a number of ruses. Football had become a popular game in one of his areas; he sent to Khartoum for some footballs, but would not allow boys to play unless they first registered as pupils in a newly established school which had been boycotted: he soon had more than the quota of pupils originally planned. On another occasion, he persuaded an official to threaten a man with jail if he took his fatherless nephew away from school, and when he happened to see another pupil, who should have been at school, following a donkey carrying fertilizers to his uncle's fields, he whacked the donkey to send it on its way, took the boy to market to buy him a new suit of clothes, and sent him back to school.

In furtherance of his aim to spread education as widely as possible, the Shaykh encouraged local people to contribute to the up-keep of their school. This self-help approach was encouraged by the administration, which would provide a building free of rent. Bedri, some three years after negotiating this arrangement, went further when he decided to start schools for girls. Administrative officials, thinking that the opposition from traditionalists would be unbeatable, were very sceptical, but they did not impede him. In 1907 he opened the first girls' school. When it became evident that it would prosper, he was given a small grant by the administration to assist him in this new venture. Indeed, he got along so well with officials at the Department of Education, that he was thought by many to be committedly pro-British, and an unbeliever to boot. In fact, he admired much about the British, the wise and just ways (to use his own words) in which he was treated by them, but, as he told one of the officials for whom he had deep respect '...the Sharia law forbids me to love you because you are an infidel'. As he saw things, government by foreign powers was temporary, and that education - albeit of value in itself - would hasten the day of independence. Devoted to the advancement of education for his own people, he was Sudanese through and through, many of the trials and tribulations he suffered stemming from these sentiments.

What has been said about Bedri's Memoirs here is by way of

an aperitif. Delightful touches light up his account on almost every page: the old man of over 80 years who did not dismount from his donkey when he was approached by an officious Englishman, explained, when reproached, that he had refrained because the official would also have had to dismount to lift him back on to his donkey; or when the Shaykh drew a local notable into a circle of eminent people, first discreetly tucking his undergarment out of sight. His dealings with people of all sorts are a delight to read, as petty as the next when necessary, magnanimous in his forgiveness even when suffering personal loss at the hands of law-breakers, and always with an eye for that ethnographic pearl of behaviour. It is a very satisfying book to read. The pity is that the third volume of *Memoirs* is unlikely to be translated.

E.L. PETERS

## RECENT BOOKS ON LATIN AMERICA

PAUL HENLEY, *The Panare: Tradition and Change on the Amazonian Frontier*, New Haven and London: Yale University Press 1982. xxii, 247pp., Diagrams, Bibliography, Index. £15.00.

The organisation of this work and its overall theoretical approach arise from the author's long interest in the nature of articulation between the indigenous peoples of Amazonia and the fast-expanding national fronts. The Panare present an excellent opportunity for such a study. They are a small group (under 2,000) living south of the Orinoco in the valleys of the Cuchivero, Guaniamo, Suapure and Manapiare, and are more or less typical of those acephalous tropical forest societies which live by shifting cultivation, hunting, fishing and collecting, in a dispersed settlement pattern. However, in one respect at least they are unusual: at some time during the 19th century they began a northward migration from the upper Cuchivero, gravitating towards a colonizing front of Venezuelan non-Indians (*criollos*) who were at the same time expanding southwards. Most Panare settlements today are within no more than a short walk from a *criollo* residence.

This migration was probably due to a desire for easy access to trade goods, especially the much prized metal tools, which sent other Guiana Caribs, such as the Ye'kuana (Makiritare) on lengthy trade journeys lasting for months. Today, Panare live in a close relationship with *criollos*, exchanging their basketry for industrial goods. *Criollo* pastoralists depend on their basketry moulds for cheese-making whilst their patterned round baskets have become a favourite item in Venezuelan artisania. Despite a long association of a century or more, the Panare have so far retained their distinctive social and cultural traditions, their remarkable

resilience contrasting with the weak resistance to acculturation of many Amazonian Indians after only brief contact. They therefore constitute a success story worthy of investigation, so that the question which arises is: why have the Panare apparently escaped the ravages of contact which many others have not?

The book begins with a description and analysis of the nature of their internal relationships. The exposition in this part seems to be based on the 'layer-cake model of society' (as set out by M.D. Sahlins and E.R. Service in *Evolution and Culture*). In this, economy and technology are sandwiched between the physical environmental base and an overarching superstructure which is the political system and ideology. An interpenetration occurs from one layer to the next which consequently adjust to each other. Henley uses the model perspicaciously and from the interrelationships extrapolates a series of underlying principles.

The atomistic nature of Panare production, centred on the hearth group (conjugal family) with its complementary division of labour by sex, is contrasted with the predominantly collective nature of distribution and consumption, mainly via communal evening meals in times of abundance. From this Henley deduces the disruption of the economic infrastructure of the residential group in conditions under which the principle of food sharing is discontinued for long periods - that is, when each hearth group keeps its food for its own members who eat in isolation. This occurs when Panare live on the plains near the *criollos*, since this is a more impoverished habitat. Although he himself does not make this point, the mode of production and consumption which he describes is the exact opposite of the popular ideal of the co-operative folk society, Amerindians often being quoted as an example, in which members are thought to possess and to work in common and then to divide up the proceeds for private consumption. Henley does discuss another ideal, that of the indigenous, conservationist society, and he finds the Panare make a rational, but also prodigal, use of their resources. However, through their pattern of extensive resource exploitation they are aptly described as 'conservationist in consequence' and not 'conservationist by intention'. Unfortunately he does not tell us whether there are inherent constraints in the conceptual system; if not through the notion of spirit masters of the natural resources then of environmental forces which are denoted by shamans as exacting revenge by inflicting sickness and misfortune on greedy humans.

The analysis of Panare marriage and kinship brings fresh air to a number of controversies as to the nature and performance of a dominant system of prescribed bilateral cross-cousin marriage. For example, there is the recognition that affines and blood kin do not form discrete groups or have separate terminologies. I was pleased to see the system firmly categorised as cognatic with information given to show that the development of a two-line or two-section system could not occur. The exposition of the unusual alternate generation marriage is an exciting scoop for marriage-type fanciers. Henley claims that, like proximate

generation marriage (the avunculate) it is a response to the situation of uxorilocal residence and son-in-law service, also permitting a higher degree of local endogamy. Certainly in these societies the problem is to avoid the departure of a son and yet be able to claim the services of a son-in-law, so that manipulation of the dependant female kin resource is to be expected in such circumstances. However, it seems to me that the rarity of MF-DD and MF-ZDD marriages counts against their being a numerically effective device of this kind, and they may simply provide an extra means for consolidating relationships between men of adjacent generations who are, nevertheless, of a similar age group. Effectively, cross-cousin marriage allows for an exchange of sisters to be followed by an exchange of daughters and then, among the Panare, the alliance can be extended into a third generation via DD marriage. The whole concept of endogamy in these cognatic systems needs careful investigation, particularly in view of a wide variety of different settlement groupings such as exists among Caribs.

The theoretical model used in the second part of the book derives from the work of the Brazilian anthropologist, Darcy Ribeiro, *Os índios e a civilização: a integração das populações indígenas no Brasil moderno* (Rio de Janeiro 1970) which Henley had carefully reviewed in English ('Os índios e a civilização: a critical appreciation', *Cambridge Anthropology*, Vol. IV, 1978) and found stimulating. Ribeiro argued that, provided the members of an indigenous society physically survive contact, the social and economic relations are more important in determining the outcome than is the predisposition of the people to adopt, adapt or reject incoming ideas and experiences. Since cultural traits pass by means of the relations between people and these operate within specific economic systems, then it is to the dominant sphere of the national society and culture that we must look for explanation, and to their effects on the numerically weaker and politically powerless indigenous group.

In testing this hypothesis, Henley considers Panare external relations and systematically examines the effects of national expansion on them. He reviews the extractive industries (collection of sarrapia and mining), pastoralism, agriculture and trade, and also the recent intervention of protectionist agencies of government and missions. He notes a recently more destructive phase beginning in Panare-criollo relations and he considers factors such as the numbers and intensity of contact, the problem of the 'outsider', of competition for natural resources and the damaging effects of selling surplus food instead of sharing it. This part of the book offers an extensive field for comparative research.

Whether the Panare can stand apart indefinitely from the mainstream culture of the national society, even given their land rights as Henley proposes, is another matter. A materialist reasoning is not sufficient, for Panare values, as he recognises, may determine the outcome. The Panare may themselves choose to enter into *criollo* economy and society to the degree that they

will bring about profound changes in their culture and society. The Wayana of Surinam and French Guiana are another resilient group of Caribs who might be investigated along similar lines to those used for the Panare. Certainly they have in common an unusual institution, which is a series of special public and group ceremonies for the initiation of boys. In both cases these are attended and served by members outside the settlement and an affective relationship with the total ethnic group seems to be stressed. This does not occur in most Carib societies which instead have a piecemeal infliction of hunting charms, taking place individually within the immediate family group. It may be that this practice of group initiation with its attendant values provides another important reason for Panare and Wayana success in maintaining an ethnic identity. Outside the South American area, the *molimo* ritual of the Ituri forest pygmies in Central Africa is another such instance. Clearly, the effects of mass initiation on the unity of the wider ethnic group is an important topic for comparative research.

Encapsulating the results of nearly two years of field research in Venezuela, Paul Henley's monograph is a valuable addition to Carib literature. The ethnography is of high standard; the analysis of both the traditional system of the Panare and of their external relations is systematic and the argument closely integrated. It is moreover, an intelligent and sophisticated work, taking us a long stride forward in our attempts to understand situations in which indigenous peoples are having to cope with expanding national interests and development.

Should another edition be forthcoming, then one might express the hope that Yale University Press will not only correct typescript errors but also somehow manage to arrange that the footnotes invariably get printed at the bottom of the appropriate page of reference. The presentation would then be in line with the high quality of scholarship which the book displays.

AUDREY BUTT COLSON

DAVID JOHN THOMAS, *Order Without Government: The Society of the Pemón Indians of Venezuela, Urbana etc.*: University of Illinois Press 1982 [Illinois Studies in Anthropology no.13]. 249pp., References, Index. £21.88.

'Pemón' is the self-designation of a number of closely-related Amerindian groups who live on the high savannas that form the water-shed between the Orinoco and the Amazon as well as the frontier of southeastern Venezuela with Guyana and Brazil. In the ethnographic literature, the Pemón are frequently divided up

into the Kamarakoto, Arekuna and Taulipan of Venezuela and the Makuschi of Brazil and Guyana. However both linguistically and culturally they are all very similar, not only to one another but also to the Akawaio, Ingarikó and Patamona who live slightly further east, mostly in Guyana. Although the latter groups designate themselves as 'Kapon', their language was reported to be easily intelligible to the Pemon by the nineteenth-century traveller Im Thurn, a judgement more recently corroborated by Audrey Colson (personal communication). Taken altogether, the Pemon-Kapon number about 25,000 people, making them comparable in numbers to the Yanoama groups living further west on the Orinoco-Amazon watershed and the Ashanika (Campa) of Peruvian Amazonas, both of which have been described as 'the largest contemporary Amerindian group of Amazonia'.

*Order Without Government* represents the first full-length monograph in English about this most important group of Amerindian peoples, and for this reason is a most welcome addition to the literature. It deals with the Venezuelan Pemon amongst whom the author carried out Ph.D. fieldwork in 1970-1, followed by a shorter spell in 1975. In contrast to many books based on doctoral research, it is written in a clear and readable style (although some might find it just a little 'folksy' at times), testifying to the intellectual confidence that the author has acquired in the years since he completed his thesis (1973). Furthermore, although the analysis of social organisation plays a central part in the book, just as it did in his thesis, this has been supplemented with additional material on the Pemon belief system, including a discussion of the syncretic cults that have flourished amongst the Pemon-Kapon since the nineteenth century, as well as an interesting analysis of a short corpus of myths selected to demonstrate the Pemon's preoccupation about certain kinds of interpersonal relationship.

However, as the title suggests, the central problematic of the book is the so-called 'problem of order', represented by an egalitarian society lacking any form of institutionalised leadership roles associated with coercive powers, and yet in which disputes and interpersonal violence are rare. Thomas begins by acknowledging the conventional arguments to the effect that order in societies of this general kind is maintained by tight networks of reciprocity, reinforced by informal moral sanctions, operating between closely-related people who spend most of their lives living in close geographical proximity to one another. In the first half of the book he gives a good account of how such kinship-based networks regulate everyday life amongst the Pemon. However, Thomas argues that a preoccupation with 'what kind of social "glue" holds people together... has rendered our understanding of societies like the Pemon somewhat myopic, at best'. Instead he proposes that the decentralised and politically anarchic character of Pemon society be viewed as an 'enabling device' for the preservation of individual autonomy. It is precisely the refusal of the Pemon to allow any permanent denial of such autonomy that provides the moral order underpinning their

society. The individual may be bound to others by a variety of reciprocal rights and duties, but this 'mutuality' has its limits. As the Pemon put it: 'When we get too close together, there are fights'.

Yet although one may sympathise with Thomas' view that an excessive preoccupation with the maintenance of social solidarity could blind one to the fact that some peoples may live in egalitarian societies by choice rather than merely by default, it is difficult to accept the importance he attributes to Pemon ideology (or rather his own idealised synthesis of the same) in explaining the order of their social life. Particularly in the latter part of the book, Thomas tends to write as if this order were little more than the epiphenomenon of a series of highly abstract notions of power, presented in a succinct typology following his analysis of Pemon myths. Personally, I do not find this discussion of 'power types' (the emic justification for which seems to me to be slight) at all convincing.

But whatever one might think about the author's more abstract theorising, one cannot deny that *Order Without Government*, considered as a work of basic descriptive sociology, is thorough, competent and a most valuable contribution to the field. Thomas' discussion of Pemon social organisation is one of the best so far produced for a Guianese society, though it is somewhat strange that he does not refer to Rivière's very well-known study of the Trio, especially given the sociological similarities between the two groups, most notably their common custom of ZD marriage. Also, I would have liked to have learnt more about the syncretic cults of the Pemon, even though to deal with these exhaustively would require a complete book in itself. It is a great pity that the desperate state of the 'job market' has obliged David Thomas to leave academic life since there is clearly much still to be written about the Pemon and their neighbours.

PAUL HENLEY

ROBERTO DA MATTA, *A Divided World: Apinayé Social Structure*, (trans. Alan Campbell), Cambridge, Mass. and London: Harvard University Press 1982 [Harvard Studies in Cultural Anthropology no.6]. xiv, 176pp., Maps, References, Index. £28.00.

The Apinayé are Gê-speaking Brazilian Indians who live in two small villages in the extreme north of the state of Goiás. The 'Apinayé anomaly', a mistaken account of their elaborate moiety system, made the Apinayé famous and was considered an anthropological oddity since it first arose in the 1930s from a minor error

in Nimuendaju's field observations. In earlier articles ('A Reconsideration of Apinayé Social Morphology', in D. Gross (ed.), *Peoples and Cultures of Native South America*, Garden City 1973; 'The Apinayé Relationship System', in D. Maybury-Lewis (ed.), *Dialectical Societies*, Cambridge 1979), Roberto da Matta laid the 'Apinayé anomaly' to rest with the results of his fieldwork. His more comprehensive published account of Apinayé social life was then available only in Portuguese under the title *Um mundo dividido*. Now there is good news for the general reader: Alan Campbell's clear, readable translation, *A Divided World*, has just appeared, and it adds considerably to the English-language literature on lowland South America.

Da Matta, a Brazilian anthropologist, undertook his study of the Apinayé in collaboration with David Maybury-Lewis and the Harvard Central Brazil Research Project of the 1960s. One aim of that project was to explain the apparently contradictory juxtaposition, within Gê society, of complex social organisations and primitive forms of technology. The clarification of Apinayé moieties fell clearly within the scope of the project's ambitions. Da Matta, along with other project members focusing on analyses of Gê social structure, sought to collect accurate descriptions of social institutions, especially moieties and age sets, marriage and residence patterns, and relationship terminologies.

This, indeed, is Da Matta's greatest strength: his exposition of Apinayé kinship and ceremonial relations is informative and detailed, although it is difficult to extract the basic data from the theoretical framework. In brief, there are two pairs of moieties, neither regulating marriage. The *Kolti* and *Kolre* moieties are associated with the naming system. An adoptive parent arranges for a close blood relative, ideally the child's FZ or MB, to bestow names and moiety membership on the newborn child. In practice, Da Matta finds that the name giver and the adoptive parent are somewhat distant relatives brought into the family circle and given closer terms of reference through this name-giving process. Apinayé notions of 'blood' and 'blood relation' are critical to understanding relations among adoptive parents, name givers, parents, and children. A man's blood turns to semen and combines with woman's menstrual blood to make a child. The transmission of blood from parents to child is at once both a physical and a metaphorical process. Da Matta gives us some rich and fascinating information here, suggesting that the Apinayé believe vital substances to be transferred from parent to child long after childbirth. As more children and grandchildren are born, blood becomes dispersed and 'blood relations' become decreasingly close and binding. A continuum of blood relation results, but Da Matta implies that naming is the institution by which distant relatives, transformed into close ones, are made equal, thus overcoming the potentially hierarchical nature of this continuum. Thus, the *Kolti* and *Kolre* moieties are complementary, and Da Matta calls this moiety structure an instance of 'diametric dualism' (cf. C. Lévi-Strauss, 'Do Dual Organizations Exist?' in *Structural Anthropology*).

The other pair of moieties is based on the system of formal friendship, which Da Matta contrasts to the system of blood relation. 'Non-relatives' form bonds that reproduce the formal friendships of their adoptive parents. The younger friend gains membership in the moiety of his or her older mentor. Da Matta says, for reasons that are unclear, that these two moieties, the *Ipognotxoine* ('people of the centre/village square') and *Krenotxoine* ('people of the periphery/houses') are ranked, the former being the superior moiety. Furthermore, there is an asymmetric relationship, within both moieties, between the older and the younger formal friend. Unfortunately, Da Matta does not directly address the interplay between these two levels of asymmetry; nevertheless he associates this hierarchical moiety structure with Lévi-Strauss's 'concentric dualism'.

The major portion of Da Matta's book is devoted to the description of these moiety structures and the relationship system, including a thorough treatment of kinship terminology with special consideration given to cross-cousin terms. Throughout, Da Matta develops a contrast between blood relations and ceremonial relations, diametric and concentric dualism. 'The Apinayé relationship system', he finds, 'is based on a dichotomy between a private, domestic sphere and a public, ceremonial sphere.... [I]f one looks at the system as a whole, it becomes apparent that all these [cross-cousin] categories establish links that integrate the two basic domains of Apinayé society.'

Having laid out the ethnographic details of domestic life (Chapter Two), ceremonial life (Chapter Three), and the relationship system (Chapter Four), Da Matta turns to the political system (Chapter Five) and concludes with a more formal, abstract analysis of the philosophical underpinnings of Apinayé social life. On the latter score, Da Matta's book can be seen as a response to Lévi-Strauss's work, attempting to attribute dualism to other principles than those that regulate marriage. These principles appear to be grounded in the opposition between the pre-eminent 'natural' importance of undiluted, shared substance, and the 'cultural' propriety of breaking the bonds of the nuclear family in favour of establishing the widest net of social relations. In operation, these principles lead to a conflict in Apinayé society between domestic, private, and exclusive institutions (e.g. the nuclear family) and ceremonial, unifying institutions (e.g. name giving). Da Matta concludes: '...[I]nstead of thinking of dualism as the result of a division of one into two, I think it is more fruitful to see it as a technique that can unite two into one'.

It is in this philosophical argument that *A Divided World* falls somewhat short of its promise. Specifically, Da Matta fails to make a clear and direct statement about the interplay of structural principles on the interpersonal, domestic group, moiety, village, and purely abstract levels. Thus, he appears to contradict the bulk of his argument when, in the last few pages, he concludes, 'The moieties are seen as symmetrical and as having the same social weight'. It should not be the task of the reader

to sort out this conundrum: if Da Matta means to apply the blood tie/social tie distinction separately on different levels of analysis, he does not make this explicit.

Despite this, Da Matta's work is worthwhile and interesting reading for the Crow/Omaha enthusiast as well as for the specialist in lowland South America. Da Matta's attention to the present status of the Apinayé, repeated references to comparative Gê data, and valuable general information on social organisation, make this new translation a welcome addition to the literature on Amazonia.

N.E.FRIED

PETER G. ROE, *The Cosmic Zygote: Cosmology in the Amazon Basin*, New Brunswick: Rutgers University Press 1982. xvi, 309pp., References, Index. \$40.00.

This book has been hatched from a metaphor of the cosmic zygote - 'the fertilized egg, a biologic perpetual-motion machine of vast pansexual import organised about dualistically defined male-female categories'. The author attempts to perceive the mythology of lowland South America and the prehistoric Andean and Mesoamerican cultures within the same shell. Selvatic features from the iconography of Chavin, Olmec art, San Augustine stone sculptures in Colombia, Mochica and Chimu cultures in Peru are all yoked to images in contemporary lowland mythology and treated as if they were all part of one conceptual system.

The method behind his work is a somewhat 'scrambled zygote' of archaeological and anthropological techniques. Roe spent time working with the Shipibo of eastern Peru both as an archaeologist and an anthropologist. When the Shipibo failed to provide him with the wealth of data such as had previously been found among the neighbouring Campa, he decided to compensate for the lacunae by reconstructing the basic elements of Shipibo cosmology using comparative data. By looking at all the similar features in lowland South American societies and putting them all together, he constructs a total model which although not applicable in any one society is applicable to all in some degree.

It is not easy to know on what level this book should be best appreciated. Without any doubt this is the anthropologist's 'curate's egg', but the extent to which it is 'good in parts' depends on whether it has been addled by an indiscriminate use of archaeological and anthropological techniques, or whether the enthusiasm generated by such a *tour de force* can make it digestible. The book's Dedication refers to its author as having a predilection for mind games. It can surely be no accident that a book entitled

*Cosmic Zygote* has been written by a man named 'Roe'. The figure on page 270 summarises the whole model by encircling a list of almost 100 natural features common to Amazon mythologies in tiny balloons. The effect is a vast 'Cosmic Roe' whereby the author becomes a metaphor for his book.

Roe comes perilously close to the fate of Humpty Dumpty in his comparison of archaeological and anthropological material. That highland and lowland societies, particularly those of Peru and Bolivia, have had interrelations for thousands of years is well established. Roe, however, gives a twist to this argument. He claims that the stylistic change which took place in Chavin and other cultures from representation of purely animal forms to more anthropomorphic human designs did not take place in lowland societies which in their myths still retain naturalistic images found in early Chavin iconography. There are two implications here. One is that at some point in prehistory contact between highlands and lowlands became sterile as one 'developed' while the other did not, and secondly that there is little or no anthropomorphic orientation in lowland mythology. From data on the Amarakaeri of southeastern Peru I would disagree with both these implications.

To a large extent this book attempts to apply archaeological techniques to anthropological data. Roe reconstructs by means of comparative evidence. Many archaeologists process and organise their data by comparing cultures in terms of trends and similar features thereby suggesting hypotheses for temporal continuity and relative dating. Anthropologists usually take such similarities for granted and concentrate on differences in order to define some form of cultural identity. Roe's method of looking at similarities need not be condemned. His five important topics comparing Time and Space, The World Tree, the Wet and Dry worlds and the Denizens of the Sky do reappear in various guises throughout lowland South America. However he avoids in his anthropological data the most important archaeological technique. Any find must always be described in its full context. However in this book most of the mythological data has been taken right out of its original context.

One of the reasons for postulating a similarity of meaning for so many animal species in mythology is their consistent behaviour. However the Amarakaeri put this axiom into question. Roe in his general model contrasts two aspects of the jaguar. It can be 'yellow' and potentially beneficial or 'black' and harmful. It lives on the dry land but spends much of its time in water, and so it is a mediator. The Amarakaeri, on the other hand, say that jaguars of all kinds are manifestations of harmful spirits. They belong to the forest and never go near water. They even have stories telling of people escaping from jaguars by leaping into the water. We know that this is contrary to the facts of animal behaviour and yet the Amarakaeri say that this is the case. This would go to show that animal behaviour may not be a 'given' but can be reinforced by cultural preconceptions of what that animal behaviour ought to be.

Anthropologically Roe's thesis is the direct opposite of the structuralist canon. Whereas Lévi-Strauss, with only a few exceptions, says that content is nebulous and forever changing while structural relations remain more or less constant, Roe puts forward the idea that relationships are always changing but that these only cause semantic shifts around a central core of a symbol which remains constant. He describes in Chapter One a 'style cycle' through which dyadic relationships semantically pass from metonymy to simile to metaphor to synonym. The relationship between these two approaches is very much that of the capon and the egg.

Roe says that there is a basic principle to the model which justifies and explains the myths. It lies in the social organisation of lowland societies and consists of the complementarity and antagonism between the sexes. Men and women are sexually dependent on each other. Men are the positive half of the dyadic relationship whereas women are negative or they represent both *eros* and *thanatos*. This duality, says Roe, is the central core of all South American cultures and all their mythology is a commentary on or elaboration of this theme. However women are not always negative nor men positive. For the Amarakaeri one of the most feared images is that of a good-looking young man intent on seducing their women.

Without doubt same and opposite sex distinctions are crucially important in lowland societies as are sexual relations or their absence, but there are other factors in social organisation such as relationships of age, residence, marriage or reciprocity, to name only a few. In linking mythology to social organisation, a discussion on mythology as ideology and as instrumental in power relations would have been very illuminating.

Finally, Roe sees the endless succession of mythological characters as resulting from the desire for metaphorical novelty. He likens this to the images in our culture such as Superman and Batman who 'do fairly much the same thing'. The Amarakaeri too have heard of Superman and Batman and they would talk of them in stories sometimes as representatives of some harmful multinational company, sometimes as champions of native rights. The characters all had similarities but what they meant altered according to the context of the story.

There are some useful points to be gained from reading *The Cosmic Zygote*. Looking at similarities can be fruitful, provided one takes context into consideration. The notion of relationships shifting and consisting of a dynamic relativity is very interesting, provided that one realises that content is not really going to present a particularly reliable constant. In spite of many reservations it could be that there is more to the book than meets the eye and that it is in fact 'caviar for the general'.

ANNE CHAPMAN, *Drama and Power in a Hunting Society: The Selk'nam of Tierra del Fuego*, Cambridge etc.: Cambridge University Press 1982. xiv, 183pp., Bibliography, Index, Photographs. £19.50.

The research upon which this book is based could no longer be done. The last few descendants of the Selk'nam and Haush Amerindians of Tierra del Fuego, who were Anne Chapman's informants over a fifteen-year period, have been dying one after the other in old age. Her ethnographic reconstruction of these hunters and gatherers' society and culture, decades after they were abruptly destroyed by genocide with the advent of White sheep-farmers at the turn of the century, is a tribute to the resilience of a culture effectively 'extinct' as much as to the author's sensitivity and skills.

Long-term research has conferred elegance and lightness to this monograph rather than the cumbersome development of ideas associated with life works. It is scholarly in that testimonies are checked both against each other and against available sources, particularly Gusinde's major ethnography of the Selk'nam, but sources and references are aptly quoted in extensive notes rather than in the text. A direct style, a clear presentation and the use of photographs induce in the reader an immediate familiarity with the Selk'nam. Best of all, the book conveys the unmistakable vigour and the enjoyment of their way of life described among pristine Amerindians in both old and recent sources. In this hunting society, women's perceptions are deliberately brought to the fore through the memories of Lola, the only remaining shaman, and Angela, who became Anne Chapman's friends.

The central subject of the book is the Hain, the Selk'nam initiation ceremony. Given the nature of her material, the author's approach to this ceremony, the last of which was performed in 1937, is particularly challenging: she is intent to show that the Hain, besides being a periodic symbolic expression of Selk'nam social structure, is also a drama where amusement and theatrical performance intertwine with ritual.

A concise but illuminating account of the history, ecology and socio-economic structure of the Selk'nam and their Haush neighbours on the Great Island of Tierra del Fuego provides a background to the description and the interpretation of the Hain. Accepted views of the Fuegians as marginal primitives with a recessive culture in a refuge area characterized by hardship are disproved. Evidence shows that animal resources were plentiful and diversified: guanaco hunters supplemented their diet with birds, sea mammals, shellfish and plant-foods. They alternated between coastal and inland sites within territories, and, as other hunters, between dispersed residence and social gatherings according to seasons and particular events such as the Hain or the finding of a dead whale on the shore.

Rejecting both Steward's notion of the 'patrilineal band' and the more recent bilateral egalitarian model advocated for hunting societies, Anne Chapman convincingly shows that the Selk'nam were 'at once patriarchal, keen on land-rights, status-conscious, individualistic, competitive, egalitarian and flexible'. She

describes an Australian section-like system in which, however, units are classificatory rather than kinship ones. Bilaterally exogamic localised lineages, occupying recognized territories associated with three cosmic divisions, were the main units of organisation. Her main focus of interest is the nature of this 'patriarchy' rather than organisation in general. Among the Selk'nam, the sexual division of labour was the only power cleavage since the differentiated statuses of shamans, sages, prophets or statuses conferred by excellence in a particular activity brought prestige but little or no economic prerogatives.

The Hain, while initiating boys to manhood as hunters, enacted the rule of men over women as a stratum. The existing social order was opposed to its antithesis, matriarchy in a mythical past. Both the men, impersonating spirits in the cosmic womb-Hain hut, and the women, who camped outside, were confronted with man-eating female monsters: both had to oppose them, since there can be no society without men. Mythology was thus an ideology reinforcing the status quo. The main question then is that of the 'secret' or the 'hoax' as Gusinde put it: were men manipulating their power and play-acting to deceive the women, or did they believe in the spirits? Did the women themselves believe in the spirits?

The answer to this question is encompassed in the interpretation of the Hain as ritual and theatre. In the words of Angela, 'spirits did not look like men, you could never tell they were'. The description of the daily scenes of the Hain is gripping by the display of creativity in the variety of forms and colours, the highly developed personalities of the spirits, the roles, the range of tones and moods in the scenes from horror to grotesque and comic. The women participated in the ambiguity of the drama, with awe and respect on the one hand, caricature and fun on the other.

This account of the richness and complexity of the Hain is a remarkable contribution to the understanding of ritual and organisation in hunting societies in relation to material culture. Anne Chapman's materialist functionalist explanation, in terms of a sexual stratification of the relations of production, is given with a light hand and is relatively jargon-free (although one may lament some stylistic errors - 'phallogyny' is a strange neologism if it makes any sense). Whilst theoretical issues are raised but not really taken up, the author could have been expected to show some awareness of comparative material in the subarctic and particularly in Amazonia: Stephen Hugh-Jones's study of He rituals in the Northwest Amazon is listed in the bibliography but is not significantly referred to. With these limitations, this book is a fine monograph which, sadly enough, sets an example for future paleo-ethnography in South America.

FRANCOISE BARBIRA-FREEDMAN

CHRISTINE A. LOVELAND and FRANKLIN O. LOVELAND (eds.), *Sex Roles and Social Change in Native Lower Central American Societies*, Urbana etc.: University of Illinois Press 1982. xix, 173pp., Index. £9.50.

This collection of essays is an attempt to describe the complementary roles of men and women in the less-intensively studied, indigenous societies of Central America. In concentrating on this region, the editors hope to provide a significant and novel contribution to the ethnographic literature and one which will provide a basis for comparison with Mesoamerica proper. Since many of the societies share a common historical experience as well as a common tropical forest environment, factors which are seen as important in structuring sex roles, it is hoped that the comparison of the impact of social change on sex roles, in this particular cultural area, will prove fruitful.

In her introduction, Christine Loveland claims that these studies show that there has been a decline in female status in all these societies in relation to the increasing economic power of males which derives from the change in traditional male occupations, hunting, fishing and subsistence farming, to increasing participation in the national economies as wage labourers. As cash incomes are used to purchase items formerly produced by women, there is a decline in the need for female skills. In all these societies, men are preoccupied with the public, outside domain and are politically dominant while women remain identified with the inside, domestic sphere. Women are more handicapped than in the past because they lack the new mobility and skills which men are acquiring with regard to negotiating with the outside world. Women remain entrenched in the conservative smaller societies. They are less able to influence or make decisions and more dependent economically on their menfolk. Where, however, these societies become more urbanized and educational levels become more similar for boys and girls, change is rapid and radical.

A careful reading of the eight essays does not support these assertions. Loveland demonstrates an ethnocentric bias in measuring women's status in these societies, using a standard derived from Western values. In Chapter One, her own account of a Rama Indian community located on the Rama Cay of Nicaragua, she seeks a theory which would link economics to status. Franklin O. Loveland's more perceptive description of the same community (Chapter Seven) shows, through an analysis of two current Rama myths, that women play an important role in Rama society, not only because they produce new individuals to carry on Rama culture, but because it is women who are responsible for conserving and passing on knowledge. Franklin Loveland sees a parallel between Rama concepts of male-female role reciprocity and those described by Rivière as characterising Trio relationships. He denies that there is any simple female-nature versus male-culture dichotomy and posits instead a paradigm where males as food producers (crop-growers) are closer to culture while as takers of life (hunters) they are closer to nature. Women as

food processors are closer to culture; as givers of life, closer to nature. Sexuality is the mediator between culture and nature. Disappointingly, he concludes that while Rama women are subordinate to Rama men 'in reality', in the symbolic world Rama conceptualise equality between the sexes. There is no attempt to justify the assumption that economic values (presumably) are more 'real' than ideological ones. It is also unfortunate that this chapter does not follow directly after Chapter One, since together they give a more balanced view of Rama society.

Virginia Kern describes the sexual division of labour among the Black Carib (Garifuna) as do Sheila Cosminsky and Mary Scrimshaw in part of their chapter. The Garifuna were forcibly removed from the island of St. Vincent in the eighteenth century and settled along the coast of Belize, Guatemala, Honduras and Nicaragua, where they have maintained their 'traditional' roles for the last 200 years. The men at home are involved in agriculture, clearing land, planting, weeding and harvesting, fishing and trading - but land is scarce and they are forced to migrate, to seek seasonal work on the *latifundias* developed for agricultural export. The women are 'keepers of tradition', stay at home, are day-to-day cultivators and maintainers of subsistence agricultural plots. They carefully cultivate and maintain relations with kith and kin, and they actively promote a morality that centres on the display of generosity and gratitude to kin. While there are no cultural constraints against women migrating as wage labourers, there is no demand for their labour in the Central American region, where agricultural work is seen (by Europeans and Ladinos) as male activity. Where there is a demand, such as in the USA, Black Carib women do migrate - as domestics for example. Many have, in fact, spent some period of their lives working away from their villages. Cosminsky and Scrimshaw point out that these women gain more respect and exert more influence with age, irrespective of their income-generating capacities. This they claim is true also of the Quiché-speaking Mayan communities they studied, both in the rural western highlands of Guatemala, and the mixed Mayan and Mestizo, coffee-producing, *finca* population on the Guatemalan coast. In all three communities, a woman's status is validated by becoming a mother. Children are not only valued economically, but are essential for a woman to gain respect and influence within a community.

Chapters Four and Six, (again irritatingly separated) by Richard Costello and Margaret Byrne Swain respectively, deal with sex roles in two Cuna communities. Among the Cuna Indians of the San Blas archipelago, off the northeastern coast of Panama, males, historically, were agriculturalists primarily and fishermen secondarily. Women were totally involved in the domestic sphere. A system of female inheritance and uxorilocality enabled women to maintain some control over households although it was the male head of household who organised labour for subsistence. In Rio Azucar, increasing labour migration of men, since 1900, has resulted in a shift to neolocal residence patterns and male ownership of homes. Cash remittances also now go more often to the

man's parents and siblings than to his wife's. However, higher female academic achievement now enables women to enter the labour market, and in urban neolocal households adult members expect to contribute and pool their incomes. Where men and women are both active in the labour market, older children assist in household tasks and child care. In the other Cuna village, Ailigandi, a strong all-male authority, the *congreso*, enforces traditional norms. Women appear to be more conservative, although they are more aspiring, than men. Ailigandi women can earn more money from their *molars* (embroidery work) than from unskilled labour and there is less incentive for them to migrate unless they are highly skilled. A hospital on the island provides an opportunity for skilled, well-paid work within the community, and women employed there often retain their traditional values. Margaret Swain's description of the Ailigandi Cuna community illustrates the complexity of assigning values to gender roles and the fallacies generated by using a single denominator, such as income.

In another essay, John Bort and Philip Young assess changes in male roles in Guaymí society, the largest group of Amerindians in Panama. They describe a growing dichotomy between the men who stay behind as subsistence farmers and those who leave to become wage labourers. For the women who remain, there appears to be little role change; men dominate and control relationships and transactions between households and the outside world but, with the disappearance of the 'Edabli Ritual Complex' and the associated goal of surplus production, there is less cooperation among kinsmen and less security against subsistence shortages.

In the final chapter Buzzali de Wille describes symbolic aspects of sex-role differentiation among the Bribri Indians of southern Costa Rica, where pregnant women and male buriers are subject to numerous behavioural restrictions and obligations. De Wille is not at all concerned with social change. Her interpretation of the significance of the rituals associated with birth and death is characterised by the female-nature male-culture opposition. Concepts of pollution associated with liminal events also colour her analysis. While it is difficult to assess the adequacy of her interpretation in this brief account, the ethnographic evidence that de Wille presents of this little-known society does serve to whet the reader's appetite. A collection of such essays cannot expect to do more.

ANN ELIZABETH FINK

VICTORIA REIFLER BRICKER, *The Indian Christ, The Indian King : The Historical Substitute of Maya Myth and Ritual*, Austin: University of Texas Press 1981. xiv, 181pp., Appendices, Bibliography, Illustrations. £29.25.

Bricker attempts to interpret inter-ethnic relations in the Maya provinces of southern Mexico and Guatemala by recourse to the consciousness of the structure of events a) according to the Ladinos' historical view and b) by the cyclical and prophetic historiography of the indigenous Maya. Bricker's most interesting point is that the Maya prophetic tradition, which interprets events as essentially repetitive occurrences of a finite and ancient corpus of archetypes arranged in cycles, appears to have had a direct influence in determining historical events. The primary evidence is drawn from Ralph Roy's notes accompanying his translation of the Book of Chilam Balam of Chumayel, in which he states that the successful conquest of the Itza Maya was achieved only as late as 1696, when they invited Spanish missionaries to spread Christianity among them and suddenly withdrew all resistance. Previously they had told the Spanish that they were not then ready to receive Christianity as the time foretold in their prophecies had not yet arrived. Despite the conquest of Guatemala being achieved in 1525 and the conquest of Yucatan in 1541, the Itza fought off all attempts at conquest and spiritual domination. Then in 1696, the completion of the cycle prompted a delegation of Itza to request the governor of the province to send missionaries in order to effect their conversion to the new religion.

The second instance is that of the 1847 Caste War of Yucatan, the most successful Maya uprising of post-conquest history. Bricker points to the prophecy of the second coming of Christ foretold in the Book of Chilam Balam for the cycle katun 9 Ahua. She contends that the form of the actual uprising and also its timing corresponded to the description and period of the prophecy.

Unfortunately Bricker does not follow up the implications of her deductions, for she uses rather orthodox empiricist methods to account for other colonial and post-colonial indigenous rebellions, without further investigating the significance of her previous formulations. She herself notes that this evidence suggests that the Maya have shaped a peculiar 'historiographical' science which presents the structure of events as following and repeating certain archetypal acts. Such a view has shaped the Maya understanding of the world and has provided an interpretation by which contemporary inter-ethnic encounters are understood.

The Maya have been dominated by a distinct philosophy of time which has shaped their world view and by which their own actions in the 'face of history' have been orchestrated. The case of the Itza demonstrates the willingness of the Maya to intervene in history to secure the realization of prophecy, even when such intervention involves the consummation of their traditional life with eschatological implications. The prophecy of the Second Coming of Christ and the subsequent Caste War demonstrates an equal

ability to manipulate resistance struggles against colonial relations - in keeping with ancient prophecy. Cyclical conceptions of history are not necessarily conservative or static, as has been argued by many.

Guzman-Böckler and Herbert (*Guatemala : una interpretación histórico-social*, Mexico 1970; *Colonialismo y revolución*, Mexico 1975) have noted the tendency among contemporary Quiché communities of highland Guatemala to divide their history into two great ages, corresponding to pre-conquest and post-conquest history. The two are placed in mutual opposition and there is a tendency to collapse events involving inter-ethnic relations into archetypal structures by which the present is characterised. Contemporaneous with this is the vitality of the belief in the return of Tecun Uman, the last and heroic chief of the Quiché who was slain in battle by the Spaniards, and the re-opening of pre-Conquest history. Guzman-Böckler and Herbert have argued that the period from the Conquest up to the present is almost viewed as empty, as if the Maya think of themselves as without history - as a means of suppressing their consciousness of oppression. But this too has been foreseen as terminating within the prophetic cycles, which should thus be considered when trying to understand the massive incorporation of the Maya population within contemporary revolutionary movements in Guatemala.

Bricker's contention regarding the determinacy on action of the Maya conception of time is of extraordinary pertinence. The motivation of human action is variable, depending on the value system intrinsic to ideology. It is a gross error to interpret history according to any pre-conceived historiography.

The Huicholes, an isolated indigenous culture of northwestern Mexico, refer all their activities to a religious inspiration which often involves the direct courting of material deprivation to attain spiritual growth. The principle of Huichole life is the felt necessity to continue the work of the ancestors as creators. This is a profoundly spiritual endeavour, and the only suitable explanation of their actions is that derived from the structure of their value system and the logic it contains. History has meaning only in terms of its own particular logic. Anything greater than this can only be extrapolated from the former.

With few exceptions, Maya history has been subject to materialistic determinations in accounts of the civilization's rise, development, and fall. Explanations have assumed the existence of a natural logic of development in some way dependent on ecology. Perhaps, more than this, the explanation of the sudden desertion of the rich Maya cities, the intentional desecration of monumental art and the sudden loss of the classical Maya civilization is to be found in the logic which governed the Maya realm, and particularly in the calendrical prophecies as the principal religious thread of social activity.

## LETTERS TO THE EDITORS

Dear JASO,

Who is this J.D.H.Collinson who claims to know me well? (JASO XIII:3, page 288). Could it be the John ('Basher') Collinson I saw last disappearing down a dark alleyway in Tabora, suitably escorted, one wild night in 1963? I had thought it sufficiently apparent that my little *jeu d'esprit* entitled 'Seeing Africa' was a spoof exercise in structural analysis à la Lévi-Strauss, spiced with a modicum of social satire. The binary wings of the albatross flap heavily indeed in the post-structuralist heaven.

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Dear JASO,

Perhaps I can provide ethnographic information relevant to the interpretation of two articles published in JASO by R.H.Barnes (Vol.XI,1980) and G.Forth (Vol.XIII, 1982). In western Rasanaé on the island of Sumbawa, Indonesia, the children play a game in which the fingers represent five different characters. The thumb and little finger are called 'old man' and 'small man' respectively, while the versatile index finger is the 'craftsman'. Because the Bima speakers of Rasanaé wear the ring on the fourth finger it is named the 'rich man'. If the hand is held upright, the middle finger is taller than the others and therefore it is termed 'leader' or 'lord'. Adults show children that should any finger wish to consult (i.e.touch) the thumb it must bow down, thereby demonstrating that all people, including leaders, defer to age in Biman society. Children go on to use this stock of characters to invent further entertainment for themselves and their friends.

In his article, Barnes showed that the fourth finger was unnamed both in Kédang and elsewhere. Forth observed this to be the case in parts of Sumba in eastern Indonesia and supposed that this might have something to do with the undistinguished nature of the fourth digit. But in north-central and the east of west Sumba, he tells us, it is the middle finger which is unnamed. Since the middle finger is not undistinguished, Forth felt that the explanation given for the unnamed fourth finger was insufficient, and therefore sought

connexions between the qualities of the middle finger and themes in other areas of Sumbanese symbolic thought. He suggested that since centres or middles are sacred in Sumba and divinity is unmentionable and the middle finger has both properties of mediality and profanity, then it is consistent that the finger should also be unnamed. My own fieldwork on Sumbawa, which lies just north of Sumba island, shows that there are difficulties in applying this kind of reasoning to explanations given by the people themselves. As was mentioned above, the middle finger is named 'leader' or 'lord' in the Biman children's finger game. Both adults and children say that the finger is so called because it is the tallest, and more particularly, its 'head' is higher than those of the other fingers, which in local terms, indicates social superiority. Now in Biman symbolic thought centrality is associated with political and spiritual power, and it occurred to me that this might also be the reasoning behind selecting the middle finger as the 'lord': but the mediality of the middle finger was not the significant feature, despite the fact that that characteristic also suits the explanation. Furthermore, the children's finger game makes much of the relative heights of heads, as in the case of bowing to the 'old man', while not considering the issue of centrality at all. The point is that the ethnographer's model, while providing explanations within the terms of that culture, might emphasize different features from those emphasized by the people themselves.

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Dear JASO,

As an anthropologist concerned with analysing the social patterns and practices of the so-called European 'Middle Ages', I feel obliged to offer a rejoinder to Dr Felipe Fernandez-Armesto's conceptions of 'medieval ethnography' put forward in the Michaelmas 1982 issue of JASO (Vol.XIII:3).

Dr Fernandez-Armesto seems to assume that historians can 'turn the table on anthropologists' by simply applying the methodology of historians to the 'matter' of anthropology. The inversion is, unfortunately, not such a simple process; anthropological 'looks at history' tend to be concerned with the way history (a social construct rather than an empirical series of events) is perceived and manipulated by its fabricators, whereas, as Dr Fernandez-Armesto seems to indicate, a historian's perception of the 'historical origins' of anthropology seems to focus on the development of an empiricism meaningless to peoples not nurtured on nineteenth-

century conceptions of scientific objectivity. Medieval travellers certainly had ethnocentric categorical pigeon-holes in which they stored the *exotica* collected on their journeys. It would, however, be similarly ethnocentric to label their conceptual frameworks as racialist or fantastic; those frameworks were organized around the central concerns of their societies (the Christian faith, a theocratic and feudalistic social organization, military power and defence, etc.), rather than around empiricist ideals which evolved centuries later under the influence of imperialistic and scientific attitudes towards the 'outside world'. 'Disinterestedly curious motives' would not arise in such a cultural context, as Fernandez-Armesto himself acknowledges in noting William of Rubruck's 'missionary interest in native religion' and later 'closet ethnographers' debts to the Llullian tradition of 'understanding thoroughly the culture in which one proposed to work as a missionary'. A traveller like Marco Polo, who was, in fact, closer to the sort of objectivity propounded by the historian because his interests in trade routes, rates of exchange and regional products were relatively non-valuational, was considered 'a travelling fable-monger' or *milono* (man of a million lies) because his experiences did not accord with popular conceptions of the world (cf. *Book of Ser Marco Polo*, Henry Yule. ed., Vol. I, p.lxxxii) and actually did offer 'a radical challenge to prevailing notions about the nature and rights of man'. Fernando-Armesto's failure to see Marco Polo's *Travels* as a profound critique of the provincial inefficiency of a decentralized Europe, and the historian's willingness to classify Marco Polo among the 'cowards' who refused to look at the real world are results of his own inability to loose his perceptions from the bonds of 19th-century empiricism and look at the world the way medieval persons would have perceived it.

The lesson that anthropologists can teach historians is that a people's or a period's world is an intricately woven web of meanings and actions which derives its significance from the pattern of the whole. Particular designs or motifs, like the odd tales of travellers or the messages of missionaries, must be analysed by studying the way they are woven into the wider web of culture. To tear them out of the fabric and transport them off to a laboratory where they can be studied as empirical facts will only lead to their being reweven into another period's web in which, for instance, medieval travel narratives are seen as expressions of 19th-century empiricist methodologies.

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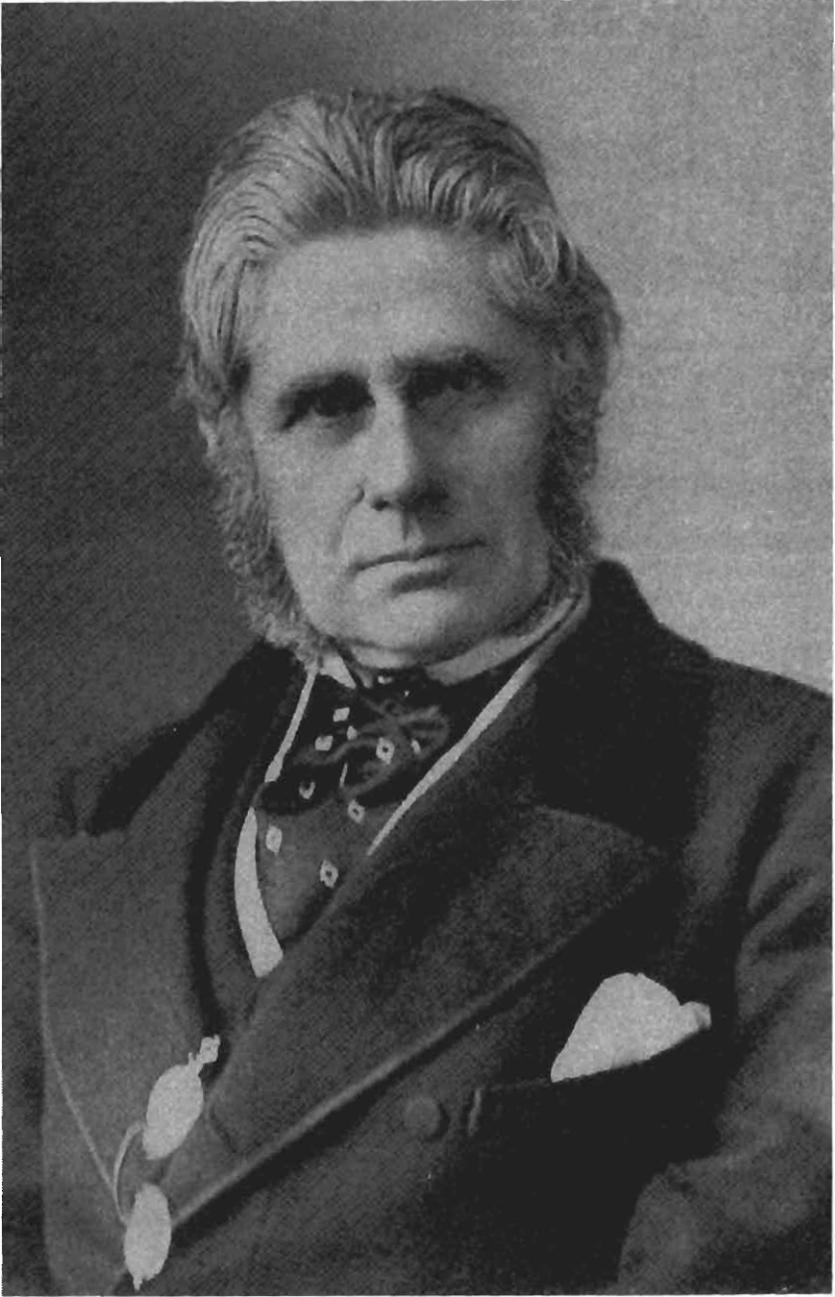
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SPECIAL SECTION TO COMMEMORATE THE CENTENARY OF THE FOUNDING OF THE  
PITT RIVERS MUSEUM, OXFORD: Editors' Introduction

The Pitt Rivers Museum has had a peripatetic history. From its humble origins as the private collection of Lieutenant-General Augustus Henry Lane Fox Pitt Rivers, F.R.S., in 1851 it was housed periodically in London's Bethnal Green Museum (in 1864) and in the South Kensington Museum (in 1878) before finally coming to rest in the University of Oxford in 1884. This year marks the centenary of the establishment of the Pitt Rivers Museum in Oxford. From the General's original gift - covenanted in 1883 and accepted by the University's Hebdomadal Council the following year - the Museum has survived to become one of the jewels of Oxford. As our contribution to the centenary celebrations, the editors of *JASO* present here four articles that reflect the Museum's history and the symbiosis between the study of material culture and social anthropology.

The Editors of *JASO* record their grateful thanks to the Officers and Fellows of the British Academy from whom a generous grant has been received making possible the publication of these Pitt Rivers papers. The British Academy has also helped with the simultaneous publication, by *JASO*, of a volume entitled *The General's Gift: A Celebration of the Pitt Rivers Museum Centenary (1884-1984)*. Edited by B.A.L.Cranstone and S.Seidenberg this book contains the Pitt Rivers essays published in this issue together with further essays by Elizabeth Edwards ('Collecting with a Camera: Pitt Rivers Museum Photographic Collections'), Helene La Rue ('The "Natural History" of a Musical Instrument Collection') and Deborah Waite ('The H.B.T. Somerville Collection of Artefacts from the Solomon Islands in the Pitt Rivers Museum'). We owe a great debt of gratitude to Mr.B.A.L. Cranstone, the Curator of the Pitt Rivers Museum, and to Miss Julia Cousins, the Administrator of the Department of Ethnology and Prehistory and of the Pitt Rivers Museum, for their support and assistance throughout this project.



AUGUSTUS HENRY LANE FOX PITT RIVERS

1827-1900

PITT RIVERS AND HIS COLLECTION, 1874-1883:  
THE CHRONICLE OF A GIFT HORSE

I. *Introduction*

The management of a newly acquired collection can be an irksome responsibility, as any modern museum director or curator knows. Often the problems of maintenance, arrangements for display, the need for funds for new staff and countless other considerations outweigh any possible benefit which might seem to accrue to an institution as the result of a chance gift or bequest. This is particularly true when the donor is still alive, and intent on having some influence upon the arrangement or care of his or her former property, as is so often the case. Museum collections also seem to make demands in their own right. Objects cannot simply be set aside or 'put on ice' while decisions are made about them. There are often immediate conservation needs. Damaging insects must be eradicated; fungi must be identified and removed. There are also cataloguing requirements. Objects must be 'accessioned', tabulated, placed in 'retrievable storage systems'. Then there are public and institutional concerns. Boards of directors, often again under pressure from donors, require that collections be put on exhibit, that catalogues and monographs be published. Members of the public, as well as specialized researchers, demand 'access'. The result is often

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The research on which this article is based was first presented in my D.Phil. thesis *Ethnology in the Museum: A.H.L.F. Pitt Rivers (1827-1900) and the Institutional Foundations of British Anthropology*, University of Oxford 1981, which should be

regret at ever having accepted the collection at all. Sometimes, in fact, steps are taken to ensure that the collection is *not* accepted, as a way of avoiding the attendant problems.

The dilemmas of modern museum managers are no different from those of their counterparts 100 years or so ago. We tend to assume that in a simpler, more optimistic Victorian era things somehow went more smoothly. There were, of course, numerous collectors - and hence donors - as there are today. Many, moreover, held philanthropic motives very close to their hearts. Nonetheless, the problems and doubts still existed. Among

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consulted by the reader for references to the material presented here. Background information on Pitt Rivers and his museum can be found in T.K.Penniman, *A Hundred Years of Anthropology*, (1935), London: William Morrow 1974; T.K. Penniman, 'General Pitt-Rivers', *Man*, Vol. XLVI, 1946; T.K.Penniman, 'A Note on the Beginning of Anthropology in Oxford', in *Anthropology at Oxford*, F. Oxford Univ. Anthr., 1953, pp.11-14; E.B.Tylor, 'Pitt Rivers', in *Dictionary of National Biography*, 1917; H.St.G. Gray, 'A Memoir of General Pitt-Rivers', *Proceedings of the Somerset Archaeological Society*, Vol. XLVII, 1900, pp.123-137; *idem*, *Index to 'Excavations in Cranborne Chase...'*, Taunton Castle: privately printed 1905; *idem*, 'Lieutenant-General Pitt-Rivers', in *Memoirs of Old Wiltshire*, London: Bemrose 1906, pp.47-65; *idem*, 'Recollections on the Occasion of the Five-Hundredth Meeting of the Oxford University Anthropological Society', in *Anthropology in Oxford*, 1953, pp.3-5; B.Blackwood, *The Classification of Artefacts in the Pitt-Rivers Museum Oxford*, Oxford: Pitt Rivers Museum 1970; M.W. Thompson, *General Pitt Rivers*, Bradford-upon-Avon: Moonraker 1977; M.W. Thompson, *Catalogue of the Pitt Rivers Papers in the Salisbury and South Wiltshire Museum*, 1979. For a more complete bibliography see Chapman, *Ethnology in the Museum*.

I would like to thank the following for their advice and assistance on this portion of my work: P.Glenn, J.Hull, K. Davies, F.Nicholson, P.R.Saunders, J.Price, A.Smith, R.Vyse, L.Williamson, R.G. Lienhardt, W.James.

The following archives are the ones I relied upon for my research and I would like to thank their respective institutions for permission to refer, and in some cases to quote from,

unpublished material: Bethnal Green Museum (archives); The British Library (Sotheby sales catalogues); The British Museum (archives); The Dorset County Record Office; The Pitt Rivers Museum (Balfour Papers); the Salisbury and South Wiltshire Museum (Pitt Rivers Papers); T.H.Aston, Keeper of the Archives, University of Oxford (Hebdomadal Council Papers); University Museum, Oxford (Westwood Papers); and the Committee for the Ashmolean Library (Rolleston Papers).

anthropological collectors, Henry Christy (1816-1865),<sup>1</sup> the quiet-spoken proponent of Aboriginal rights, gave the British Museum its first comprehensive collection of ethnographic and prehistoric pieces in 1865. But for many years the collection had to remain in Christy's apartments in Victoria Street, watched over by a privately paid custodian in the employ of A.W. Franks (1826-1897), head of the then multifarious Department of British, Medieval and Oriental Antiquities and Ethnography, and very much unwelcomed by Bloomsbury. George Peabody (1795-1869), the noted Anglo-American philanthropist, and proponent of model workers' housing, searched for many years for a recipient for his collection of American antiquities before it was accepted by Harvard in 1866. Then Peabody had to provide both a lecturer and a building to house the collection before Harvard could consider the offer. The same was true of numerous other collections, both of objects of scientific interest, as anthropological collections were generally considered, and of the more easily accommodated art collections of the period. In short, a donation was seen more as a liability than a benefit. If not a gift horse, a 'white elephant' or 'albatross' might equally apply.

The ambivalencies inherent in museum custodianship are no better illustrated than by the circumstances surrounding the donation of the well-known Pitt Rivers collection to Oxford University in 1883. Pitt Rivers'<sup>2</sup> collection was, at the time, one of the best known in England. It consisted of some 14,000 items collected over nearly a 30-year period beginning around 1851, or the time of the Great Exhibition from which in many ways it took its lead. Initially, it was a military collection reflecting Pitt Rivers' career as a professional soldier. One of the first 'series', as he called the collection's divisions, was of muskets and other firearms illustrating 'the successive steps by which the rifle has attained its present efficiency'.<sup>3</sup> Other series followed a similar pattern, beginning with bows and arrows, then shields and throwing-sticks, down through more elaborate weapons and related objects, such as flails and early

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<sup>1</sup> Biographical information, unless otherwise specified, is from the *Dictionary of National Biography*.

<sup>2</sup> Until 1880 Pitt Rivers was known by the name of Lane Fox, more commonly Fox; the name-change was occasioned by the conditions of a will (that of the 2nd Lord Rivers). His forenames were Augustus Henry. This makes for some confusion when writing about him or in compiling a list of his works. For convenience and clarity I have referred to him as Pitt Rivers throughout the course of this paper and have listed his works in the same way as well.

<sup>3</sup> A.H.F. Pitt Rivers, *The Instruction of Musketry*, Hythe: Hythe School of Musketry 1856, p.7.

bucklers,

Sometime during the late 1850s, Pitt Rivers began to add objects of a less martial character, including a number of items illustrating what he referred to as the evolution of an ornamental form. Many were acquired during his travels, mostly in Europe and the Mediterranean, while others were presented by travellers and adventurers as they returned to London, often presenting their findings at one of the several scientific associations with which Pitt Rivers was associated. Pitt Rivers himself was not a wealthy man, particularly during his earliest collecting years. Still, his family ties and social connections were decidedly aristocratic, which, in turn, made both his contacts with dealers and other collectors and his purchases easier. Luckily, too, ethnographic objects could be acquired at the time at very little cost.<sup>4</sup>

By the 1860s, Pitt Rivers had begun to identify his collection, then housed at his home in South Kensington, with collections formed by natural historians and compared his own work directly with that of Charles Darwin. Around the same time he began to add to the antiquarian side of his collection, an important feature in several ways from the first, both through further purchases and as a result of his own excavations. During the early 1870s, the latter, in fact, began to take precedence as Pitt Rivers' career began to take a more decidedly archaeological direction.

In 1874, with the new appointment as head of the Brigade Depot in Guildford, he decided to pass the collection on to a public body, initially to avoid the expense and trouble of maintaining it in London, but also as a way of further promoting his ideas to the general public and to fellow anthropologists. His choice initially was the newly-established Bethnal Green Museum, both because of its convenience and because as a new institution it seemed to be more amenable to his dictates. He also had had some contact with officials there through an earlier loan exhibition. The new move was to be on a loan basis as well, as a way of reinforcing his continued interest in the collection. Pitt Rivers would soon discover, however, that even a new and uncertain institution such as Bethnal Green could begin to make demands in its own right.

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<sup>4</sup> One anthropologist writing in the 1870s remembered when, not long before, a South Seas club could be obtained for 'an empty beer or pickle bottle' (see 'Godefroy Museum in Hamburg', *Journal of the Royal Anthropological Institute*, Vol. IX (1879), p.462). The cost in London shops was not much higher (see Chapman, *op. cit.*, pp.40-41).

## II. *The museum at Bethnal Green*

The Bethnal Green Branch Museum to which Pitt Rivers was to transfer his collection represented an attempt by the Commissioners of the South Kensington Museum to extend their influence to parts of London which had never received the full benefits of an educational institution such as that at South Kensington. The facility was literally a South Kensington cast-off, consisting of several prefabricated cast-iron structures, popularly known as the Brompton Boilers, left over from the International Exhibition of 1862 and made obsolete by the new building programme in South Kensington. Sensing the opportunity for their re-use Henry Cole (1808-1882), for many years Director of the South Kensington Museum, had pressed for their re-erection at Bethnal Green, an area of east London noted for its poverty. Other civic leaders, including Sir Antonio Brady (1811-1881) and the Reverend Septimus Howard, had joined him on behalf of their re-use. In 1871, Major-General Scott was induced to provide a new bright-red brick-front, a feature completed in the summer of 1872. The first exhibition was of Sir Richard Wallace's collection of paintings, pottery and porcelain figurines, displayed with the aim of conveying the history of art as well as of providing models for the area's craftsmen (many of them descended from Huguenot weavers). The second show, held the following year, was a display of various animal and vegetable products, tracing their origin, evolution and in many cases their etymological derivation. In terms of their general theme, therefore, both displays closely approached Pitt Rivers' own collection and can be considered as tantamount to harbinger.

Pitt Rivers made his final arrangements with the South Kensington authorities during the winter of 1874. The collection was finally set up during the late spring of the same year. Most of the work was apparently carried out by the curator at Bethnal Green, G.F. Duncombe, a figure with whom Pitt Rivers exchanged occasional letters. In its final arrangement, the collection exemplified many of the newest ideas in display and organization. New display cabinets were employed, both standing cabinets and desk cabinets, and modern descriptive labels were included for each display. A.W. Franks, of the British Museum, and John Evans (1823-1908), a noted numismatist-anthropologist and close friend of Pitt Rivers - both of whom earlier had been induced to provide materials from their own collections (Franks of ceramics and Evans of flint implements) - were also consulted by Pitt Rivers prior to the transfer of his own collection.

Pitt Rivers' catalogue for the collection, completed only after his move to Guildford, provides a fairly detailed picture of the collection and its extent at the time. The first segment of the exhibition was devoted to skull types and other physical features including samples of skin and hair. Drawings, reminiscent of the pioneering ethnologist James Cowles Prichard's

gallery of ethnological types, supplemented actual specimens. Among the more interesting pieces were casts of the Neanderthal skull, presumably obtained through Thomas Huxley (1825-1895), again an acquaintance of Pitt Rivers, along with a number of modern Australian and, interestingly, Irish skulls, brought together, as Pitt Rivers emphasized, for comparative purposes. To underline his scientific allegiances, a number of primate skeletons and portions of skeletons were also on display, along with those of 'primitive' man.

The second part of the collection was 'Weapons', beginning with his display of throwing-sticks and parrying-shields and proceeding to shields of more recent manufacture, including a number obtained from the sale of the collection of Samuel Rush Meyrick (1783-1848) or illustrated in Meyrick's *Ancient Armour*. Body armour came next, augmented with actual examples from Meyrick's collection, as well as examples of Japanese and Chinese armour and more recent examples of chain-mail. Included among the latter were examples manufactured by British founders for export to India and elsewhere in Southwest Asia. The next series was head-dresses, followed by his more complete series of boomerangs and clubs, again accompanied by illustrations. Displays of blowguns, darts, bows and arrows, crossbows, flails, canoe paddles and finally halberd spikes, swords, daggers and bayonets completed the section.

The remainder of the collection remained uncatalogued. Only a summary was provided in Pitt Rivers' published catalogue of that year, although the latter still provides a fairly good idea of the collection as it existed at the time. 'Part 3', also treating essentially ethnographical materials, was perhaps the most diverse. Included were examples of pottery, tools, clothing, glassware, leather ornaments and items associated with religious practices. Reminiscent again of the Great Exhibition and many subsequent industrial or commercial exhibitions, there were also practical demonstrations, such as looms to demonstrate weaving techniques and a step-by-step visual description of methods of hafting stone implements in various parts of the world, among other displays. Overall, the latter was the most heterogeneous segment of the collection but also the most coherent or integrated.

Probably the most thorough as well as the best-documented of Pitt Rivers' early displays was that dealing with early modes of navigation. Never actually described in catalogue form, Pitt Rivers nonetheless discussed that portion of his collection in a paper of the same title presented before the Anthropological Institute later that year.<sup>5</sup> From his paper it is apparent that

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<sup>5</sup> A.H.L.F. Pitt Rivers, 'On Early Modes of Navigation', *Journal of the Anthropological Institute*, Vol. IV (1874), pp.399-435.

there were five major divisions:(1) dugout canoes, (2) simple rafts, (3) bark canoes, (4) vessels of skins or wickerwork, and (5) outrigger canoes. Outrigger canoes were, in turn, subdivided into categories based on various minor features, such as types of sails or rudders, keels and so on. Finally there was a display of modern vessels, in broad terms not unlike those on display at the Admiralty Office or more recently established at South Kensington. The most important feature of the primitive navigation display, however, was the overall emphasis on the geographical distribution of canoe and ship types. In each case, distribution maps supplemented other displays, tracing, for example, the spread of the bark canoe over North America or the use of outriggers in the South Pacific. As with the series on the degeneration of art, Pitt Rivers' aim was clearly to provide a picture of the diffusion of cultures and material traits, emphasizing the 'amount of intercourse that took place across the sea in prehistoric times'.<sup>6</sup> It is in fact, through such a well-developed and documented series of the kind recorded by his primitive navigation collection that Pitt Rivers' own research ambitions for his museum become most clear.

The best indication of Pitt Rivers' attitude toward the custody of his collection during its first years in Bethnal Green lay in his approach to acquisitions. New materials were periodically added to the collection by Pitt Rivers; Duncombe was expected merely to set them up. The additions are extremely well-documented, the museum staff having carefully recorded each item as it was transferred. Unfortunately, the earlier 'Day' or 'Van' Book at Bethnal Green has been lost, and the record, therefore, begins only in the latter part of 1875. The South Kensington receipts, however, date to 1st January 1874, when the collection was initially deposited in Bethnal Green, and extend to the end of the summer of 1879. As a result, they provide a remarkably accurate record of the collection as it appeared during those years, and a good indication of its extent at the time it was presented to Oxford.

The first major transfer after the exhibition was opened during the summer of 1874 was a mixed number of West African and Japanese materials transferred on 24 July 1874. Other similar materials, sometimes linked thematically, other times organized according to their place of origin, arrived nearly every other month for the duration of the loan period. Whether all of the items were recent additions, or simply materials not previously transferred, is less clear, although at least in some cases - judging by the large number of thematic groupings - they must have been part of his earlier collection. There appears, nonetheless, no evident order to the transfers; such items as birch-bark canoes were as apt to follow examples of African ornament as models of prehistoric sites. Many are recorded as 'Brought in

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<sup>6</sup> *Ibid.*, p.435.

by hand by General Lane Fox'; others were evidently delivered by those working for him, such as his secretary or a clerk named B.M. Wright.

### III. *Transfer of the collection to South Kensington*

In late October 1878, the receipts for the Pitt Rivers collection no longer list Bethnal Green, suggesting that the transfer to the main museum building at South Kensington had already begun. The existing day-books would appear to confirm such an assumption as well. For Pitt Rivers it was obviously his first step toward a more permanent solution to the management of his collection, and it is clear that he had pressed for the change, despite the fact that Bethnal Green was obviously to lose what Pitt Rivers himself saw as a valuable adjunct to the community's educational life. South Kensington, however, offered a better opportunity. Expanded considerably during the late 1860s and early 1870s through the addition of a lecture theatre, refreshment room, and the famous Square Court, the South Kensington Museum was the ideal location for a collection of the type represented by that of Pitt Rivers. While Pitt Rivers himself resented the so-called 'aesthetic' flavour of the institution, he was equally willing to reconsider his own assessment, in view of his eventual ambitions. South Kensington was simply the most practical place to which his collection could be moved. His return to London, his having retired from his Guildford post, merely made the actual process far simpler.

The details of the transfer can be reconstructed with relative accuracy. Pitt Rivers' and his family's own move back to London, initially to Sussex Place and then to Earls Court, in the autumn of 1878 coincided roughly with the reinstallation of his collection, and it was probable that he was on hand to supervise at least the beginning of the transfer. The museum assigned Richard Thompson, the Assistant Director and hence a member of its curatorial staff, to undertake the responsibility for arrangement. Pitt Rivers was evidently satisfied with the Commissions' choice, and had left for France on a four-month expedition recording ancient monuments before the job was completed. The collection was placed in two of the larger rooms of the museum's new west gallery, following, it appears, roughly the same scheme as that at Bethnal Green. The fact that a new edition of the catalogue, published in 1877, was still intended to serve as a guide helps to bear this out. On 21st December, Thompson finally wrote to Pitt Rivers: 'Your collection was opened for public inspection Thursday last - at South Kensington,

and looks well in its new home'.<sup>7</sup> From that date, until over five years later, it was to remain a standard attraction at South Kensington.

In the meantime, Pitt Rivers continued to add to the collection whenever possible. Toward the end of September, or even before leaving for his trip to France, he obtained the collection of Andamanese implements, belonging to E.H. Man. One of the largest of its type, Man's collection numbered over 400 objects, ranging from harpoons and arms to bamboo water-vessels, woven mats, pottery, fish hooks and clothing. The collection was described at length at a number of meetings of the Anthropological Institute, and, again, Pitt Rivers had stressed the important role material culture could play in reconstructing the histories of the remote peoples represented in Man's collection. As he explained at the time:

In so far as my examination of this valuable collection enables me to form an opinion, there is nothing in the implements of the Andamanese which would lead us to differ from the conclusions arrived at on the grounds of physical constitution and language.<sup>8</sup>

To emphasize his point, Man's collection was placed on display at South Kensington soon after its acquisition, and several copies of Man's monograph, *The Arts of the Andamanese and Nicobarese*, were set out for sale to visitors along with Pitt Rivers' own catalogue. Pitt Rivers, apparently, was responsible for the arrangements.

During his stay in France during the winter of 1878-79, Pitt Rivers' collecting efforts continued unchecked. On 30 December 1879, for example, a parcel - 'not opened' - from M. Paul Recappe, a Paris dealer, was received by Thompson. Other materials, ranging from Bulgarian necklaces (familiar to Pitt Rivers since his own early travels there during the Crimean War) to peasant implements from Brittany, the latter obviously obtained during his tour, were also sent to South Kensington for later inclusion and display there.

Prehistoric materials also continued to play a major part in the collection. Again, a number of objects were collected in France and sent on immediately. Others were purchased in Denmark

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<sup>7</sup> R.A. Thompson to Pitt Rivers, 21 December 1878, *Pitt Rivers Papers*, Pl21, Salisbury and South Wiltshire Museum.

<sup>8</sup> A.H.L.F. Pitt Rivers, 'Observations on Mr. Man's Collection of Andamanese and Nicobarese Objects', *Journal of the Anthropological Institute*, Vol. VII (1878), pp.434-451, at p.450; *Pitt Rivers Papers*, P46a-49, Salisbury and South Wiltshire Museum.

the following summer when he and George Rolleston, his close friend and associate, visited there. Finally, a number were obtained through his own excavations, both in France and Denmark. Indeed by the late 1870s such procedure had become standard practice and, with the exception of his earlier prehistoric materials excavated under the authority of the Anthropological Institute at Cissbury, some of which were presented to the British Museum, nearly all the pre-historic and later remains excavated by Pitt Rivers were transferred immediately to the collection. Always, excavated materials continued to be supplemented by purchases from antiquarian dealers, despite Pitt Rivers' claim to the contrary, and entries attributing collections to dealers such as Rollin and Feuardent, located at the time near the British Museum, or to Procher and Co., Oxford Street, continued to appear on the South Kensington list.

Actual authority for the collection, in the meantime, remained curiously undefined. Technically, it was still Pitt Rivers' property and only temporarily on loan to South Kensington. On the other hand, the collection was, in an important sense, already in the public domain, subject to interpretation and revision by the South Kensington staff and, therefore, effectively out of Pitt Rivers' hands. Nonetheless, Pitt Rivers continued to exert an influence upon the collection, if only through his periodic additions of new materials. He also continued to advise Thompson and others, suggesting new ideas for series or for changes in display. The situation obviously presented ample opportunity for resentment on both sides; later complaints by Pitt Rivers suggest that disagreements were not unknown - 'and in fact were fairly common'.

Pitt Rivers' occasional arguments with those in charge of his collection at South Kensington served to underline a far more fundamental concern: whether he was planning to make his collection a truly public foundation by relinquishing his ties with it, or whether he was to keep it for himself. It was a decision that Pitt Rivers had been avoiding for a number of years. Still, something had to be done soon, and it was clear that the South Kensington authorities would no longer tolerate his attempts to retain control over the details of arrangement or add to or subtract from his collection as he pleased. The outcome was, as the Council on Education informed Pitt Rivers in late 1879, that the Museum would have to be given complete control if the collection was to remain on display there. In the light of his changing interests and the fact that he obviously cared less about the day-to-day management of the collection than before, his final decision would appear to have been a simple one.

#### IV. *Negotiations with South Kensington*

In the early part of 1880, Pitt Rivers became suddenly and unexpectedly the heir to a great fortune belonging to his distant cousin Horace, the sixth Lord Rivers. This change of circumstances was to have an important impact upon his life. His home at Earls Court was suddenly exchanged for the Rivers mansion at Grosvenor Gardens near Buckingham Palace. He was also heir to some 30,000 acres of rich agricultural land straddling the Dorset-Wiltshire border which now required his attention. There were prehistoric and early medieval remains spread out over the estate which ranged from Roman camps to Iron Age hill-forts and villages. These also attracted his interest and indeed were to do so increasingly over the next 20 years, as his famous excavations at Cranborne Chase clearly illustrated. In short, his attitudes both toward his professional activities and his collection were to change significantly.

The impact upon his collection was perhaps the most immediate. For the first time Pitt Rivers had the means to purchase in an unrestricted way, and he soon added a number of pieces to the collection, mostly through dealers. He also now had a more ambitious view of its potential. Only a few weeks after receiving notice of his inheritance, he let Richard Thompson at South Kensington know that he would 'extend much more rapidly than hitherto the Ethnographical collection now exhibited at South Kensington'. He was also anxious, as he explained, to provide for a more permanent kind of foundation. Further reflecting his change of status, he stated conditions with greater authority: 'I shall want nearly double the space at once, and if my intentions are fulfilled, more room will be required immediately'. He also offered to pay the costs of an officer or curator assigned to supervise the installation of new materials and explain the series to visitors, that is if the Council on Education (the body overseeing the educational work at the South Kensington Museum) acceded to his other demands. However oddly stated, it was the first formal indication that Pitt Rivers was actually contemplating a gift either to the Museum or to the Government, whichever was willing to accept responsibility.

As a result of his proposal, steps were taken almost at once. The first move rested with the Lords of the Committee of the Council on Education. The Council, in turn, appointed a special committee formally to consider the offer. Richard Thompson and Norman MacLeod of the Museum's Science and Art Department were no doubt asked for their own recommendations. Their first choice was the well-known prehistorian and parliamentarian John Lubbock (1834-1913), and shortly afterward Lubbock was appointed chairman. Other members of the committee included J.F.D. Donnelly, Huxley and Philip Cuncliff Owen; Pitt Rivers had worked with the latter on provisions for the collection when it was still at Bethnal Green. The last two members

were Edward Poynter (1836-1919), the well-known art critic and instructor at South Kensington, and George Rolleston, Pitt Rivers' long-time friend. John Fergusson, the architect and critic, had been proposed by MacLeod, but for some reason was dropped in favour of Franks. The decision, however, appears to have been a strategic one and may have been influenced by Pitt Rivers' own wishes.

Nothing remains of any possible exchange on Pitt Rivers' part with Lubbock or Rolleston, but a letter to Franks, of 27 June 1880, gives an indication of the course of the proceedings. It was obviously not their first communication over the matter and, indeed, there is much in the letter to suggest that Pitt Rivers had spoken at great length to Franks prior to his offer. Moreover, there was a hint that Franks was already disappointed that Pitt Rivers' choice had not fallen on the British Museum, and Franks apparently had already explained that he was prepared to oppose the establishment of a second collection at South Kensington, expressly on the grounds that it would be in competition with Bloomsbury. Pitt Rivers was evidently intent on proving him wrong. His decision, he explained, had been largely one of convenience. Furthermore, his own collection, with its emphasis on 'continuity', addressed a more general educational purpose:

So far from its being antagonistic to the B.M. [Pitt Rivers wrote in his letter] it will be a most useful adjunct. The very wealth of the nation's collections precludes the possibility of their being arranged in subordination to educational purposes. As a means of education to the public the B.M. is useless. I shall supply that want. If you could give me the space I require with a life interest in the management of it I should be very glad but you cannot, and South Kensington can.

His terms, he emphasized, were final ones, and as he implied, he could well afford to hold to them from his present position. He continued in his letter:

If I cannot get more space at South Kensington to enable me to develop my museum on the plan I had developed hitherto the course I shall take will be this. I shall build a museum in or close to London about the size of the room I have at present. Keep the bulk of the collection in trays and drawers and exhibit only a few things in cases but I shall not have space available to continue the series and I shall make the museum valuable in other ways. I shall become a collector of ethnographical gems and when I die, I shall have received no encouragement to leave anything to the nation. If the nation will not accept my offer now

on account of a [illegible] rivalry between the two departments I shall take good care it never gets anything from me. Science is cosmopolitan and I had rather leave everything to the United States. Meanwhile I am waiting for the decision of the authorities... I hope you will change your mind and support my plans. It is clearly the best thing you can do under the circumstances.

Within a few days, the beginnings of a compromise had begun to take shape. The main responsibility for the details of the negotiations rested with Franks, who informed Pitt Rivers of the proposal privately. The main point of the proposed agreement was that the collection would remain at South Kensington but that it would be under Franks' department at the British Museum. With Franks now officially on the committee, the chances of acceptance of the scheme seemed favourable as well. Writing to Franks on 1st July 1880, Pitt Rivers explained:

I am very glad you are going to be on the committee. There are one or two other points I might as well mention. I see there is a suggestion that my museum, remaining at South Kensington, should be attached to the British Museum rather than the Science and Art department. Of course to me it is a matter of indifference what the department is called [as long as all of] the conditions remain the same. I should prefer the B.M. [in thinking that?] it should be associated with officers who have a thorough scientific knowledge of the subject whereas South Kensington is more aesthetic than scientific. I have experienced the inconvenience of this and have expressed it. On the other hand will the British Museum adapt itself to the peculiar conditions and accept the museum subject to my having the control of it during my lifetime. I consider this a *sine qua non*. It would not be possible to carry out my views in any other way. My object is, more space with a view to increasing the collection, and as the accumulations will be made with a view to a special arrangement in so far as the arrangement of the objects is concerned [it] must be in my hands. Moreover, the advantage I have over all Government Institutions is that, having one head, I can do as I please ... I should not think of giving up that advantage.

Pitt Rivers was, nonetheless, at least partially aware of the difficulties and admitted to them. He also realized that there were limits to what he could expect.

I should not propose in leaving my collection to the Nation at my death to make any special stipulations. If my system were accepted by men of science, it would be continued. If it were not, there would be no object in continuing it. Moreover, views become so much changed as knowledge accumulates that it would be mischevius to hamper the future with ideas of the present.

Soon afterward, in response to a formal request by Thompson - and apparently a short note from Lubbock - an official statement of the requirements of the gift was drafted, and on 21 July, the latter was presented to the committee. The conditions were as follows. First, no part of the collection was to be sold during Pitt Rivers' lifetime. However, during the same period, he would be free to add to it or take from it at will, providing the elements for new series or making suggestions for the re-arrangement of other ones as he saw fit. The Government, for its part, was to provide gallery space, cabinets and screens and would accept full responsibility for the safety and maintenance of the collection as well as any incidental costs such as labels, guide-books and the like. In detail, the conditions became more complicated. Specimens were to become Government property, but only after six months, and even then Pitt Rivers was to have the power to remove objects from the collection, if he found them 'useless for the purposes of the collection'. At the same time, Pitt Rivers insisted that 'no object could be loaned from the collection without his permission', that repairs had to be undertaken at Government expense, and that both insurance and what he described as 'police supervision' must also be paid for by the Government. His proposed scientific professional had been reduced to a mere 'curator', whose duties, it would appear, were to be more custodial than curatorial. The latter, however, would be paid a salary, at least during Pitt Rivers' lifetime, and the British Museum or South Kensington were to be allowed to make the selection.

It is a measure of Pitt Rivers' confidence in the importance of his collection and his offer that the terms should have been such unfavourable ones, from the Government and Council on Education's standpoint. Understandably the Council found the demands unrealistic and were obviously wary from the first. The committee established to consider the offer, on the other hand, was enthusiastic and reported toward the end of the year that its members were 'unanimously of the opinion that the collection offered to the Government, under the conditions stated ... is of great value and interest'. Their only reservation was that the total number of specimens accepted should be limited to those required for the 'efficient illustration of the principles upon which it has been formed', suggesting at the same time that the present space allotted to the collection, as a result, would probably be adequate for its future needs. Rolleston, out of loyalty for his long-time friend, even differed on that point,

suggesting that the collection be allowed to extend indefinitely and along the lines suggested by Pitt Rivers. The impression is that neither Rolleston nor the committee members could have recommended otherwise, given their long-time association with the donor.

The Council, in the meantime, was slow in forming their official response, and their decision was not made public until June of the following year. Their findings were set out in a letter to Pitt Rivers dated 3rd June 1881 by F.R. Sandford, the Council's Secretary:

I am directed by the Lords of the Committee of Council on Education to acquaint you that their Lordships have had under consideration the report of the Committee appointed to advise them in reference to the liberal proposal you have made in regard to your Ethnological Collection now being exhibited in the Galleries belonging to the Commissioners of the Exhibition of 1851 on the western side of the Horticultural Gardens.

The report in question proves the value and interesting nature of the collection, and recommends that it should become the property of the nation.

Their Lordships while accepting the conclusions to which the Committee have arrived, are however compelled, for the following reasons, to decide that it is not possible for them to accept the collection for permanent exhibition in connection with the Department of Science and Art.

In the first place, the space which the collection at present occupies has to be relinquished by the Department, and there is no other space at their Lordships' disposal, or likely to be provided elsewhere, in which the collection could be placed.

It is however chiefly on other grounds than want of space that my Lords have felt it incumbent on them to decline custody of the collection. Ethnology is not now represented in the collections of the South Kensington Museum, and it is undesirable to commence a collection with special reference to this branch of science while there is in another national establishment, the British Museum, a large collection of a similar kind.

It has been represented to their Lordships that your collection is arranged in a different system than that adopted at the British Museum, and as showing the development of form and shape, it would constitute an appropriate part of a museum like that at South Kensington, which is intimately connected with education in General and Industrial Art. Admitting to some extent the force of this argument, it, nevertheless, appears to my Lords that

your collection, if the Trustees of the British Museum should be willing and able to accept it, would not in any way interfere with that already contained in that Museum, but, on the contrary would increase the interest of Ethnological specimens which it now possesses.

My Lords feel strongly the inexpediency of national museums competing against each other, and wish that, so far as possible, a distinct line should be drawn between the collection at South Kensington and those at the British Museum. Each should be made as perfect as possible, but should occupy different grounds. My Lords must add a few words as to the question of expense. Although you have liberally proposed to keep up the collection mainly at your own charge during your lifetime, the whole cost of the maintenance would eventually devolve on the department which accepts your offer. This might lead to heavy expenditure for a curator, attendants, further purchases, cases, &c., and the collection would require an amount of space not only large in itself, but out of proportion to that which they can ever hope to be able to set aside for other branches of more immediate practical and educational use. The expenditure would be exceptionally large at the South Kensington Museum, where there is at present no one connected with Ethnological Science on the establishment; and after you had relinquished the management it would be necessary to secure the services of a gentleman with special qualifications for the care of this valuable collection.

My Lords thoroughly appreciate the liberality and public spirit which have prompted you to make the offer, whilst they regret that they are unable to take advantage of it on behalf of the Department of Science and Art.

Pitt Rivers was understandably taken aback by the Council's decision, but it was hardly surprising that the response of the Lords of the Committee should have been as it was. First of all, as the reply had explained, there was the matter of the expense involved, a public expense for what was still essentially a private collection. Then, too, there was the problem of competition with the British Museum. Since, of course, Pitt Rivers himself had described his own collection as an 'Ethnological' one it was difficult for them at that time to redefine it as one involving merely 'education in General and Industrial Art'. Even Pitt Rivers had stressed that South Kensington's interests were more 'aesthetic' than 'scientific', as he had explained in his letter to Franks. His sudden attempt to restate his own aims had been unconvincing to everyone

involved.

*V. The final settlement with Oxford*

Throughout 1880 and 1881 the question of a place for Pitt Rivers' collection was to remain unanswered. Pitt Rivers continued to add to it throughout the time of the negotiations with South Kensington, presumably in part as a show of goodwill. Between March and May of 1881, or just before his collection was formally rejected, he presented nearly a thousand new items. After the Council's decision, however, the number of loans was cut back, and between June and September there were less than fifty separate objects placed on loan. His series on locks and keys was also removed during July, but was returned soon afterwards; presumably the pieces were needed for the monograph he was then preparing.

Most of the new materials presented at the time were purchased from London dealers or were obtained through long-time contacts, such as Thomas Hutchinson or J.G. Wood. Other pieces resulted directly from his excavations. Those from Caesar's Camp in Folkstone were given in April 1881, just after his return from a trip to Egypt. To materials unearthed at Cranborne Chase, however, he assumed a different attitude, and everything resulting from his excavation of the barrows at Rushmore and the camp at Winkelbury remained at his country seat of Rushmore, where plans were already under way for a new, and more modest, museum of his own.

Nonetheless, his as yet unrealized plans for his new museum did not solve the problem of the collection at South Kensington. By the end of the year the authorities there were becoming impatient as well. And when Pitt Rivers submitted a few small items in the autumn of 1881, he was promptly informed that the museum would no longer accept any materials from him on loan, indicating at the same time that the new arrangements were going to have to be made soon.

For Pitt Rivers South Kensington's attitude posed something of a dilemma. While the possibility of a private museum had been considered, as he had indicated to Franks, he was obviously reluctant to commit himself to such a course. The expense alone, as he must have realized, precluded such a solution. Also, there were the new demands of his estate and his increasing ill health to be taken into account. The possibility of setting the museum up at Rushmore was evidently considered, but, of course, such a move would have defeated Pitt Rivers' main purpose - to make his museum a centre for scholarly and public interest.

His hope, then, was that some more satisfactory possibility would somehow present itself. His most obvious choice was one

of the universities, as many before him had realized. Oxford, Cambridge and Edinburgh had long accepted private collections such as Pitt Rivers'. Indeed, the first recognizable public museum, Oxford's Ashmolean, was the result of the bequest of an individual donor who two hundred years before had presented his assortment of 'natural and ancient curiosities' to the University with the understanding that the University would build 'a house' for them. Other similar gifts, sometimes linked to individual donors and sometimes presented anonymously, had further expanded the University's collection in later years, as had similar bequests to Cambridge and other universities including Edinburgh in particular. Both Oxford and Cambridge, moreover, had received major gifts of both archaeological and ethnographical materials. Oxford could claim Sir Richard Colt Hoare's well-known collection from Wiltshire among its archaeological treasures, and a number of Cook-related materials, presented by Johann Reinhold and George Forster around 1777, among its ethnographical collections. Cambridge had the Disney collection from Greece and an assortment of ethnographical objects gathered over the years.

In either case, then, Pitt Rivers' collection, despite its vast size, would have closely followed an accepted pattern. From his point of view, however, that was precisely the problem. It was true that he needed a place in which to house his collection, but at the same time it was important to him that the integrity and method of arrangement be maintained. Other collections, of course, had been allowed to remain intact. Colt Hoare's collection at Oxford, for example, was displayed separately in the Clarendon Building. But more often new collections were simply absorbed into the general collection and reassigned to their special departments. Moreover, with the exception of Elias Ashmole's original Oxford bequest, none could be said to form the foundation of a new and separate subject of discipline, as Pitt Rivers would have liked. New departments and research schools based on museum collections had been established over the years, but it was the universities that made the decision, not the donor himself.

Interestingly, Oxford was not Pitt Rivers' first choice. The Cambridge anthropologist Alfred Haddon (1855-1940), in a single reference many years later, revealed that Pitt Rivers had considered Cambridge, but had changed his mind for uncertain reasons.<sup>9</sup> Haddon knew Pitt Rivers during the 1890s, corresponding with him on occasion, and it is likely that Haddon's remark was not unfounded. But little more is known of Pitt Rivers' decision in this regard. He had good reasons, of course, for picking Cambridge. His friend Lubbock's connexions were there,

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<sup>9</sup> A.C. Haddon, 'Pitt-Rivers' (Obituary Notice), *Nature*, Vol. XL11 (1900), pp.59-60.

as were those of Franks, who was also a graduate at Cambridge. Moreover, Cambridge had already demonstrated at least the beginnings of a commitment to British prehistory, largely through the efforts of the Cambridge Antiquarian Society; the latter had donated its own collection in 1880, and had established a central gallery in the newly-founded University Museum in 1881. Nothing survives however, among the Pitt Rivers papers of any correspondence on the matter, and exchanges with Baron Anatole von Hugel (1852-1925), the Curator of Ethnology and Archaeology during this period, involved technical matters and contained no hint that Pitt Rivers had once considered adding his collection to those under von Hugel's care. Nonetheless, the fact that Cambridge had taken steps to form its own archaeological museum must have acted as something of an inducement to Pitt Rivers to make some arrangements for his own collection before it was too late to claim full credit for the advancement of the subject. Furthermore, the fact that Cambridge had already begun to take steps of its own must have made his choice of Oxford even easier.

Toward the end of March 1882, Franks received a letter from Henry Moseley (1844-1891), then Linacre Professor of Human and Comparative Anatomy at Oxford, stating that Pitt Rivers, on the suggestion of J.O. Westwood (1805-1893), had finally offered his collection to the University. Westwood, who had been Hope Professor of Zoology since 1891, was obviously someone for whom Pitt Rivers had a high regard. A long-time friend of Pitt Rivers' uncle, Albert Way (1805-1874), Westwood was also considered the pre-eminent authority on the derivation of ornamental design. Pitt Rivers must have hoped that Westwood's presence would have at least a residual influence on actual provisions for the collection once it was moved to Oxford - something which in fact never worked out according to plan. Moseley, Franks' correspondent, was also acquainted with Pitt Rivers, having been an active member of the Anthropological Institute since his return from a three-year voyage as Chief Naturalist on H.M.S. *Challenger* in 1877. Pitt Rivers had once praised a paper of Moseley's at the Institute for 'the evident accuracy of the observations which the author has made upon these ... almost newly discovered tribes'. And to return the compliment, Moseley had presented a number of objects from his own collection, principally Andamanese and other implements from South Asia and the Pacific, to Pitt Rivers shortly afterward. That Moseley would in the end be connected with the collection, therefore, was another factor in favour of Oxford. Moseley's own written protestation to Westwood that the credit for attracting the collection was Westwood's suggests that he was aware of his own influence on Pitt Rivers' decision as well.

Probably the deciding point in Pitt Rivers' choice, however, was the fact of his friend George Rolleston's connexion with the University. A Fellow of Pembroke since 1851 and Linacre Professor of Anatomy and Physiology since 1860, Rolleston had been closely involved in the establishment of scientific studies at the University, and during the early 1850s,

was one of the first lecturers at the new University Museum. He had been close to Pitt Rivers since the late 1860s and early 1870s, advising him on faunal remains and helping him on excavations on a number of occasions. His death in the summer of 1881 was a blow to many; Pitt Rivers wrote his obituary for the Institute's Journal and contributed generously to the Rolleston Memorial Fund. His decision to leave his collection to Oxford, therefore, could be seen as a further gesture to Rolleston's memory. That Moseley had inherited Rolleston's chair (or technically speaking, a subdivision of it) only underlined the connexion more clearly.

Nonetheless, the matter of the collection's donation was far from settled, and it was up to Moseley to persuade the University authorities to accept Pitt Rivers' offer. One of Moseley's main reasons for approaching Franks at the time was on precisely that point; he needed backing in his campaign on Pitt Rivers' behalf. Shortly afterward, the well-known anthropologist Edward Burnett Tylor (1832-1913) and John Evans were also approached, and, together with Franks, each was asked to provide a short statement of support to be read before the Hebdomadal Council, the main University governing board, later that spring. As Moseley explained to Franks:

I think the collection would be a splendid gain to Oxford and would do much [illegible] in the way of letting light into the place and would draw well.

Furthermore, as he continued,

it would act as an introduction to all the other art collections ... and would be of extreme value to students of anthropology in which subject we hope all men to take degrees very shortly.

Anticipating the opposition of some members of the Council, Moseley asked that both the collection and its arrangement be represented as favourably as possible.

Franks, Evans and Tylor complied shortly afterward, and in submissions published in the *University Gazette* on 30 March 1882 offered their own justifications for its acceptance. Franks was the least enthusiastic:

The collection is a very instructive and valuable one....[T]he system upon which it is arranged is different from that I have adopted in arranging the national collection of ethnology, but it seems to me very desirable that collections should be arranged on different principles from each other, as each system brings out special points of information and enables the student to see the various aspects of a subject.

Evans struck a similar note, again offering as much an apology as a commendation:

As a school for studying development in form and in art it is unrivalled, and the mere fact of its peculiar arrangement, with the view of illustrating development, does not at all distract from the value of the Collection from an ethnological or anthropological point of view.

Only Tylor, apparently already anticipating the possibility of a position, offered unrestricted praise:

Oxford would I think do a very important service to Anthropology and History by taking and housing the Collection, which would not only do its own work but would enhance the value of the Ashmolean [Museum] by making it intelligible.

Armed with his letters of recommendation, and under a directive from Pitt Rivers, Moseley made the offer to the University in late April 1882. Pitt Rivers' own conditions were similar to those offered the previous year to South Kensington. The University, for its part, would be required to accept the collection as it presently stood (including its arrangement), and Pitt Rivers would continue to have the final word over its control until his death. The University would also be required to provide a building and supply the necessary museum cabinets, cases and screens. The subject of a stipend for a lecturer or curator, however, had apparently been dropped, as had Pitt Rivers' earlier stipulation that he be allowed to borrow from the collection at will. Otherwise, the University was free to do as it chose. As a further inducement to the members of the University, copies of the *Catalogue* and off-prints of an article in *Nature* of 1880 describing the collection were placed in the Radcliffe Science Library at the University Museum. Again Moseley was responsible for the arrangements.

Moseley's efforts were successful, and on 30 May 1882 Evan Evans, the Vice-Chancellor of the University, delivered the Council's opinion, 'That the offer of Major-General Pitt Rivers, F.R.S. to present his Anthropological Collection to the University be accepted'. It was also suggested that arrangements should be made for its management and maintenance and further suggested that an annexe be appended to the east end of the existing museum. Echoing Moseley's earlier findings, it was proposed that such an accommodation could be carried out at a cost of between £7,000 and £8,000.

It will be seen [the report continues] that the Collection, besides having great intrinsic value, which from the scarcity of the objects themselves must necessarily increase as time goes on, it is

of very wide interest, and cannot but prove most useful in an educational point of view to students of Anthropology, Archaeology, and indeed every branch of history.

All that remained was for a committee, comprised of Henry Acland (1815-1900), Regius Professor of Medicine, Prestwich, Moseley, Westwood, Henry J.S. Smith (1826-1883), Keeper of the University Museum, and Henry T. Pelham (1804-1886), one of the Curators of the Park, all 'selected from Convocation', to provide a more detailed set of recommendations to the Council and establish guidelines for acceptance. Cautious as ever, the University had taken its first steps toward acceptance, which was completed on 20 May 1884 by the affixing of the seal to the Deed of Gift.

## VI. *Conclusion*

The Pitt Rivers Collection was successfully transferred over to Oxford over the four years between 1884 and 1888. Responsibility for it rested for a short time with Moseley, then afterward with his assistant, Henry Balfour (1863-1939); Edward Tylor, officially attached to the Pitt Rivers Museum as a lecturer, gave assistance and sometimes actually helped with arrangements. Pitt Rivers from that point on had little official involvement in the management of his collection. On several occasions, however, he did attempt to intervene. Still, his forays were effectively blocked, primarily by Balfour and the University. He was never particularly happy with this state of affairs and, indeed, regretted the end of his active connexion. As he wrote two years before his death to F.W. Rudler, then president of the Anthropological Institute:

Oxford was not the place for it [his collection], and I should have never have sent it there, if I had not been ill at the time and anxious to find a resting place for it at some time in the future.

But of course, nothing more was to be done. The collection was finally and effectively out of his hands.

### THE PITT RIVERS MUSEUM IN 1983

It is now a century since General Pitt Rivers offered his collection to the University of Oxford. Although its acceptance was agreed in principle by Convocation in May 1883, the seal was not affixed until a year later. The Museum's centenary year, therefore, is 1984. The construction of a building to receive it, an Annexe to the University Museum, was begun in 1885, and it seems that the public was first admitted early in 1890. In accordance with the General's wishes E.B. Tylor was appointed to lecture on the subjects covered by the collection; the Museum, and the subsequently-established Department of Ethnology and Prehistory of which it forms part, can therefore claim to be the birth-place of academic anthropology in Britain.

It was also the General's wish that the collection should continue to be arranged in accordance with his own views and method, reflecting the approach he had developed over the preceding two decades during which he had been profoundly influential on the growth of ethnology and archaeology as subjects for serious study. For this reason the exhibitions are arranged, with minor exceptions, by a comparative or typological method. They illustrate, not whole cultures, but problems which have presented themselves to man from the earliest times and the solutions which man has devised: fishing and trapping, pottery manufacture, treatment of the dead, musical instruments, hafting of tools, surgery, to give some random examples. It is a logical consequence that except for industrial products, the collection covers the whole world and all periods: objects from prehistoric Europe or America, modern or recent material from outside Europe, and 'folk' items from Oxfordshire, can be seen side by side. The reserve collections are arranged on the same principle. This method raises some difficulties in providing a

service to research workers, who nowadays usually study a culture or a period rather than a technology, for a considerable search may be needed to assemble all the different categories of objects from one people or area. On the other hand the juxtapositions can be interesting and revealing, and for some visitors, such as musicologists or students of textile design, the method is ideal.

The Balfour Library covers all the subjects included in the collections or taught by the Department, and among its holdings are many works and periodicals not available elsewhere in Oxford. The full value of its rich photographic and documentary archives has been realized only in recent years as progress has been made with cataloguing, the early ethnographical photographs, going back to the middle of the 19th century, being especially important.

### *The Present Situation*

It is not always realized outside Oxford that the Museum forms part of a teaching department, and that the academic establishment includes lecturers who have no direct responsibility to the Museum and assistant curators whose duties do not include teaching (though all do so). Because of this unusual combination the teaching and the research interests of the academic staff incline towards a historical, environmental, technological and economic approach, with an awareness of the common ground between ethnology and prehistory. It is felt that the collections should not be merely a pool of research material - though this is an important part of their functions - but should be actively used in teaching at both undergraduate and graduate levels; and indeed this is so, for in addition to teaching for Diploma students and helping those working for research degrees, the Department provides instruction for undergraduate Geographers and Human Scientists. The curatorial staff now in post believe that not only should they be willing to teach, but that the research entailed and the 'feed-back' from students are of value in relation to their museum duties.

The Museum has an active collecting policy, a basic principle of which is that nothing offered which would be a valuable addition and is of a type not likely to be available in the future should be refused simply because of lack of space. The latter criterion applies to most ethnographical material. Of course a degree of realism is necessary; the offer of a canoe or a totem pole would have to be considered carefully, and in such cases another appropriate museum may be suggested. The preservation of material rapidly becoming obsolete, however, is a major duty of ethnographical museums. Real duplication is avoided, but apparent duplication can show the distribution of a

trait in time or space, and is then acceptable.

The Museum does not compete with local museums for archaeological or 'folk' material of local origin; nor does it normally compete with a national museum for material of importance lacking in the national collection. A valuable degree of co-operation exists between the Pitt Rivers Museum and the Museum of Mankind (London). Material is not accepted if there is doubt as to the legality of its original acquisition or export; but, especially in the case of items bought at auction or from dealers, this may be difficult to establish.

Cultural change is a proper study for a museum, both as a historical process of universal application and when accelerated by Western contacts. The recording of such change, for example the adaptation of traditional techniques to introduced materials, is a relevant museum subject, but collecting to illustrate it is 'open-ended' and, despite what has been said above, limitations of space and other resources have to be considered. Pieces made primarily for sale to Europeans and not reflecting indigenous values are not normally acquired.

At a time when the record price for a piece of ethnographical art (a Hawaiian wood sculpture) is £250,000, the Museum's very small purchase funds exclude it from competing in this field; but the Pitt Rivers is not an art museum. The General's insistence on collecting the ordinary and average as well as the exceptional is still a guiding principle. It is possible to buy, at auction or from dealers, reasonably-priced pieces which fill gaps in the collection. Private owners, too, are still a useful source, either by gift or by purchase. There are many, often now elderly, who have served in former colonial territories or who travelled before the days of mass tourism. However, the preferred method of collecting is by giving relatively small sums to graduates of this or other departments when they undertake research in the field. Their expenses are paid from other sources, so all of the Museum's contribution can be spent on collecting, and being trained in anthropology or archaeology they are able to provide the essential documentation. First-class collections, from South America, the Sudan, Kenya, New Guinea and Indonesia, have been obtained in this way during recent years.

The Museum has long been active in publication, with fifteen titles available in its Occasional Papers in Technology and Monograph series and several more at various stages of preparation. It recently published a booklet on the archive photograph collection, and sells postcards and guide leaflets.

The establishment of the academic museum staff is a curator and two-and-a-half assistant curators (one being a half-time lecturer). There are eight technicians, which may seem a large number until it is realized that they provide electrical and security services and carpentry for exhibitions and storage, do all display work and are responsible for the maintenance of the exhibited and the reserve collections. One is occupied full-time as photographer, working on public orders, teaching require-

ments and museum records. Another is the only full-time conservation officer, a ludicrously and dangerously inadequate provision for a collection of this size and importance, and is also in charge of the textile store. There are six attendants, who also have to clean the museum, offices and library. This is why the museum can be opened to the public only from 2.00 to 4.00 p.m. on week-days. With this level of staffing it is obviously impossible to provide expertise on all the subjects and areas covered by the collections. Every effort is made to meet the needs of research workers, but it is not always possible to locate and assemble all the pieces which they wish to see. Though the Museum's duty is primarily to the University, the staff are strongly aware of an obligation to the public and that the service offered is not adequate. Schools and other organized parties are admitted by arrangement during the mornings, but no guided tours, introductory talks or questionnaires are available. Specimens belonging to the public are identified and authenticated (but not valued) whenever possible, and advice is given on reading or on other sources of reference. It should be added that although the University museums provide what public services they can, and are a considerable tourist attraction, the local authority contributes nothing to them.

The Museum is at present split between four sites. The main building still houses all the displays, a large amount of reserve material, the Balfour Library, offices, workshops and lecture room. Across South Parks Road is the conservation laboratory, which also accommodates the textile store. Much of the archaeological material and some musical instruments are kept at 60, Banbury Road, which also houses the Donald Baden-Powell Quaternary Research Centre, a flourishing section of the Department's archaeological teaching and research activities. The main reserve store is in part of the Old Power House, a University property on the other side of the city. All these buildings are grossly overcrowded, and in none are the atmospheric conditions satisfactory, though considerable improvements have been achieved during the last two decades by *ad hoc* methods such as improving the heating systems and painting over the glass roof of the main building. The problems are not limited to the display and storage of specimens. Conditions for staff are unsatisfactory, and there is not adequate quiet working space for visitors or the Department's own students.

### *The Future*

The University's long-term plan is that the Museum and all its activities will eventually move to a new site at numbers 60 to 64, Banbury Road, new buildings being erected on the gardens

behind and the Victorian houses being retained for administrative use. The first stage of this project, financed by a bequest from Mr. Lewis Balfour, was completed structurally more than four years ago, but unfortunately serious problems of atmospheric control have made it impossible to move any specimens into the building. It has now been agreed that air-conditioning will be provided, and it is hoped that by the middle of 1983 it will be possible for work to commence on installing the new exhibitions.

The new building is too small to provide significant alleviation of the museum's space problems; that will have to await further development of the site. It will, however, make it possible to offer new exhibitions more modern in conception than anything which could be attempted in the old museum, and it will provide valuable experience before the main development proceeds.

The building divides into two unequal parts, forming about two-fifths and three-fifths of the display area. It is intended that the smaller part will illustrate the pre-agricultural way of life, with the main emphasis on Old World paleolithic cultures but including also some material from modern hunter-gatherer groups such as the Eskimo, the Bushmen and the Australian aborigines. The larger part will be given to musical instruments, and it is to this project that the Museum's resources will have to be mainly devoted.

Helene La Rue has written elsewhere on the Museum's musical instrument collection.<sup>1</sup> It will be enough to say here that it is perhaps, over-all, the best in the world, and is certainly among the two or three most important. The intention is not only to show instruments in typological series - though this will be an important element in the exhibitions - but to illustrate the ways in which musical sounds are produced and the social and ritual contexts of music, for example with masks and dance costumes. There will be listening points at which recordings can be heard, and audio-visual equipment will be used where appropriate. Since only a few museum specimens can be played by visitors, it is hoped to procure instruments of similar types for use by organised parties and to arrange recitals of exotic and of more familiar music.

To bring these plans to fruition will be beyond the present resources of the Museum. Funds will be needed not only to finance the exhibitions - to provide display materials, audio-visual equipment, photographs and other illustrative matter - but also for the salary of an assistant curator for the music collections, to care for them and to organize the exhibitions and the supporting activities for students and the public. The University has agreed that the Museum may raise funds for these purposes, mainly by approaches to industry and to organisations

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<sup>1</sup> See H. La Rue, "'The Natural History' of a Musical Instrument Collection", in B.A.L. Cranstone and S. Seidenberg (eds.), *The General's Gift: A Celebration of the Pitt Rivers Museum Centenary (1884-1984)*, Oxford: JASO 1983 (in press).

linked with the musical world, though gifts from individuals will of course be very welcome. Help in the form of equipment (by gift or loan) or of display materials will also be most valuable.

Its first century has seen the Museum grow into one of the most important in the world in its field, not only with regard to the size and scope of its collections but also their quality and documentation. If it seems to have lagged behind in display techniques and the services it provides, this is a result of starvation in space, staff and all other essential resources. Some recent re-arrangement of parts of the exhibition have shown what can be done even within the present severe limitations. Visitors often express a hope that it will not be changed radically. It certainly has charm and a unique atmosphere, but the staff are well aware of its deficiencies. At the beginning of its second century the Museum is poised to show what a museum can and should do in a field such as ethnomusicology, breaking out of the constraints of static display and actively involving its public and its students in obtaining the greatest possible value from its collections.

B.A.L. CRANSTONE

## THE RELEVANCE OF MATERIAL CULTURE TO ANTHROPOLOGY

Interest among anthropologists in the study of material culture has fluctuated considerably throughout much of this century. Prior to 1900 anthropologists had relied on material, verbal and observed behavioral data equally and without hesitation. Indeed material data, as Tylor and Pitt Rivers ably demonstrated, lent themselves particularly to arguments regarding the evolution and distribution of cultures. Yet by 1914 Wissler could remark:

For some years the study of material culture has been quite out of fashion, though not so very long ago it was otherwise. Field-workers still record such random data as come to hand and gather up museum specimens, but give their serious and systematic attention to language, art, ceremonies, and social organization.<sup>1</sup>

Although Harrison later complained,

The systematic study, and the systematic teaching of the material side of human culture receive less than their due share of attention in this country,<sup>2</sup>

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<sup>1</sup> C. Wissler, 'Material Cultures of the North American Indians', in *American Anthropologist*, Vol. XVI (1914), pp.447-505, at p.447.

<sup>2</sup> H.S. Harrison, 'Evolution in Material Culture', *Proceedings of the British Association for the Advancement of Science*, 1931, pp.137-159, at p.137.

there was in fact a revival of interest in material studies in England during the early 1930s.<sup>3</sup> This interest was to continue until the end of the War when there began a further decline to what was perhaps the nadir in the late 1950s. During the 1970s, in part under the stimulus of archaeology, research in the field of material studies has been revitalized and interest raised to an encouragingly high level.

The reasons for the overall decline during the first half of the century are complex and form a fascinating topic in themselves for historical research. One can isolate major factors: the shift of mainstream anthropology from a museum to a university base; the unfortunate identification of material culture studies with the more extreme diffusionist schools of thought; the emergence of the functionalist school and its understandable efforts to distance itself from the latter; the narrow equation of material culture with museums, which themselves were acquiring a poor image, both public and scholarly.<sup>4</sup> The great concerns of the post-World War II era with social and political problems helped to direct anthropological attention once more away from material topics. The isolation of material studies from mainstream anthropology increased as structural-functionalism came to dominate anthropology,<sup>5</sup> reducing the flow of good scholars to them and creating a widely held opinion that material studies were 'unproductive' and sterile.

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<sup>3</sup> R.U.Sayce, *Primitive Arts and Crafts: An Introduction to the Study of Material Culture*, Cambridge: Cambridge University Press 1933.

<sup>4</sup> Cf. Wittlin, '...museums, with few exceptions, have remained essentially what they were in the early nineteenth century when the public museum began to be a feature of European culture. This deficiency would seem to have been caused by the museum's insufficient contact with reality' (A.S.Wittlin, 'The Part of the Museum in Modern Society', in *Proceedings of the British Association for the Advancement of Science*, 1944, pp.57-62, at p.57). Wittlin continues, '...up to the present date no standards exist for the professional qualifications of the museum curator, not even for the curator of the "general museum". The traditions of the curator who was a courtier, a second-rate artist, a retired officer or private "connoisseur" still loom in the halls of our museums and in the minds of our curators.'

<sup>5</sup> Some anthropologists did make serious attempts to apply functionalism to material studies. See, for example, McConnel, 'My task as a social anthropologist is to place these artefacts in their social setting and to describe their function in the way of life to which they belong' (U.H.McConnel, 'Native Arts and Industries on the Archer, Kendall and Holroyd Rivers, Cape York Peninsula, North Queensland', in *Records of the South Australian Museum*, Adelaide, 1953, pp.1-42, at p.2).

It is against this historical background that I choose to consider the 'relevance' of material culture to anthropology. Material culture forms the subject matter, of course, of a number of disciplines: archaeology, geography, material history and, surprisingly perhaps, even some aspects of environmental psychology and sociology. Here we are concerned only with anthropology.

The more immediate objectives of anthropological research change in response to new theoretical developments and the shifts in emphasis that result. This is understandable. What, however, remains unchanged is the overall objective: to understand Man as a cultural being. Whether we belong to the school of thought that considers material culture to be an integral part of culture, whether we hold the opposite view or whether, like Kroeber, we consider the point of no consequence, the reality of the situation is similarly unchanged.<sup>6</sup> All societies have material assemblages, Western societies most obviously so.<sup>7</sup> As we know from our own personal and direct experience material items play an important part in our lives. They both influence and in their turn reflect influences from our cultural beliefs and social behaviour. To understand fully any society, therefore, one cannot exclude its material aspects.

Even when material culture studies were at their lowest ebb and most field anthropologists were obviously disinterested in them, these same anthropologists relied heavily on the material cultures of the societies in which they worked. Such reliance was for the most part on material items as visible markers: of the level and thrust of subsistence and economic activities; of the built environment within which social activity took place; as symbols of political, religious and other beliefs; and as indispensable adjuncts to all aspects of social

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<sup>6</sup> Cf. Kroeber and Kluckhohn, 'Strictly speaking there is no such thing as "material culture" - what is culture is the idea behind the artifact' (A.L.Kroeber and C.Kluckhohn, *Culture: A Critical Review of Concepts and Definitions*, Cambridge, Mass.: Harvard University Press 1952, p.65); Harris, 'Moreover, like most modern anthropologists, Tylor regarded material objects as an essential part of culture' (M.Harris, *Cultural Materialism: The Struggle for a Science of Culture*, New York: Random House 1979, p.279); Kroeber, 'Accordingly we may forget about this distinction between material and nonmaterial culture, except as a literal difference, that it is sometimes of practical convenience to observe' (A.L.Kroeber, *Anthropology*, New York: Harcourt & Brace 1948, p.296).

<sup>7</sup> Harris suggests, '...a complete inventory of the material culture of U.S. society would certainly exceed a trillion items' (i.e. *types* of objects, not just objects) (*op.cit.*, p.124).

life. Malinowski rightly observed, 'There is no single type of human activity without its material accessories'.<sup>8</sup> As may be seen from the field monographs of any anthropologist since Malinowski, the researcher, no matter what his or her main topic, has depended heavily, if perhaps unwittingly, on the material aspects of the culture studied.

But this, of course, is not the same as systematic research deliberately focused on material culture. All too many anthropologists see this as an arid field, one best left to 'technologists'. Yet as we have noted material culture forms a substantial and significant part of the cultural heritage of every society. What is also most evident is the strong interest people in the society take in this tangible aspect of their heritage. Each year billions of visitors throng our museums. Almost every small town has its group of enthusiasts seeking to preserve old buildings. The urge to collect is both widespread and varied, covering everything from postage stamps and sporting memorabilia to porcelain and antique motor cars. Obviously, a phenomenon that plays such a major role in society, as does material culture, cannot be ignored. It merits study because it is there. Could anthropology, deeply interested though it is in human society, have been a little myopic in its disinterest in things material? Or does it consider them to be the proper subject of study for some parallel discipline? If so, I know of none. Archaeology, concerned with past societies, has no such reservations and fully accepts material data as valid for study.

Traditionally we, as anthropologists, have concentrated much of our research efforts in societies that are non-Western. As modern anthropologists have come to realise, fieldwork is not just a one-way process, with the individual researcher gathering data from a passive body of informants. The host society is an equally important participant in the process and the researcher, whatever his or her initial views and personal research objectives, accepts automatically a responsibility towards the society, a responsibility to document its culture *for its own benefit*. This is how anthropological research is increasingly being seen in such societies, concerned as they are with recording their disappearing traditional heritage.

As is evident from the strict legislation enacted by most recently independent countries and from international pressure for the repatriation of anthropological collections, concern for the cultural heritage, whatever its motivation, places a high level of emphasis on tangible material. Regardless of the distinctions we ourselves draw within anthropology, host-peoples today usually see material and non-material aspects of culture as equally important and expect anthropologists, whatever their

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<sup>8</sup> B. Malinowski, 'Anthropology', in *Encyclopaedia Britannica*, Vol.1 (13th edition), 1926.

specialty, to document both aspects of their culture. The failure of anthropologists in the past to honour sufficiently this responsibility has contributed substantially to the reluctance of many countries to allow anthropological research and also to the application of stringent and very specific requirements regarding the data gathered and their publication.

Many anthropologists have obviously felt that the documentation of a material culture, whatever its value to the society concerned, is an unproductive pursuit from the professional, theoretical viewpoint. The more one explores the subject, however, the more evident it becomes that this dismissal is the product of a frustrated inability to unlock the information contained in artefactual documents and indeed of a confusion of thinking regarding what constitutes material culture and its study. For an apparently obvious and straightforward subject this is surprising.

Material culture consists of the tangible phenomena of a human society that are the purposive products of learned patterns that are not instinctive. It includes portable objects, fixed structures and landscape features. Material culture is *not* a synonym for technology and it does *not* include related processes of manufacture and usage. Again it is *not* a synonym for portable museum objects; it extends well beyond these.

Within any society no item of material culture stands in total isolation from other material phenomena nor from the human members of that society, their beliefs and their behaviour. The interaction between the items and these other elements is a continuing process. Even where an item is removed from its original setting, for example to a distant museum, this process continues though within the new setting. What we are seeing is a network or system of interaction of which the material item forms the core.

The term 'material culture studies' is in a sense a misnomer. Our interest as anthropologists lies in what these phenomena can tell us about Man. Our focus, therefore, is on the relationship between material phenomena and Man in other words the material system containing the material culture, not just the material culture itself. Because of its apparent simplicity, material culture as a term has been used loosely to refer to material culture proper, to material systems and again to their scientific study, in other words material anthropology. This loose usage has been apparent both within the field of material studies and among its critics in other fields of anthropology. One result is that it has made difficult the framing of new theoretical objectives, the formulation of stimulating new ideas. All too often material studies have been allowed to remain ethnographic or to shift into technological studies. If we are properly to appreciate the relevance of material culture to anthropology, however, it is essential that we are clear in what we mean by the term.

But clarity of definition is not enough in itself. We can recognize that material cultures and material systems are of

value to field anthropologists, whatever their speciality. We can identify for the purposes of study each field of material culture and its associated system. We can recognize the mass of data that exists within this field. The problem is how best to make use of it in order to achieve our goal, a better understanding of Man as a cultural being.

For the field material anthropologist concerned with cultural ecology or the built environment, with symbolism or symbolic interaction, with material determinism or with ethnicity, the position is not especially difficult. The systematic gathering of data according to objectives and methodologies already formulated is virtually the same as for any branch of anthropology. It requires particular emphases, some special skills and a familiarity with particular aspects of current anthropological thinking. But so does all research.

The problems arise in field attempts to cover the full spectrum of a material culture as an ethnographic exercise, without a clearly pre-defined focus and objective. Such studies encounter great difficulty in treating evenly and in depth, yet also as a coherent whole, such diverse topics as houses, water transport, personal ornaments, body markings and the products of individual subsistence and manufacturing crafts. The problem is compounded, as far as museum research is often concerned, by inadequate research preparation, too short a time in the field, and subsequently inadequate time to analyse thoroughly the collected data before writing up the project.<sup>9</sup> The result can all too often be an uneven catalogue that includes a series of essays on particular features of the material system. The problem is one of inadequacy of method, not of material.

When we turn to the anthropological collections held in museums across the world, the same is even more true. To argue that archaeologists can profitably extract data from undocumented excavated material, albeit stratified, or that languages and cultures can justifiably be reconstructed on the basis of fading memories of aged informants, yet at the same time to dismiss the vast museum collections of the world, conservatively estimated at some 4.5 million artefacts,<sup>10</sup> as valid sources meriting

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<sup>9</sup> Within most museums anthropology curators often experience a serious conflict between their roles as (primarily) field researchers and as administrators and collections managers within the museum itself. Museums are rarely structured to enable them to cope with extended field work (12 months or more) on the part of a curator.

<sup>10</sup> W.N. Fenton, 'The Advancement of Material Culture Studies in Modern Anthropological Research', in M. Richardson (ed.), *The Human Mirror: Material and Spatial Images of Man*, Baton Rouge: Louisiana State University Press 1974, pp.15-36, at p.29.

research is not logical. Yet this is what has in fact all too often occurred. Anthropologists take pains to read with great care the relevant literature before embarking on a major field project. How many take similar pains to locate and examine with comparable care the artefacts from the region?<sup>11</sup> On the other hand, how much of these vast museum holdings remains unresearched and unpublished?

Anthropology's inability to utilise effectively these museum collections, gathered in its name, must represent one of the great failures of the discipline. Yet the fault does not lie entirely on the one side. Those of us familiar with such collections recognise the practical problems involved in their research usage. Collections management systems, particularly documentation systems, are usually antiquated and inefficient. We have to accept a high percentage of 'no-show' responses to requests for specific artefacts. The documentation is often inadequate or suspect and retrieval speed is usually slow and time-consuming. Curators are normally very helpful but how many management systems today could truly withstand more than five or six different researchers on the same day without serious dislocation occurring? Most of these systems are generally unwieldy and gravely under-staffed and under-budgeted.<sup>12</sup>

At a different level the researcher is faced with the problem of how to extract data from the objects presented for study. The supporting documentation can prove invaluable. All too often, however, it can be scant and of questionable reliability. It is therefore the artefacts themselves that offer the greatest potential and therefore the real challenge.

From museum records we can reasonably expect to learn when, where and by whom an artefact was collected. In general terms these records should also indicate the region and the particular

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<sup>11</sup> An interesting recent example is that of Jorgensen's 1980 comparative study of the Western Indians of North America wherein he and his colleagues analysed a substantial body of literature in order to plot the distributions of various linguistic, faunal, floral, social and (material) culture traits of 172 Indian tribes (J.G. Jorgensen, *Western Indians*, San Francisco: W.H. Freeman 1980). The book gives no indication that any museum collections were examined. One can see this as criticism both of the study and of museums as research resources.

<sup>12</sup> In 1981 I made a study of the collections management systems of four major museums in Australia. The results were published in B. Reynolds, 'Ethnology Collections and Their Management in Australian Museums', in *COMA Bulletin*, Conference of Museum Anthropologists, No. IX, Brisbane, 1981, pp.8-20. The inadequacy of management systems elsewhere in the world has been well documented by Sturtevant (W.C. Sturtevant, 'Does Anthropology Need Museums?', in *Proceedings of the Biological Society of Washington*, 1969, pp.619-649), Fenton (*op.cit.*), etc.

culture. There may well be substantial additional information on the artefact, on similar artefacts, or on the collection as a whole, either in the same records or in published literature.

From the artefact itself we need to learn initially four things: its locality of origin, its age, its component materials, and details of its manufacture and usage. Given modern botanical and other scientific techniques we can usually identify the component materials and thereby perhaps locality. By direct observation, microscopy, X-ray analysis and other laboratory techniques, we can similarly deduce the methods of manufacture and usage. The determination of age, however, is more difficult for, except in instances where comparative dating is possible, we are usually left just with a 'latest date' - the date of field acquisition. This is particularly true of artefacts made from organic materials such as plant fibres or animal skin.

Once beyond these initial basic questions one can begin to explore various avenues of research. Here it is not sufficient to think of artefacts as complete entities in themselves. Particular elements, such as rowlock styles in watercraft, particular design motifs, particular forms of treatment, as seen in knots, or in the spinning of string; all these can prove most useful indicators of contact, of the history of movement, of distribution and of ethnicity.

On a broader scale, much is to be learned from the examination of a broad range of artefacts of the same kind, or of collections of objects from the same culture. From these one builds up a picture of the different aspects of life in the society - its subsistence activities, its economy, its settlement patterns, its ritual beliefs, and its values. Such comparisons also help us to appreciate the extent of adaptation to the immediate environment, the uses to which local resources are put. From particular objects, from colours used and from design motifs, we begin to learn something of the symbols of the society and of their importance. The unconscious motor patterns of craftsmen, as expressed through their artefacts, form an excellent area for exploration.

As in all research, one source but reinforces others. Artefact analysis confirms, or is itself confirmed from literature or personal accounts though in using it we have to allow for the bias of the original collectors - what they preferred to collect, what they obviously never observed in the culture. Despite such bias a picture can be built up of the culture and perhaps of distinctive local sub-groupings within it. There are limits, of course, to the range of data obtainable, just as there are with all cultural research. But laboratory techniques of analysis for anthropological objects are as yet in their infancy. Who knows what exciting avenues await discovery? In this our museum collections are unusual. What other sources of data can offer anthropologists such a potential for expansion?

On a different level, museum collections offer possibilities in terms of broad comparative studies across a wide range of cultures, for the exploration of such fundamental questions as:

where is the borderline between instinctive and cultural responses; what drives Man to develop his material culture beyond certain minimal levels and along such similar lines; the patterns of material culture that develop in response to particular physiological, subsistence, settlement or social conditions; the speed of technological change within a society and the potential acceptability of proposed new changes; the relative values placed by different peoples on their material possessions. Such questions might also be asked of non-material phenomena. Where tangible phenomena offer an advantage is that beyond a certain level they do not reflect subjective bias on the part of the researcher. They are also unique in their unchanging nature thus allowing re-examination at will.

Throughout this paper I have sought to show that our thinking about material culture, its meaning, its nature and its potential, has often been far too casual and dismissive. As a term, material culture covers a substantial part of what people consider to be their culture or cultural heritage. As the core of material systems it forms the subject matter of material anthropology and has made significant though perhaps unappreciated contributions to the success of much field research in parallel areas of anthropology. Through its substantial presence in museum collections it provides anthropology with a rich resource for further study and, through museum exhibitions and related programmes, an invaluable opportunity to increase public awareness, and obviously support, for the discipline. In concluding, however, it is only right that I should return to the initial question implicit in the title to this paper: is material culture truly relevant to anthropology? To that there can only be one answer: material culture forms, has always formed and will long form part of the subject matter of anthropology. As such it is obviously relevant to anthropology. Now whether some parts of anthropology are relevant to material studies....

BARRIE REYNOLDS

## THESIS RESEARCH AND COLLECTING: A FIELDWORKER'S VIEW

Sir Edward B. Tylor took particular interest and delight in the ethnological collections of the Pitt Rivers Museum,<sup>1</sup> but he perceived the study of material culture as merely one of many departments in the wider subject of anthropology.<sup>2</sup> During the first two decades of the twentieth century British scholars interested in material culture, who were known as ethnologists, '...tended to be more preoccupied with things than with people'; nevertheless, behind the study of objects there remained an interest in the people who made them.<sup>3</sup> It was clear that while ethnological specimens might be objects of intrinsic interest worthy of study in their own right, they might also reveal much about the societies whence they came. Sturtevant was later to emphasize the role of ethnological specimens as an important primary source and indeed argued that '...artefacts also have advantages over written records of behaviour and belief in being concrete, objective, difficult to distort, and little subject to

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<sup>1</sup> A.C. Haddon, 'Introduction', in E.B. Tylor, *Anthropology Volume I*, [1881], London: Watts & Co. 1946, p.viii.

<sup>2</sup> E.B. Tylor, *Anthropology Volume I*, [1881], London: Watts & Co. 1946, p.xii.

<sup>3</sup> G. Lienhardt, *Social Anthropology*, [1964], Oxford: Oxford University Press 1979, p.5.

personal or ethnocentric bias'.<sup>4</sup>

If ethnological specimens are to be worthy of scholarly attention above the level of mere curiosities and possibly, as was suggested by Sturtevant, as an undistorted source of information, then their manner of acquisition must be open to investigation. If they are to be used as a source, then the scholar needs to know in what way the specimen records the ethnography and whether it is representative; and, since it is impossible to accept Sturtevant's claims of objectivity for the material record, whether personal prejudice on the part of the collector influenced the selection of specimens.

Field collecting is one of the methods by which anthropological museums and departments acquire ethnological specimens, and yet it is a subject that has been sparsely mentioned in the specialized literature. In his *Guide to Field Collecting of Ethnographic Specimens*, Sturtevant argued that the best collections were usually those made during the course of fieldwork by anthropologists interested in both artefacts and the local ethnography;<sup>5</sup> but he did not give many specific examples. It is my intention to provide an account of how a field collection was recently made for the Pitt Rivers Museum, in the hope that this may be of assistance to future field collectors. It is hoped that this paper will show how the ethnography is illuminated by the specimens and what considerations influenced their selection, with a view to assessing the collection as an anthropological source.

Between 1980 and 1982 I carried out approximately twenty months of fieldwork in the Bima regency of Sumbawa, one of Indonesia's larger eastern islands. The purpose of the research was to examine closely a variety of Biman craft industries, tracing aspects of their history and recording their adaptation to the contemporary social and economic environment. The research was funded to doctoral level by the Social Science Research Council, and field permits were granted by the Indonesian Institute of Sciences (Lembaga Ilmu Pengetahuan Indonesia). The Pitt Rivers Museum, which has a policy of asking students to collect for it, agreed to purchase Biman materials gathered during the course of my fieldwork, and to advance funds for the purpose.

The Museum was interested in acquiring specimens from a range of Biman crafts, though since an important part of the research was concerned with the manufacture of textiles, both the curator (Mr B.A.L. Cranstone) and I were particularly enthusiastic

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<sup>4</sup> W.C. Sturtevant, *Guide to Field Collecting of Ethnographic Specimens*, Smithsonian Information Leaflet 503, Washington: Smithsonian Institution Press 1977, p.2.

<sup>5</sup> *Op. cit.*, p.4.

to collect examples of textile technology and the completed fabrics. This emphasis on textiles and their technology was also influenced by the Museum's tradition of collecting examples of craft processes and the fact that we knew that Bima had been historically a significant textile-producing region;<sup>6</sup> but since the regency had hardly been documented by anthropologists we had no information about the present condition of the industry.

Before commencing fieldwork I had several meetings with the curator to discuss methods of collecting and he provided many useful suggestions based on his own experience in Papua New Guinea. I had undertaken both library and museum research in Britain and Holland prior to leaving for Indonesia, which further enabled me to assess Bima's collecting potential from the academic perspective: as has been pointed out by Sturtevant, a collector informed in this manner is better able to fill the gaps in existing museum collections.<sup>7</sup>

My fieldwork revealed that while some of Bima's craft industries had declined, many, such as textiles, were still important. The latter had managed to stay in business, despite competition from mass-produced goods, by exploiting the regional ethnic and custom-made markets, which had not been penetrated by larger competitors. I thus had the good fortune of being able to collect specimens from a contemporary domestic textile industry, and it is worth examining three aspects of the technology which were of significant research interest.

The first of these concerns the manufacture of thread in the Indonesian archipelago, which is recorded in publications and represented in a number of British museum collections. Despite the acknowledged skill of some of the authors on the subject,<sup>8</sup> there remain vague areas and it is unclear whether museum collections illustrating the process are complete. In this context Bima is of academic importance because its cotton industry was still operating, though in decline, and the thread-manufacturing equipment was still available.

Secondly, the method of transferring thread to a loom (warping-up) has been poorly recorded. In Bima this task can be undertaken with or without the use of specialized equipment, the former method being quicker and easier. The latter method is generally well understood,<sup>9</sup> and information from Bima was likely

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<sup>6</sup> J.E. Jasper and M. Pirngadie, 'De Weefkurst', in *De Inlansche Kunstnijverheid in Nederlansch Indie*, Vol. II, The Hague: Mouton 1912, pp.142-143.

<sup>7</sup> Sturtevant, *op. cit.*, p.6.

<sup>8</sup> See for example A.C. Haddon and L.E. Swart, *Iban or Sea Dayak Fabrics and their Patterns*, [1936], Carlton: Ruth Bean 1982, pp. 5-9.

<sup>9</sup> Cf. Jasper and Pirngadie, *op. cit.*, p.98.

to provide only further evidence of regional variation. However, while the technology of the first method is well represented in British and Dutch museum collections, its use has been only partially described.

A third potential area of interest both to researchers and possibly to the craft-oriented general public (such as the Weavers' Guild) was one of the Biman methods of patterning textiles. While the techniques of *batik* and *ikat* are widely known in Britain, another of Indonesia's great textile traditions has received scant attention. This technique, called supplementary weft, is one of the foremost in Bima, and therefore a collection in this area was likely to have wider scholarly and craft implications.

Following this outline of the technical significance of Biman textile methods it is worth considering two aspects of the technology which have wider social and economic implications. Firstly, one of the reasons for the persistence of the Biman textile industry, in spite of factory-based competition, lies in its reliance on simple domestic technology. The main apparatus, the back-strap loom, is flexible because it can be swiftly brought into operation and easily stored when not in use. This allows women, who are the weavers in Biman society, to manufacture cloth at odd moments in the working day. Since they are not totally dependent on weaving for an income, and as the equipment is not capital-intensive, they are able to take advantage of local market changes with low financial risk. The latter point can be compared with the garment trade of southern India, where Swallow has argued that the domestic industry has maintained its competitiveness by low overheads and market adaptability.<sup>10</sup>

In the second place, until the 1950s Biman unmarried women were largely confined to their homes in accordance with local Muslim practice. Men appreciated the economic advantages of having wives who were good weavers, and young women were expected to advertise their industriousness to potential husbands. However, since they were unable to demonstrate these skills in public, the unmarried women broadcast their abilities by means of rattles attached to or built into their looms, which clattered as they worked. Examples of similar devices from Indonesia can be found in British museums, such as the model of the Bugis loom in the Skeat Collection, in the University Museum of Archaeology and Anthropology at Cambridge; yet there is insufficient documentation to explain the technical features. Therefore an example from Bima with accompanying background information was likely to provide useful comparative material.

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<sup>10</sup> D.A. Swallow, 'Production and Control in the Indian Garment Export Industry', in E.N. Goody (ed.), *From Craft to Industry: the Ethnography of Proto-Industrial Craft Production*, Cambridge: Cambridge University Press 1982, p.151.

While the textile technology was interesting and worth collecting, the fabrics themselves were also noteworthy from the perspectives both of social anthropology and of the general public. An example of this is the manner in which textiles had been used as indicators of social position during the period when Bima was ruled by a Sultan. The region had been governed through a complex bureaucracy in which people of high social status had worn sarongs woven of expensive materials and occupation was indicated by the colour of the clothing. Though this use of textiles declined when Bima came under republican government in 1950, another social aspect of textiles is still of contemporary significance. In common with many other Indonesian peoples, the Bimanese exchange elaborate fabrics at festivals held at salient points in the life cycle, and this tradition has not diminished in importance. With their Biman aesthetic and symbolic value, these textiles indicate vividly the modern relevance of the festivals, and are therefore of interest to social scientists.

As indicated above, the selection of specimens was undertaken with reference both to museum and literary records, and to what was available and noteworthy in the contemporary society. It is, however, worth considering two aspects of European and North American ways of perceiving artefacts which would have been ethnocentric and possibly socially irresponsible in the Biman context. Anthropologists such as Paul Henley have drawn our attention to the ethnocentrism inherent in ascribing worth to objects of antiquity in societies without such values.<sup>11</sup> Furthermore, he has indicated the limited usefulness of praising certain materials because they appear to be traditional, as these often may not have been indigenous.<sup>12</sup> As will be examined below, both these observations and an attention to collecting ethics were pertinent to the Biman situation.

It is very difficult to define what might be a traditional material in a society such as that of Bima, where imported thread has been available since at least the turn of the century.<sup>13</sup> Today, most of the handloom industry employs synthetic thread made on the island of Java, which is preferred by the Bimanese to locally-spun cotton for practical and aesthetic reasons. In the first place, synthetic thread is easier to weave because it does not break as often as does the local cotton, and garments made with it are both lighter and cooler than those made of heavier

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<sup>11</sup> P. Henley, 'The Economic and Aesthetic Value of Ethnic Art', in *New Society*, 1979, p.640, citing Bascom.

<sup>12</sup> *Ibid.*, p.641.

<sup>13</sup> J.E. Jasper, 'Het eiland Soembawa en zijn bevolking', in *Tijdschrift voor het Binnenlandsch Bestuur* 34 (1908), p.100.

home-spun. Secondly, though the Bimaneses were able to obtain a range of bright colours with vegetable dyestuffs, these were difficult to fix and tended to fade. Since local aesthetics demanded colour definition, especially when they were combined in checks, fading was particularly undesirable and therefore the vivid, long-lasting aniline dyes of the imported thread were easily incorporated into Biman society. A textile made of synthetic thread, in the eyes of the people, is no less Bimaneses than one woven from home-grown products; and as the collection was intended to reflect contemporary reality, examples of both types were acquired.

In the art-dealer markets of Europe and North America antiquity is associated with value, yet these preoccupations are not relevant in the Biman context where objects are not revered on account of their age. In the Biman markets an old item is not necessarily more expensive than its modern counterpart. Nevertheless old objects, such as textiles, are important in Biman society, not because they fetch a better price, but because they are often used as a design resource. When weavers design a new high-quality textile they sometimes employ old cloths as a reference from which successful motifs and combinations of colour can be copied. The local market price does not reflect this value possibly because the resource does not usually leave the area, nor is the local economy sufficiently wealthy to support inflated prices like those of the antique markets of the western industrialized nations.

Old Biman textiles are of academic interest because they were employed as indicators of social status, and since they were available on favourable purchase terms they would have made a valuable addition to the museum's collections, but this would have been at the risk of depleting a local resource. The solution to these ethical considerations was to focus the collection on the contemporary industry, recording the use of established designs in the modern fabrics and tracing their history with photographs of some of the older textiles.

While the varying academic, ethnographic and ethical demands influenced the selection of specimens, the process of collecting itself was not straightforward. Textiles, for instance, were available in the markets or could easily be bought from peddlars in the villages, but had the collection consisted only of objects acquired by outright purchase it would have been devoid of examples from the important custom-made trade. A further problem was that, while the museum wished to collect some of the textile manufacturing equipment, these items were not usually for sale. Biman weavers normally inherit their tools or have them specially made by their male kinsmen, in which case they hardly ever appear in the market-place; also, since they are not items of common trade, it was difficult to put a price on them. These specimens therefore had to be acquired by a variety of often complex transactions which it would be tedious to list here. In summary, however, the items were secured for the museum either by direct purchase or by being made to order. It is the

latter method I propose to discuss here, with reference to the largest apparatus brought back, which was the back-strap loom.

During fieldwork I had regular contact with a local cultural organization comprising a group of enthusiasts who were interested in regional history and the performing arts. After I had told them that I was interested in purchasing a loom they introduced me to one of their members, a well-known carpenter called Idrus Yahya, who agreed to build one for me. As Idrus had to buy special materials and equipment I arranged to pay him in advance; and to protect the investment we drew up an official contract, written in the national language, which was in accordance with Indonesian law.

Idrus commenced work in May 1981 and a deadline was set at the end of September the same year. As the component parts of a loom are subject to dissimilar stresses when in operation, they are made from different timbers selected for their appropriate qualities, and it took longer to gather the materials than was anticipated. Furthermore, a completely new Biman loom is seldom made, the new parts usually being added to an existing apparatus as old sections wear out. Therefore it was beyond anybody's experience to be able to predict exactly how long it would take to build it. As it happened we were not able to start entering the thread (warping-up) into the loom until February the following year, and so were fortunate to have commenced the project fairly early in my fieldwork.

In order to keep the project running I had to keep calling on Idrus to provide encouragement and monitor his progress. Despite the irritation of having to make constant visits, which increased in intensity as we went beyond the deadline, I did have the rare opportunity of observing at first hand many aspects of local carpentry; and the data gained through this kind of participant observation later formed an invaluable part of the doctoral thesis. What was interesting was that the design for the carvings which were to embellish the loom were, like the designs for the textiles, quite eclectic. Some motifs were traced from wall panels in the Sultan's palace, while others were copied from old looms, which in turn were combined with yet further ideas from the carpenter's imagination. Usually the customer is expected to participate in the design process by adding suggestions or even helping the craftsman though, being a novice in local terms, my role seldom progressed beyond holding steady pieces of wood. We both agreed, however, that the loom would be more complete if fitted with a rattle of the type once used by unmarried women, although this mechanism is seldom employed today.

Since the loom was required to be in an operational condition to provide an example of the supplementary weft technique, it was necessary to set it up and weave a portion of the fabric (because the component parts of a back-strap loom are mainly held in place by the warp threads, some degree of weaving was of course a basic requirement). Two women from a nearby village, Halimah M. Said and Siti Samsia, arranged to take on the task,

and between us we selected the materials and designs. By this time many people had become interested in the project, a number of whom (including the eldest daughter of the last Sultan of Bima) provided drawings of motifs or old textiles for reference purposes. The work progressed swiftly and the only problem occurred when, on reaching the half-way point, there was general agreement that part of the design did not suit the remainder of the textile. The weavers decided to unpick approximately eight inches of fairly finely woven cloth and try another series of motifs which, fortunately, were successful. This reworking delayed the textile's progress and the women were still weaving it on the morning of my departure from the field. Even then they were slightly reluctant to part with it as it was incomplete.

While there were immense difficulties in arranging the complete manufacture of complicated specimens for the museum, there were, in addition to its usefulness for research, two advantages. Firstly, it involved the museum in some minor patronage of craftsmen in a poor country, which may not have been possible had the modest funds been diverted by dealers and other middle-men. In the second place, since the Indonesian government is concerned about the removal of items of local significance, there might have been difficulties in obtaining shipping permits for older specimens. Because the specimens were new they came under the commercial category of handicrafts, the export of which the government is keen to encourage.

For museum specimens to be of value, either to the scholar or to the general public, they need to be well documented. In the case of this collection the documentation was likely to be greatly increased by the information contained in the thesis. In addition to my thesis field notes, I kept a collection notebook in which I entered as many details about each specimen as could be gathered. Where possible, I recorded the local names of the objects and their components, and in what circumstances they had been used. The source of each item was included and in some cases whether it had been moved since its original manufacture. The names of people involved in making, owning or selling the object were also noted, and occasionally it was possible to record their (or other people's) opinion about it. Also of importance was the price paid for each specimen, whether in money or in kind, and the date of the transaction, because these might be of interest to future researchers. I added sketches of the specimens in the notebook to aid identification in Britain and attempted to provide labels for each item, though both of these tasks proved difficult to achieve in the final rush at the close of fieldwork.

Since the collection was to be shipped across three islands along sometimes extremely rough roads by assorted means of transport, the packaging had to be secure. The large specimens were dismantled, and we made extensive use of cigarette cartons for padding and plastic bags for protection from damp and dust. There was an abundance of locally-grown wood for use in packing cases and it was both cheap and straightforward to arrange the

construction of purpose-built crates. The collection was finally air-freighted from Bali, which is an important commercial centre for the export of handicrafts. In this case it would have been more expensive to take the collection to a major Javanese port or to Singapore for shipment, as this would have involved long and costly overland journeys and possibly delays in various harbours with extra hotel bills as a consequence.

The field collector may and should do all within his power to recognize and overcome subjectivity and ethnocentric bias in an attempt to approach Sturtevant's ideal for obtaining ethnological specimens, and I have shown some of the ways I tried to achieve this while collecting in Bima. Yet however informed and careful one may be, it must not be forgotten that each field situation imposes its own host of problems beyond practical control: droughts or floods may affect accessibility; transport may not operate; sickness may cause delays; items may be lost or damaged in transit. Academic ideals and field realities always conjoin in the resulting collection.

MICHAEL J, HITCHCOCK

## THE WEDDING RITUAL AMONG THE KEL FERWAN TUAREGS

In a paper previously published in *JASO*, I gave an analysis of the name-giving ritual among the Kel Ferwan, a Tuareg group of northern Niger.<sup>1</sup> I would now like to follow this up with a description and analysis of the wedding ritual among these Tuaregs. First of all, however, I must give a few sociological details about these people, by way of background to my study.<sup>2</sup>

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This is a revised version of a paper given at the Institute of Social Anthropology, Oxford, in Michaelmas Term, 1982. The author is most grateful to Jeremy MacClancy, Maryon Macdonald, Helen Lambert and Brenda Miller, who have helped to give his English a more acceptable shape than it would otherwise have had.

<sup>1</sup> D. Casajus, 'Autour du rituel de la Nomination chez les Touaregs Kel Ferwan', *Journal of the Anthropological Society of Oxford*, Vol. XIII (1982), No.1, pp.57-67.

<sup>2</sup> For further details, see D.Casajus, 'La Tente et le Campement chez les Touaregs Kel Ferwan', *Revue de l'Occident Musulman et de la Méditerranée*, Vol. XXXII (1981), pp.53-70.

## I

The Kel Ferwan form a confederation of tribes roaming the plains surrounding Agadez. One of these tribes might be called 'noble'; the others are 'commoners'. The wedding-ritual is always the same, whether of the noble tribe or of a commoner one. The Kel Ferwan are nomads living in camps composed of a man and his wife, their divorced or unmarried daughters, their sons and daughters-in-law and, of course, all the young children of the couples living in the camp. Each married couple lives in a tent with their young children. The important fact is that this tent belongs to the woman, she having received it from her mother when she married. More precisely, when one of her daughters marries, a woman gives her part of the components of her own tent and keeps the rest; the missing components are then made up again by slaves or blacksmiths.

When a young boy enters puberty, he no longer consents to live in his mother's tent, but makes a precarious shelter of branches, which he might share with other boys of the same age. He will not live in a tent again until he marries - that is, when he is twenty-five or thirty years old. Even then he will, in a sense, be only a guest in the tent he enters at that time. In the case of divorce he will again be deprived of a tent, and will revert to the precarious position of an adolescent, to the detriment of his social position. A widower, even if he is an old and respected man is, as far as a tent is concerned, in the same position as an adolescent.

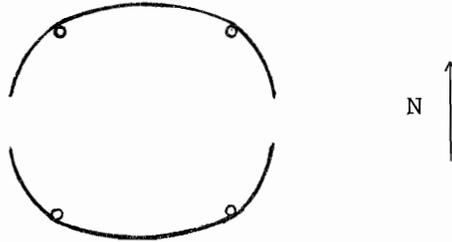
Girls, on the contrary, are married at a very early age, around fourteen years. Their first marriage is often short, ending in divorce, but they are not deprived of their tent once they have possession of it. Once divorced, a woman comes back to the camp of her father with her tent, and there she can use her tent in whatever way she wants.

A woman has the same status in her own tent as she has in her mother's: she is at home in both. Conversely, a man is a guest in his wife's tent, although when he was a young boy he held the same position as his sister in his mother's tent. The Tuaregs comment on this in saying that 'a woman's tent is her mother's'. It does not mean that a woman's tent actually belongs to her mother, but that in her own tent she has the status she had in her mother's tent. The fact that a woman's tent is partly made of elements from her mother's tent is a way of emphasizing this social phenomenon.

It is, therefore, understandable that the Tuaregs regard a tent as a female domain, in relation to which a man is in some sense a stranger. The woman is said to be the 'tent-custodian', and sometimes she is called a 'tent' (*ehan*). A man might speak of his wife as 'his tent'. To marry, for a man, is 'to make a tent' (*āgu ehan*) or 'to enter a woman's tent'. The womb is also called 'a tent'. Women of the tribe can collectively be referred

to as 'those-of-the-tents' (*tin-inan*). It should be emphasized that this close association of woman and tent is not due to some substantial identity between them, but is the consequence of the fact that throughout a woman's life her status remains the same vis-à-vis the tent, whereas it is the lot of a man to move from one tent (his mother's) to another (his wife's) in which his status is quite different. Being linked to two almost identical tents, a woman is linked to *the* tent, one could say to the Tent.

The Tuaregs also say that the tent is similar to the cosmos. Their tents are arched, mat-covered constructions. The vault of the tent is compared to the sky-vault, and its circular shape to the circle of the world,



The four principal posts are regarded as being similar to the four pillars sustaining the sky-vault. No human being has ever seen these pillars, but it is said that God, in order to teach men how to build their tents, placed four stars in the sky in the same positions (NE, NW, SW, SE) as the four pillars (these stars constitute what we call the Pegasus square).

Two rectangular mats are placed on either side of the tent, allowing it to be open to the west and to the east. The normal entrance of the tent is on its western side. It is possible - but not at all usual - to enter the tent by the eastern side. One does not enter the tent by the northern or the southern side, except, as we shall see, during the wedding ritual.

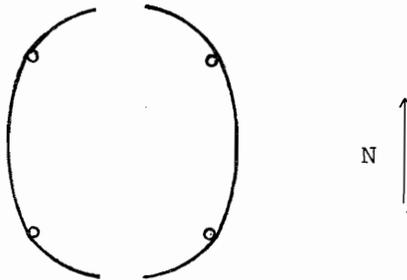
During the day, the two lateral mats can be lifted in order to permit some airing, but at sunset the northern side must be thoroughly closed. It is said that to the north of the tent live the *aljinan* or *kel esuf*, the 'spirits', and they become particularly dangerous at sunset. *Aljinan* (singular - *aljin*) comes from the Arabic *jinn*. Tuareg *aljinan* are in some respects similar to Arab *jinn*. The northern side of the tent is, therefore, its dangerous one. It is also its male side, the southern side being female. In bed, the spouses lie in a west-east direction, the husband being to the north, the wife to the south. He thus protects her against the *jinn*.

## II

The wedding tent, which after the marriage will be the tent of the newly-married couple, is put up near the camp of the bride's family. This, I think, shows that it is in the process of moving into another camp. On the day of the ceremony the bride spends the day in a tent of her family's camp. The bridegroom stays in his own family's camp or in a bivouac at some distance from the bride's camp. All the guests - those of the two families - will spend the day in or near the bride's family's camp. The bridegroom is assisted by a best man who accompanies him during the entire ceremony, while the bride is assisted by a bridesmaid. Apart from the best man, only some friends and cross-cousins spend any part of the day with the bridegroom.

The first ritual act is that of henna-coating. The bride and the bridegroom, each in their own camp, have their feet and hands coated with henna. After this a bull given by the bridegroom is slaughtered near the bride's camp, and then cut up by the blacksmiths of the bride's family. Some songs and dances accompany this sacrifice.

At sunset the bridegroom, preceded by his own blacksmiths, is led to the wedding tent. Some female relatives of the bride, on the western side of the tent (the side normally entered), pretend to prevent the groom from entering. This tent is not put up in the usual way. The two lateral mats are placed in such a way that the two entrances of the tent are not situated to the west and east, but to the north and south,



As his friends pretend noisily to negotiate with the women at the western entrance of the tent, the bridegroom and his best man discreetly enter the tent by the northern side. Since there is a horizontal beam on the southern and northern sides of the tent, the bridegroom is obliged to enter it by crawling on his hands and knees. Once he has entered, his friends laughingly say to the women: 'Well, he is inside now, we are afraid there is no longer any use in negotiating.'

Before the arrival of the bride, her young friends - girls only - attack the tent where the friends of the bridegroom are crowded. They try to snatch something belonging to the bridegroom (for example, a ring, a bracelet) or to touch the top of

his veil. It is said that if they succeed in these activities, the bridegroom will become a henpecked husband. But the friends of the bridegroom assist him and threaten the girls with their swords and riding crops. It should be noted that the men on the one side and the women on the other are mixed, regardless of kinship relationship. This contrasts with the beginning of the ceremony, where all the guests are strictly separated in groups - bride male guests, bride female guests, bridegroom male guests etc.

Very late in the night - sometimes just before dawn - the bride is led to the tent in procession, leaning on an old woman. The procession circles anti-clockwise three times around the tent, and the bride is finally led into the tent on the southern side. This entrance is made after negotiations, at the end of which the bridegroom's friends are supposed to give the bride's friends some presents.

The guests then depart, leaving the couple alone. It is said that, when they are alone, the bridegroom *must* be the first to speak if he does not want his wife always to be the first to speak in their future life together. The marriage is not consummated before the third night. On the first night the bridegroom must behave as if he were the mother of the bride; on the second night, as if he were her little sister; and on the third night, as if he were her male cross-cousin: only then does he become her husband. (There is a joking relationship between cross-cousins, and the ideal marriage is between them). This means, of course, that the bridegroom must be gentle and motherly on the first night, and cheerful and willing to help on the second night; but it is remarkable that he must act as if he were a *woman* - sister or mother - during these two nights.

Before sunrise, the couple return to their camps, going back to the wedding tent at night. For seven days the tent will be empty during the day and only occupied by the couple at night. On the seventh day the side-mats are ceremoniously put back in the right position, and the tent is dismantled. The two spouses then move with their tent to the husband's family camp.

### III

The bridegroom enters the tent by the northern side, which is both the male side and the side of the *jinn*. Let us consider first the fact that it is the male side. A woman gives birth on the southern side of the tent. A boy is thus born - as is a girl - at the southern end of his mother's tent. The name-giving

ritual<sup>3</sup> feminizes a new-born boy. In particular, one of the two names he receives is chosen by his mother and is often a feminine word. In his mother's tent a small boy does not actually appear as a boy, and at puberty, when he begins to become a man, he leaves this tent. By entering the tent on the northern side, the bridegroom affirms himself as a husband, the man of the tent. The bride, on the contrary, enters the tent by the southern side, the side where she was born. Even if this tent is new for her, her status will remain the same as the one she had in the tent where she was born; her place of entrance marks this continuity.

On the other hand, the bridegroom is a guest in the tent he enters as a husband. This explains why he must wait for three nights before consummating the marriage. When asked about this custom, informants compare it with the following one. When a traveller comes to spend some days at a camp he must first spend three days (in Tamacheq, three nights) just outside it. During these three days the inhabitants of the camp do, of course, observe all the necessary requirements of hospitality. Women prepare food for him and men bring him tea. After these three days he takes his luggage and is admitted into the camp itself, where he can then share the same plate as the other men.

The fact that the bridegroom must act as a woman during the first two nights does not actually mean that he is in some sense feminized or that something feminine in him has to be expressed. On the contrary, it is because he is, for the first time in his life, acting as a man in a tent, that the female character of the tent is made apparent in this way. Acting as if he were a woman is the recognition he has to pay to the female character of the tent. It should be noted that all such restrictions imposed on him are, in a sense, counterbalanced. He has to make sure he will be the master of the tent: he must not have any of his jewellery snatched away, he must be the first to speak when he is alone with the bride, and so on. Something, therefore, is left to chance. His status in the tent is not quite secure.

Let us consider now the fact that the side by which the bridegroom enters the tent is also the side of the *jinni*. Informants compare the unusual orientation of the tent to the orientation of graves. There are two entrances to the wedding tent, one on the northern side and the other on the southern side. Although the bed itself has its usual orientation (west-east), informants compare the orientation of the entrances with the fact that the dead lie in their graves in a south-north direction.

Most informants merely comment on the henna-coating by saying that it is a pious custom recommended by the Prophet Mohammed. Henna coating is, indeed, quite common in the wedding ritual of other Berbers and among Moslems in general. However,

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<sup>3</sup> See D. Casajus, 1982.

some informants, after I had been in the field for years, discretely took me aside to tell me that the henna-coating in the wedding ritual had to be compared with the fact that the dead also had their hands and feet washed with henna. The orientation of the entrances (and the consequent fact that the bridegroom enters the tent by the northern side) and the henna-coating are thus evocative of funeral rituals. There is even a proverb saying that there are two wedding rituals: one in this world and one (the burial) in the next.

The reason for all these evocations of death is that, when entering the wedding tent, the spouses are, in fact, entering the tent where they will die. For the bridegroom it is a new tent, but not for the bride. One could object that upon marriage a woman enters the *camp* where she will die; but the Tuaregs consider that one dies in a tent and not in a camp. There are two reasons for saying this. First, except for those who die in war and are buried on the battlefield, a dead person receives the funeral rites in a tent. Of course this tent is situated in a camp, but here the second reason becomes apparent. Tuaregs say that after death, men (and women) become *jinn* who will always haunt the tent where they died. This tent is inherited matrilineally and so moves from one camp to another. One can thus say that for the Tuareg it is the tent and not the camp which is of greatest importance.

When entering the tent by the northern side, the bridegroom announces that he will some day, as a *jinni*, try to enter this tent by the same side. Of course, the bride will also try to enter the tent. But the marriage makes no difference for her as regards her relationship to the tent. The tent she enters upon marriage is essentially the same as the one in which she is living. However, divorce is frequent, and a man does not necessarily die in the tent where he married for the first time. Furthermore, it is at the occasion of his first marriage that the ritual is performed the most scrupulously. But he will have the same status in all the tents he will live in henceforth, one of those being the tent in which he will die. *He is not exactly entering a new tent, but a new status in the tent. He will still have that status in the tent in which he dies.* We have seen in our introduction that the tent, built on a cosmic model, has, in a sense, an eternal character. Now we encounter this eternal character again: the tent is the last place where human beings live in this world and the next.

According to informants, the wedding ritual is a prefiguration of death for the bride as much as for the bridegroom. If the ritual does not deal with each of them in the same way, it is because they do not have the same relationship with the tent in which they will die. Similarly, the henna-coating is for each of them - but in a different way - an evocation of their death. For the bride, coating her hands and feet with henna is quite normal, whereas marriage and death are the only occasions on which a man is coated with henna. Similarly, entering the nuptial tent is for the bride no different from entering the tent in

which she was born. Unlike the bridegroom, she has always been in proximity to her own death.

There is one last point to make here: at the same time as he enters the tent as a husband, the bridegroom effectively announces that he is entering a tent he will haunt (or a tent equivalent to the one he will haunt). One can now understand how the northern side of the tent is both his male side and its *jinn*-side.

The symbolic 'death' of the bridegroom has a further specific meaning for him. We remember that before she enters the wedding tent, the bride circles it three times. The bridegroom lies still and silent inside the tent. He is surrounded by his young friends. Each time they circle the tent, the friends of the bride address the men inside with the traditional Moslem greeting. The men do not answer the first time, and the girls are supposed to ask themselves: 'Are they all dead in this tent?' However, the men answer the second time, in a very low voice, and the third time they answer quite openly. Notice that this hint of death now concerns only the bridegroom and his friends. Furthermore, these three circuits around the tent are compared by informants to the following custom. When one passes the grave of a holy man, one should circle it three times in order to receive the *albaraka* of the saint lying there. *Albaraka* derives from the Arabic *baraka*, and means 'divine blessing'. A man has not, of himself, any *baraka*. He can only be the means through which Allah chooses to display His *baraka*. Here, the bridegroom is again considered similar to a dead person, even to a dead holy man. In this comparison, of course, the bridegroom's friends are in the tent with him when the bride circles around it, and are thus associated with him. But during the ritual they are called the 'bridegrooms' (conversely, the bride's friends are not called the 'brides') and so considered as similar to him. It is only insofar as they are similar to him that they have access to the same dignity as him. Some of his temporary saintliness spreads around him and reaches them. *Baraka* is in some ways synonymous with fertility. So the bridegroom, although he is 'as dead', and although he does not act as a man at this time, he is the means through which Allah gives fertility to the tent and to the bride circling it.

The analysis of songs accompanying the slaughter of a bull can reinforce this interpretation. I shall not enter into details here. Let me just point out that, in these songs, the bull is regarded as similar to the bridegroom and has a divine character. This is not as surprising as it might seem at first, for it is yet another indication of the bridegroom's symbolic death. The bull is similar to the bridegroom and has a divine character; this does not mean that the bridegroom has any divine character, but simply that he is the instrument of Allah, the means through which Allah has chosen to display his *baraka* to the tent.

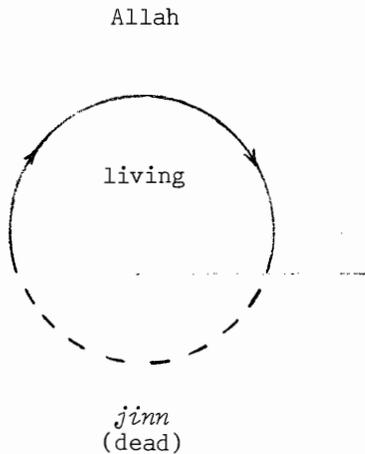
We can conclude this specific point as follows. The wedding ritual announces to the bride and bridegroom that they will die

in the tent they are entering. For the bridegroom, this assumes an especially dramatic meaning. Moreover, in another part of the ritual the bridegroom is regarded as similar to a dead holy man. This has the consequence of bringing fertility, or life, to the tent. Here the bridegroom has a quite passive role, like holy men, whose *baraka* spreads passively from their graves.

This can balance our previous statements. The bridegroom is a guest in the tent he is entering, and he is strongly reminded that he will die in this tent, which is a little humiliating for him. The fact that he has to enter the tent almost crawling emphasizes this humiliation. (The bride does not have to crawl but is carried inside). At the same time, however, this symbolic death gives him the opportunity to bring Allah's *baraka* into the tent. His death is in a sense necessary for the tent to reproduce itself. It is the reason why, though the bride is entering a tent which is already hers, she can only enter it *after* the bridegroom.

Here arises a problem. We have on two different occasions spoken of the dead. We have seen them as *jinn*, malevolent spirits wandering in deserted areas, and we have seen them as holy men, intermediaries between Allah and human beings. In two different episodes of the ritual the bridegroom has been considered similar to each of these two kinds of the dead in turn; this is contradictory, unless we can find further explanations.

I have shown elsewhere<sup>4</sup> that at their birth, also, the Tuaregs are similar to the *jinn*. They thus come from the world of the *jinn* and return to it when they die. The human condition is viewed in this way, as a sort of circular travel from the world of *jinn* and back to it. We can symbolize this travel as follows.




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<sup>4</sup> *Ibid.*

Part of the circle is drawn as a dotted line because there is no belief that men are reincarnated, as is the case in other societies. Even during their lives, men are very close to *jinn*, having to make efforts to distinguish themselves from them and to escape their malevolence. But there is a space transcending that dramatic confrontation between living and dead (*jinn*), and it is Allah who occupies it. If the human condition is an eternal confrontation between living and dead, Allah, who is eternal, has no part in this confrontation and transcends it.

It is sometimes offered to humans to partake of this transcendence. This is the case, very temporarily, for the bridegroom; it is the case for some holy men; it is also the case for all the dead, during the days or the weeks following their death. In fact, as long as the dead are not forgotten and some of those living remember their name, one cannot be sure that they actually are *jinn*. One cannot say: 'So-and-so is a *jinni*.' One can only say in a very general way: 'Dead people are *jinn*.' Only anonymous dead are certainly *jinn*. There is no genealogical memory, and only when the dead men enter the anonymous mass of ancestors are they likely to become *jinn*. Some holy men, because of the pious memory the living have of them, escape this oblivion. Perhaps they are *jinn*, but who knows? Certainly, however, when one pronounces their name or circles piously around their graves, or practises certain sacrifices near their graves, one does not view them as *jinn*.

I still have to comment on the mock struggle around the tent between men and women. The principal form of exchange inside the tribe is marriage. Marriage can be considered an exchange in which camels (bridewealth) are given by one side and a tent, a woman, sheep and slaves (dowry) are given by the other. War, and especially feuding, can also be considered an exchange in which men riding camels bring slaves and cattle back to their tribe. War can thus be viewed as an extension outside the tribe of the exchange within it. From this point of view, there is some analogy between war and marriage, and the mock struggle between men and women around the tent is a hint of that analogy. As I have said, men and women face each other, regardless of their kinship relations, as if they were strangers, just as warriors face one another as strangers on the battle-field.

But of course the analogy cannot be complete. In point of fact, the mock struggle around the tent hints at war in a very special way. Men frequently attacked camps during feuds, entering the tents and snatching jewellery from the women inside (and sometimes worse). In the case of the mock struggle, it is women who are besieging the tent and trying to grasp the jewellery of the man inside. I have said that women are the tent custodians, but in this ritual, men act as if *they* were tent custodians. There is at the same time a hint of the feud and a complete reversal, which corresponds to the reversal actually existing between war and marriage. Men killed in war are buried

on the battle-field. They do not return to their country to haunt the tent of their wives. Marriage introduces a man into the tent in which he will die. It supplies the tent with future dead. War, on the contrary, takes men away from the tents. In war, men take death outside and are taken away from the tents. We could say that men receive death through marriage, if not from women, at least from the tents. This is probably what informants are referring to when they explicitly compare the bridegroom entering the wedding tent with a man entering a hyena lair.

To conclude, let me point out that this short study is, at a quite modest level, a confirmation of one of Robert Hertz's intuitions. In my attempt to understand the wedding ritual of the Tuareg, I found myself compelled to speak of death as well as marriage. In this paradoxical situation one is reminded of Robert Hertz's words: 'Death is not originally conceived as a unique event without any analogue'.<sup>5</sup> Certainly in the Tuareg case marriage is the first step towards death.

D. CASAJUS

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<sup>5</sup> R. Hertz, 'Contribution à une étude sur la représentation collective de la mort' [*L'Année sociologique*, 1907], in *Death and the Right Hand* [trans. R. and C. Needham], ed. R. Needham, London: Cohen & West 1963, at p.81.

## RECENT BOOKS ON SOUTH ASIA

ADRIAN C. MAYER, ed., *Culture and Morality: Essays in Honour of Christoph von Fürer-Haimendorf*, Delhi etc.: Oxford University Press 1982. xvii, 261pp., Index. £10.00.

T.N. MADAN, ed., *Way of Life: King, Householder, Renouncer: Essays in Honour of Louis Dumont*, Paris: Editions de la Maison des Sciences de L'Homme 1982. x, 418pp., Index. FF140.

These *festschriften* are dedicated to two of the leading figures in South Asian anthropology, but though they cover the same general area they are quite different in content and orientation. The differences between the two books no doubt reflect the varying interests and achievements of the respective honorands. The Haimendorf volume has thirteen contributions which span a wide range of different social groups, and its emphasis is on ethnography. The Dumont volume contains sixteen papers, most of which are longer than those in the Haimendorf collection, and one essay by the editor reviewing the contributions and the current state of studies 'for a sociology of India'. The book is mainly concerned with the ideology of Hindu society.

In both cases the books are organised around a theme, and in this way some measure of homogeneity is achieved. The Haimendorf collection is focused on his book *Morals and Merit* (1967), an obvious choice since it is his main work of a more theoretical and general nature and probably also the one most widely known. The contributors are all anthropologists, and are drawn from those colleagues or students of Haimendorf's at SOAS who have a continuing interest in South Asia. By contrast, the Dumont *festschrift*, written to honour him on his seventieth

birthday in 1981, contains contributions from historians, Indologists, sociologists, social anthropologists, and philosophers. The thematic unity is provided by focusing on the Brahmanical concept of the 'good life' as contained in the ideology of *puruṣārtha*.

Mayer's introduction gives a brief outline of Haimendorf's career and emphasises his considerable achievements as a field-worker, noting that indeed few anthropologists 'can point to more than ten years spent in the field' especially in as many different and difficult locations. We are given a glimpse of Haimendorf's interests and activities outside his professional life, and are told of his deep concern with the duty of the anthropologist to record faithfully the nature of each culture encountered, and to present data which can help improve the lot of the people studied. Galey's introduction to the other collection emphasises the continuity in the development of Dumont's ideas and highlights certain features of his contribution to the social sciences which have been neglected, mentioning in particular Dumont's ethnographic monographs, his concern with the central opposition between 'traditional' and 'modern' universes, and his preoccupation with situating the social scientist in his analytical endeavour. In contrast to Mayer's introduction, we learn about Dumont through his own words in the transcript of a conversation between him and Galey, and this is successful in providing a welcome personal touch and insight into the unfolding of his career. It is worth adding that Dumont's comments hold out the hope that his long-awaited monograph on mourning in north India will be published.

The bias towards ethnography in the Haimendorf collection leaves little room for discussion of more theoretical questions, such as how one is to delineate and treat an area of social life which can be labelled 'morals and ethics', but this does not detract from the interest of the essays themselves. The papers range throughout the sub-continent. In the north there are three essays relating to Nepalese societies, two of which describe Buddhist communities (Aziz and Levine), and one deals with Newar priestly castes (Greenwold). Two papers concern Vishnavism, with Nicholas examining differing 'understandings' in the worship of the *tulasi* plant in Bengal, and Cantlie discussing the significance of food in Assam. There are three papers on Indian tribal groups, of which Standing's provides an interesting account of envy among the Munda, while Bailey seeks to explain why it is that the Konds do not have 'saints' when their Hindu neighbours do. Caplan's analysis of a dispute in a Christian hospital and Mayer's paper on *seva*, the idea of unselfish service, are both set in urban contexts. In the south, Rao gives an account of two anti-caste movements in Kerala, and Tapper analyses how the altruistic ideals expressed in a play about King Harischandra relate to everyday reality. Madan's essay, the only one not linked to a particular locality, discusses pan-Indian themes. He examines how three Indian novels illustrate the moral dilemmas involved in the conflict

between asceticism and eroticism.

The production of the book is good, with the only major fault being the omission of several references at the end of the last essay. With its ethnographic orientation this collection contains much of interest not only for South Asianists, but also for a wider anthropological audience.

The essays in the Dumont *festschrift*, on the other hand, benefit from some familiarity with his ideas and with Hinduism more generally. The volume contains papers concerned both with sociological problems and with metaphysical issues and textual questions. This reflects Dumont's (and Pocock's) proposal that for the study of India, Indian civilisation must be taken as a whole, and Indology and sociology must come together in an interdisciplinary approach. The influence of Dumont's ideas is evidenced throughout the collection. His emphasis on a holistic approach, his concern with ideology, his notion of hierarchy with the entailed distinction between status and power and so on, all are taken up in one way or another by the contributors, all except two of which directly link their essays to his work.

Taking the *puruṣārthas* together and relating Dumont's notion of hierarchy to them, Malamoud finds that it is difficult to establish the encompassing character of any one of them, and argues instead for a 'revolving hierarchy', though *mokṣa* remains separate, since it is of quite another order from the other three. Similarly, in his essay on *ārtha* Shah points out the complementary nature of the *puruṣārthas*. Four essays deal with kingship, though of these it is Marglin's which more than the others takes Dumont's distinction between status and power as its starting point. Three essays discuss renunciation, reflecting the importance of Dumont's perception of the tension or dichotomy between the renouncer and the man-in-the-world. On death, Nicholas's view that in *śrāddha* rites the paradigmatic action is one of birth or rebirth, seems to find a striking parallel in Parry's conclusion that in Kashi (Benares) 'good' death constitutes a sacrifice which recreates the cosmos. Among the other papers, Das discusses *kāma*, Madan argues that, for the Kashmiri Pandits, being a householder constitutes a variety of renunciation, and Selwyn examines the relation between *dharma* and its dark side, *adharmā*. Two contributions stand out as somewhat different from the others. Ramchandra Gandhi's rather unusual essay defending *brahmacārya* or celibacy, may baffle some readers, not least anthropologists who will be startled by such comments as: 'Only on the basis of the theory of reincarnation can a satisfactory explanation of incest prohibition be developed'. Pocock's paper, which is the most ethnographic, points to the problems related to the extent to which 'social anthropologists are prone to accept the ideology of caste as social reality'. He shows how the majority of the members of a caste of temple priests in fact do not pursue their traditional vocation but work in a variety of other jobs, and he makes the important point that often anthropologists' representation of

castes as associated with traditional occupations has a homogeneity which fits better with *varṇa* theory than with social reality.

In his conversation with Galey, Dumont expresses the belief that the researcher's work should be seen as part of a collective endeavour. In this regard he feels it unfortunate that his work, which was oriented to the community of researchers and was intended as a contribution to the collective construction of theory, paradoxically has been interpreted, and achieved recognition, as 'personal'. We are told that this is due in part to the conditions under which we work, which force us to move away from a collective orientation to a more personal stance, with the consequence that the stability and cumulative progress of our work is undermined. One must agree with Madan, however, that this volume forms a contribution to this common task, and is itself evidence of the influence of Dumont's work in establishing some common ground for discussion. At the same time it is a fitting tribute to one who has been so influential in the making of the sociology of India.

CHRISTIAN McDONAUGH

LOUIS DUMONT, *Affinity as a Value: Marriage Alliance in South India, with Comparative Essays on Australia*, Chicago and London: University of Chicago Press 1983. x, 214pp., Bibliography, Index. £17.50.

LINA M. FRUZZETTI, *The Gift of a Virgin: Women, Marriage, and Ritual in a Bengali Society*, New Brunswick: Rutgers University Press 1982. xiv, 134pp., Appendixes, Glossary, Bibliography, Index, Illustrations. \$22.50.

It is an excellent idea to make some of Louis Dumont's most significant articles available in a single accessible volume, if an obvious one in view of their author's undoubted importance as a leading thinker on Indian society (and Western society, in the light of it), and on kinship. The present volume can be said to honour his work on the latter rather than the former, since it gives attention to Australian as well as to South Indian kinship, and touches only tangentially on the question of caste. It is a corpus which can bear some highlighting, particularly for those who still think of Dumont primarily as an analyst of caste and other hierarchically-structured conceptions of society. (This is perhaps the point to enter a plea for the issue of some of the articles from *Contributions to Indian Sociology* in a similar handy format.) More particularly,

this book might at first sight be described as dealing with terminological systems of prescriptive symmetric alliance; but we are left in no doubt here as to the superficiality of comparing Dravidian and Australian systems as like and like.

Five of these six papers have appeared before (two only in French), though the author has added new Comments (dated 1981) to the first and third of these, and shorter Postscripts to Chapters 5 and 6; in addition, occasional emendations have been made throughout. The papers previously published are (with original publication details in parentheses): Chapter 1, 'The Dravidian Kinship Terminology as an Expression of Marriage', including the subsequent correspondence with Radcliffe-Brown (*Man*, 1953), in which the Dravidian kinship terminology is used to illumine the equal presence of alliance (or 'continuous affinity' in contradistinction both to consanguinity in indigenous terms, and to descent theory in terms of contemporaneous anthropological analysis; Chapter 2, 'Hierarchy and Marriage Alliance in South Indian Kinship' (Royal Anthropological Institute, 1957: Occasional Paper No. 12), which amongst other things adumbrated the structuralist, in contrast to the functionalist study of Indian society, a process eventually to culminate, of course, in *Homo Hierarchicus*; Chapter 3, 'Nayar Marriages as Indian Facts' (*L'Homme*, 1961), which set out to bring the apparently anomalous Nayar back into the Indian fold sociologically, and not merely culturally, as Gough had done; Chapter 5, 'The Kariera Kinship Vocabulary: An Analysis' (J. Pouillon and P. Maranda, eds., *Échanges et Communications: Mélanges offerts à Lévi-Strauss*, Paris: Mouton 1970), in which he explodes the myth of the *total* similarity (cf. Radcliffe-Brown, etc.) between Dravidian and Australian alliance systems, and distinguishes the 'global' dualism of the former from the merely 'categorical dualism' of the latter (pp. viii-ix of the present volume); and Chapter 6, 'Descent or Inter-marriage? A Relational View of Australian Section Systems' (*Southwestern Journal of Anthropology*, 1966), in which he challenges the once received wisdom about four-, etc., section marriage systems - that they are based on systems of double unilineal descent - and demonstrates the lack of real evidence for any such phenomenon in Australia.

The Comments to the first and third chapters - and the whole of the fourth - provide some welcome clarification of and enlargement upon the theses of the original papers, but scarcely present retractions from the positions held previously. Quite the reverse, in fact; in respect of the Comment to Chapter 1, for instance, those who have previously found it difficult to swallow the view of one's mother's brother as an affine will surely choke on the emphatic reaffirmation of one's father's sister as the same class of relative - but now we are told that she appears as an affine more for a female than for a male ego, 'affinity holding strictly between persons of the same sex...'; with parallel consequences for one's mother's brother, one supposes. However, the author does now regard his isolation of

the terminology as a sub-system of the kinship system as less useful than formerly, especially in view of the subsequently-discovered recalcitrance of the North Indian kinship system to similar analysis. In addition, much of this Comment is taken up with a reply to Scheffler, who, as a result, is shown to be at the opposite pole to Dumont in both attitudes and understanding where kinship terminology is concerned. Similarly, the Comment to Chapter 3 tackles Yalman on his view of the *tali*-tying rite among the Nayar (roughly, is it to do with puberty, or marriage, or both?), and Fuller for his inconsistencies and fence-sitting in accepting the Nayar as sociologically Indian, while denying their integration with their 'social environment'.

This brings us to Chapter 4, the one chapter never published before, entitled 'Stocktaking 1981: Affinity as a Value'. In this the author deals - representatively rather than comprehensively - with the leading direct and indirect responses to the original versions of the papers on South India in this volume. He starts by refuting the explicit denial of Schneider and the more implicit ones of Tambiah and Scheffler 'that there is a domain or system of kinship'. He then goes on to discuss the related problem of deciding which features are 'extrinsic' and which 'intrinsic' to the domain of kinship, and the danger 'of confusing an empirical juxtaposition with a necessary combination', citing in particular the place of stable (corporate or other) groups in marriage alliance, and questioning whether instead it might not be preferable to see *relationships* as the stable factor, the one governing the nature of such groups rather than vice versa.

These two problems take up approximately only the first one-fifth of the chapter, the remainder of which is devoted to the substantive nature of South Indian kinship itself, and the questions it gives rise to. Dumont deals successively with the way in which descent theory has prevented the proper importance of affinity in South Indian kinship from being recognized (Gough, Yalman); the attempts to devalue affinity, or to avoid the issue altogether, by introducing 'overarching cultural concepts' - in particular, 'the idea of substance (as a paramount conception) ...' - in order to produce unity between North and South India on the one hand, and between caste and kinship on the other (Marriott, David, Barnett); and Carter's attempt to unify India sociologically by using componential analysis to dismiss the cross-cousin marriage of the South as a mere 'surface feature'. Running all through this, of course, is the problem of the confusion of 'levels', such that the holism of a single cultural idea is mistaken for relational structure, hierarchy is introduced analytically as a dominant or equal principle where empirically it is subordinate, and above all, there is the unwillingness, even inability, of many ethnographers to recognize in India especially 'the full status of affinity in the system, a status equal to that of consanguinity...Hence the title of this little book'. For Dumont's chief concern here is 'the assertion of affinity *as a value* equal to that of consanguinity,

the assertion, that is, of identity and relation as indissolubly solidary' (my emphasis). Dravidian is contrasted with Western kinship, where affinity in one generation becomes consanguinity in succeeding ones - wife's brother becomes uncle - so that the latter 'encompasses' the former within a 'hierarchical opposition'; in the Dravidian system affinity is constant - wife's brother becomes father's wife's brother (or in anthropological language, 'mother's brother') - and balances consanguinity in 'an equistatutory opposition'. This message underpins the whole of this edition of Dumont's papers, and as he points out: 'We have thus come not only to understand the Dravidian system, but to learn something from it about our own'.

Fruzzetti's book on Bengali kinship concentrates on the place of women within it, and on the ritual actions they perform in the marriage ceremony. Its theoretical approach in many ways represents the antithesis of Dumont's ideas, and the book exemplifies many of the points he argues against in Chapter 4 of the foregoing volume. First, the mother's brother is given consanguineal status, and the affines of one generation are merged into the consanguines of the next, so that affinity is subordinated to consanguinity, and its continuity denied. Secondly, the main aim of marriage is presented here as the continuity of the husband's line (*bangśa*), so that the spectre of descent theory is seen obtruding itself yet again into Indian anthropological analysis. We are not given any real insight into the motivation for alliance from the point of view of the girl's family, which is, after all, the source of the initiative in making the match. It may be, in this milieu where exchange marriages are both common and acceptable, that the desire for an alliance with a family of higher status is less pressing than in the Gangetic Plain. But to erase the pollution and risk of incest presented by having a virgin daughter residing at home is hardly a sufficient alternative explanation for this part of India, especially in view of the lack of regard for child marriages here (at least in higher-caste circles). Furthermore, we are left in no doubt that dowries are essential to the making of an alliance, and that they are often large; but can their significance really be economic only, as the author alleges?

Thirdly, there is the elevation of an indigenous conception of the person - in this case involving blood - to the point where, in conjunction with ritual symbols and ritual action, it is felt able to elicit the meaning of kinship. In other words, instead of being examined directly, structure is approached through cultural values, and the way they act upon - even determine, in this view - structure through the performance of ritual, so that structure appears subordinate to culture, and even to a single cultural idea. One thinks at this point of such figures as Geertz, or Turner, but never Dumont, save as an antagonist of such views. This is not a new approach, of course, and it is true that the author states her aim as the unified study of the economic, ritual and ideological aspects pertaining to Bengali marriage; but the choice of ritual as the key element

from which all else is to be explained seems risky, even arbitrary, and unlikely to be wholly successful. While one can certainly expect ritual to correlate to a large extent with other aspects of the society one is studying, it is surely unreasonable to expect (or to achieve?) a totally coherent view - there are commonly many symbolic and ritual details that are obscure in their meaning to anthropologists and indigenes alike, and it is precisely at this point that rationalisations begin to nudge out reasons. The danger of this approach is exemplified by an instance provided by the author herself. In Bengal, it seems, ritually-expressed inequality in the marriage ceremony does not necessarily reflect the sort of hypergamous ranking found in the Gangetic Plain, because of the very real possibility of the return of a bride at some stage, with its consequent reversal in the flow of prestations, etc. This, surely, is tantamount to an admission that here ritual inequality is purely situational, and has no necessary correlation with social structure.

Despite its analytical contradictions and inconsistencies, however, the book is still a worthwhile contribution to the anthropology of India. Above all, while concentration on women in this area is nothing new, there is a clear value in demonstrating that the nature of their relationship with men is not merely one of 'subordination' and 'oppression', to use a common Western stereotype, but one of complementarity within a clearly-demarcated sphere of their own. On a more modest level we have a description of female ritual action in the marriage ceremony as detailed as one could wish, if open to the objection that the author's setting-aside of the male role therein leaves us with a one-sided interpretation of limited general value. Nonetheless, we are left with a clear impression of the ideological status of women in Bengal - separate (but not isolated) from men, yet subordinate to them, in a relationship that is 'hierarchical and reciprocal at the same time'; they are thus 'encompassed by their contrary', to use Dumontian language.

Ethnographically, one of the most striking things that emerges from this study is the reaffirmation of the idiosyncratic nature of Bengali society when compared to the North Indian standard. Here we have Hindu ideology, caste hierarchy, dowries, and the inauspiciousness of the widow, but apparently no widow inheritance, more child marriages among low than high castes, and above all, as we have already mentioned, the acceptability of exchange marriages. Bengal is an area that fully deserves the increased attention it has received in recent decades, but in the long run such attention can only be expected to fuel the controversies concerning the nature of Indian society that these two books represent.

R. J. PARKIN

ROY C. AMORE and LARRY D. SHINN, *Lustful Maidens and Ascetic Kings: Buddhist and Hindu Stories of Life*, New York etc.: Oxford University Press 1981. xii, 189pp., Bibliography, Illustrations. £9.95.

WENDY DONIGER O'FLAHERTY, *Siva: The Erotic Ascetic*, Oxford etc.: Oxford University Press 1981 [1973] (Galaxy Book 650). xii, 318 pp., Appendixes, Bibliography. £5.50.

Two collections of stories from India could hardly suggest a greater divergence of interest and methodology than those considered here: one is concerned with moral truths discovered at the narrative level of tales orally transmitted in the socially-intimate context of cousins, courts and monasteries, while the other describes eternal conflicts logically resolved at the structural level of disengaged, 'free-floating' cycles of mythology. In the first text, *Lustful Maidens and Ascetic Kings*, Amore and Shinn present 65 stories selected on the basis of their entertainment and didactic value. Story-telling, designated as India's major educative device, is best understood by reference to the notions of 'model for' positive behaviour and 'image of' undesirable action. This distinction, appropriated from the anthropologist Clifford Geertz, encapsulates the authors' basic analytic procedure and serves them well in their attempt to demonstrate the moral and ethical dimensions of Indian religious lore.

The tales are organized around four major themes - Family and Social Roles and Lay and Monastic Values. Each of these is explored according to several sub-categories for which a selection of Hindu and Buddhist myths are cited as appropriately illustrative. Brief statements locate the various subjects within their social and religio-legal context and the tales themselves, rendered in a simplified and chatty style, are followed by condensed commentaries. This particular mode of presentation is potentially productive of both clarity and insight, and constitutes perhaps the most valuable feature of the book. Indeed, Amore and Shinn have treated some elusive yet important features of Indian life and, as a work which proceeds to unravel some of the socio-religious complexities of South Asia for a general, non-specialist audience, it is to be applauded.

However, some problems beset their enterprise. One of these, a certain ethnographic naivety, is readily apparent and may be briefly mentioned. For example, the variety of subdivisions within the brahman caste is not recognized, and this results in the misrepresentation of categories such as teacher, priest, and ascetic. A second problem is not so easily designated, but is a direct product of the authors' attempt to demonstrate the moral and ethical qualities which they believe inform the Indian ideological universe. It results specifically in the superimposition of a system of meaning which denies or

misinterprets notions fundamental to Indian thought. An instance of this is evident in their disregard for the role of *māyā*, that is, 'illusion'. Amore and Shinn construe human and divine intercourse in Indian tales as a grand battle within which the forces of good and evil rage, and incidents in which *māyā* prevents an actor from accurately seeing the true relation between things are curiously rendered by the authors. *Māyā* is presented as a *post hoc* excuse rather than as an essential element in the imperfect human apprehension of existence. Thus, for example, when Yudhisthira discovers his blameless relatives suffering in hell, he refuses to leave until their release. The narrator of this tale clearly states that illusion has landed them all, even Yudhisthira, there in the first place: they are released only by recognizing this truth, by piercing the veil of *māyā*. Yet Amore and Shinn refrain from comment on this central theme and instead proffer the tale as an illustration of familial devotion.

Frequently, a given tale's appropriateness to serve as the 'model' or 'image' specified by the authors is questionable. Those familiar with Hindu mythology might hesitate to accept Kṛṣṇa's slaying of the serpent-demon Kāliya as an exemplification of warrior/king as protector, or Durgā's battle with the buffalo-demon as an illustration of courage. When King Parakṣit insults an ascetic only to be killed by the latter's son, does this really demonstrate the virtuousness of teacher/priest, or does it not have more to do with the power conferred by engaging in austerities? And assigning titles such as 'Pride Goeth Before a Fall' or 'The Buddhist Job' to Indian tales, serves to obscure more than clarify.

Finally, one questions the title of the book itself with its designation of lustful maidens and ascetic kings. Not one tale gives evidence of an ascetic king. Within Indian tradition the pursuit and implementation of power is incompatible with asceticism. Even in the tales presented here, rulers abandon absolutely their throne and engage in penances in a territory distinctly separate from that of worldly power. Furthermore, in the given tales one meets few lustful women, rather an abundance of those such as Savitrī and Sitā who embody virtues of self-denial and chastity. By its very style the title, *Lustful Maidens and Ascetic Kings*, presages some form of structuralist inquiry which is certainly not pursued here; and as the content of the tales fails to validate these polarized categories, one is led to conclude that its selection proceeded with a conscious view to popular, even sensational appeal. The adoption of a structuralist posture, in a well-intentioned though inappropriate ethical straight-jacket, somehow renders the material inauthentic. The authors might have done better to let their enjoyable translations stand on their own, to allow the reader simply an exploration of Indian tales at the level of commonsensical and even nonsensical narrative delight.

That Amore and Shinn should without compunction entitle their particular book in such a manner as to imply the exploration of underlying, polarized tensions in Indian thought, is

rendered possible in the first place by the pioneering work of Wendy O'Flaherty, whose seminal study, *Asceticism and Eroticism in the Mythology of Śiva*, first appeared in 1973. Such has been its popularity that in 1981 it reappeared as an OUP paperback, although under a significantly different title. If the publishers of this book also wish to capture unsuspecting prurient interest, then an 'erotic ascetic' will surely do the trick.

O'Flaherty's study demonstrates superb expertise in Puranic literature and in the application of Lévi-Straussian structural analysis. Major *motifs* in the mythology of the god Śiva are outlined and tabled, interconnections traced, and the entirety rendered valuable as a handbook by the construction of a comprehensive coding system whose numbers appear in the page margins at appropriate points in the presentation of a myth. The basic theme of the work is that the god Śiva is ambiguous and enigmatic, the supreme ascetic yet simultaneously lustful, incarnate in the imperterbable yogi and the erect phallus. The key to unravelling this paradox lies in a structural appraisal of the entire body of Śaiva myths found in classical, tribal and contemporary sources. In this manner, the study demonstrates that attitudes, as structured categories, considered mutually exclusive in the West exist in Hindu thought in a state of complementarity.

Again, criticism has proceeded largely on the basis of the author's neglect and even rejection of ethnographic context. Society plays no role at all and culture is admitted only in the sense of other, written or expounded interpretations of mythology. By contrast, later publications of this same author (such as *The Origins of Evil in Hindu Mythology* or *Women, Androgynes, and Other Mythical Beasts*) pursue an avowedly eclectic approach, allowing a greater intimacy with the human myth-bearers. This, one suspects, is partly a product of inevitable difficulties which arise in the structuralist enterprise, particularly in the process of isolating significant categories.

O'Flaherty herself admits the dilemma - how is one to attain a proper balance, how to avoid selecting overly-specific and thus (in structuralist terms) insufficiently patterned *motifs*, and how to designate categories with a content not so broad that they approach pointless abstraction? In O'Flaherty's study of Śiva, the reader is at times victimized by this very predicament: one comes to wonder when an opposition and its mediation represent a specifically Hindu insight and when it might demonstrate a purely structuralist elucidation of universal polarities. Certain instances can be cited here. As stated by the author, it is a structuralist tenet that character motifs can, in mythological contexts, exchange function with impunity. Is the arbitrary nature of Hindu gods, the difficulty in arriving at a substantive definition of their character to be understood then as a phenomenon not particularly Hindu but rather as a universal revelation of structuralist theory? The interpretation of one Baiga tribal myth in which a brahman's severed penis kills the

tribal man who innocently eats it, suffers from this same rejection of specificity. O'Flaherty believes this myth demonstrates a universal theme - that 'the sexual act is dangerous' - and in so doing she avoids the undeniable yet socially rooted significance of the contradiction posed by the two categories 'brahman' and 'Baiga'.

The axis upon which the character of Śiva is plotted has for its co-ordinate sexuality (whether expressed as desire, *kāma*, or in the cult of the *linga*) and control (expressed in the steadfast yogi and also in the erect phallus). This theme is expanded to conclude that the entirety of cosmic life is dependent on the constant tension between eroticism and asceticism. The reader might feel ill at ease with the seemingly tautological quality of the entire argument and with its exclusiveness. It is difficult to avoid the suspicion that classification itself, that is, designation of motifs and their variants and demonstration of their dominance and mode of operation, is the product of a foregone conclusion. This is perhaps most strikingly confirmed by the relative unimportance attached to other mythologically significant aspects of Śiva's activities, particularly the relationship between him and the god Viṣṇu. One wonders if there are not *motifs* other than that of sexuality and chastity to be explored in events such as the constant interruption of Śiva's *tapas* (austerities) by the other gods. What in fact is the relationship between Śiva and the entire sphere of order (*dharma*) embodied in Viṣṇu?

Finally, preoccupation with the erotic/ascetic polarity results in a condensation of certain categories. One hesitates to accept the narrowly-implied relation obtaining between, for example, *tapas* and *kāma*, austerities and desire. These are each treated as singular and uniform phenomena, when in fact both refer to a variety of notions, and the relationship between them is not constant. Thus love, lust, the desire for progeny, or even simply erotic power may all be contained in the notion of *kāma*. Besides, there is in the case of both these categories an important distinction to be made between their efficacy as mechanical force on the one hand, or as emotional on the other. These niceties are important in determining the actual meaning they contribute to any mythological event, and thus in allocating a value to any given motif.

Certainly *Siva: The Erotic Ascetic* in paperback will secure a wider audience, one which might not otherwise appreciate either the dazzling variety of Hindu mythology or the rewarding complexities of structuralist analysis. And though O'Flaherty would consider Amore and Shinn's work to be a latter-day example of nineteenth-century pursuits in which myth is reduced to 'a moralizing comment', both works are consciously seeking popular readership. Fortunately, one factor remains constant in both works: the myths, fables, stories and tales maintain their own integrity. Despite analysis and synthesis, speculation and interpretation, the tales remain distinctively Indian in character and never fail to exert a compelling force over the

reader, to catch the imagination in the heady delight of the violent or pacific affairs of the gods and the gentle, often resigned self-mockery at the absurdity of man.

LYNN TESKEY

M.K.A. SIDDIQUI, *Muslims of Calcutta: A Study in Aspects of their Social Organization*, Calcutta: Anthropological Survey of India 1979 [1974]. xi, 120pp., Appendixes, Bibliography, Index. No price given.

This is a second edition of a monograph based on an extensive survey of the Muslim population of Calcutta. The survey was conducted as a follow-up to a broader survey of the City done under the directive of the Anthropological Survey of India in 1962-3. The earlier survey showed that Calcutta's Muslim population was residentially, occupationally and culturally distinct from the wider population of the City. Siddiqui's research was directed towards describing how national, ethnic, linguistic and caste boundaries persist within the Muslim population and his book provides much useful descriptive material on this theme. However, in examining the question of the persistence of a caste-like social organization among Calcutta's Muslims, he adds little new to our understanding of Islam in India.

Studies of the social organization of Muslim groups in India tend to be preoccupied with asking whether Indian Muslims have a caste system comparable to that of the Hindus, despite the egalitarianism of Islam. Posing the question in these terms, it is not surprising that most studies conclude that Muslim society in India represents a corruption or variant of an ideal Hindu model.

Siddiqui tackles the question of caste among Muslims by showing that the Muslim population of Calcutta is divided along national, regional and ethnic lines and into religious sects and *Khanqahs* (centres of Sufi religious training). He then describes the hierarchy of endogamous descent groups, with the *syeds* (putative descendants of the Prophet via his daughter Fatima), at the top and the sweepers at the bottom, and concludes: 'the basic structural elements of caste are present in the Muslim society of Calcutta'.

His explanation for this is that the ideology of the numerically-predominant sect, the Brelwi, and the requirements of the caste system are 'adaptive to each other'. Much of Brelwi ideology rests on accepting the clientship of the *pirs* (spiritual guides), who as putative descendants of the *khanqah*

founders (themselves *pirs* who belonged to the hereditary Silsilahs, or schools of Sufi religious training) are *syeds*. Brelwi-type Islam may therefore be seen to justify the caste hierarchy. Furthermore, since Siddiqui defines the customs and rituals of the Brelwi tradition as extra-Islamic, or outside the Islamic 'Great Tradition', he is forced to conclude that the system in Calcutta 'violates the spirit of Islam'. In other words, his view is that the social structure of the Muslim society of Calcutta represents a variation of the Hindu caste system (rather than of Muslim social structure), because the religion which justifies it is not properly Islamic. Since his informants would not question their identity as Muslims, Siddiqui adds that this 'violation of the spirit' of Islam occurs at the level of the 'unconscious'.

Although there may be some truth in his explanation, Siddiqui's failure to accept the possibility of contradictions in Islamic practice and variations in Islamic styles leads him to overlook other explanations for the apparent paradox that Calcutta's Muslim society has a caste structure. One is suggested by a reading of Gellner's book *Muslim Society*. Gellner argues that the tension between egalitarianism, on the one hand, and the need for religious organization and leadership and, as a corollary, inequality, on the other, is endemic in Islam and that one or the other aspect tends to be emphasised in different contexts in order to make the religion socially meaningful. Taking this line, we could explain the ancient tradition of Sufism (Islamic mysticism) and the high status of the *syeds* in terms of the need for religious organization, leadership and hierarchy.

Sufism apparently flourished in the Indian context. By the thirteenth century, branches of the major Sufi orders had been established in India and a new order founded. Brelwi-type Islam, with its notions of religious hierarchy, which predominates in present-day Calcutta, has its roots in Indian Sufism. In Siddiqui's view, the Wahabis, a small sect in Calcutta, who dismiss Brelwi beliefs and practices as *bida't* (religious innovation) represent 'maximum conformity to the Islamic Great Tradition'. Siddiqui fails to point out that the Wahabi movement originated in central Arabia in the eighteenth century as a campaign against Sufi 'innovation', and began in India in the nineteenth century mainly as a reaction to western influences. While Siddiqui's personal view may be that Wahabi-type Islam, which predominates in central Arabia today, is the 'true' Islam, this should not have blinded him to the importance of the Sufi tradition in India.

MEERA MUKHERJEE, *Metalcraftsmen of India*, Calcutta: Anthropological Survey of India 1978. ix, 461pp., Illustrations. No price given.

The Anthropological Survey of India funded the initial research for this book between 1962 and 1964, though the results remained unpublished for a further fourteen years. Meera Mukherjee recorded metalworking in many regions of India, but she was prevented from including Assam, Gujarat and Maharashtra because of insufficient funds. The first two chapters, dealing with metalcraft castes and miscellaneous social and personal details, are organised little beyond the 'raw data' level of a field notebook. No reference is made to any of the Indian anthropological literature, a surprising omission considering the source of the fellowship, and the content is more anecdotal than ethnographic. Nevertheless, the author's training in sculpture is effectively exploited in the third and final chapter on metalworking techniques, where, despite an often eccentric use of English, the description of craft processes reflects keen observation and is presented with numerous incisive and charming illustrations. A skilled anthropological researcher could probably extract from the book many useful socio-cultural details, while the last chapter should assist the museum curator to identify and provenance Indian metalcraft tools and their products.

MICHAEL J. HITCHCOCK

MELFORD E. SPIRO, *Buddhism and Society: A Great Tradition and its Burmese Vicissitudes*, Berkeley etc.: University of California Press 1982. xvii, 477pp., Appendix, References, Index. £26.25.

The new eleven-page Preface to this book lets the reader know that the text has not been changed from that of the first edition, which was published in the U.K. in 1971.

In *Buddhism and Society* Professor Spiro is concerned with the relation between doctrinal Buddhism in a Theravāda form, and the religious beliefs and practices of the people of Burma. As an anthropological work it is a study of Buddhism transformed from a virtuoso to a mass religion, moreover a social form which he now regards as typifying an Asian rather than solely a Burmese Buddhism.

The primary focus is context rather than text (in the sense of Milton Singer). In the first instance Spiro is concerned with the ways in which '...social actors attempt to

negotiate their religious world in the same context in which they attempt to negotiate their economic world, or political world, or any other world ...' (p.xvii), that is, in a changing social context.

The book is in three parts: the first looks at the ideology of Buddhism, the second at ritual and the third at the monastic system. However, all three cover related issues, though from slightly varying standpoints. In the first part the important distinction between 'world-renouncing' or canonical Buddhism, and what might be termed the 'better-wordly' beliefs of the populace, is made. He also introduces other 'types' of Buddhism concerned with relief from trouble and for a better world here and now. This makes the work somewhat compendium-like in style.

There is a wealth of empirical material, organised around such traditional themes as calendrical, life-cycle and crisis rituals. There is also quantitative material: questionnaires have been used for topics such as peoples' ratings of different forms of action according to their meritorious consequences.

The work was originally criticised for being too western in approach, that is for not looking at the material primarily from the standpoint of Buddhist philosophy. It was also criticised from the other side, for not being an Asian equivalent to Weber's *The Protestant Ethic and The Rise of Capitalism*. These are rather fixed positions of criticism against ethnographic studies, and perhaps it would be wiser to concentrate on this work rather than on one which it is thought he should have written.

Away from the empirical level, the book is slightly amorphous. In part this comes from its breadth; in part it comes from an eclecticism of both method and theoretical approach; and in part it comes from a premature and over-elaborate typologising. One might have hoped for some clearer direction in a revised edition. But then that might have required further directed field-material: in any case, probably then it would have been a different book.

GRAHAM E. CLARKE

POLLY HILL, *Dry Grain Farming Families: Hausaland (Nigeria) and Karnataka (India) Compared*, Cambridge etc.: Cambridge University Press 1982. xvi, 296pp., Illustrations, References, Index. £9.95.

Polly Hill is a distinguished student of rural economies of West Africa, whose empirical studies are exemplary paradigms of 'indigenous economics'. Her method encompasses detailed field studies through interpreted interviews, sensible quantification

(especially of directly recorded land-holding surveys), key informant information, and archival research. She is best known for her studies of cocoa production in Ghana, and of Hausaland in the semi-arid north of Nigeria. By accident she came to do research in six villages in semi-arid south India, and the experience is reported here as a comparative study with her Hausaland material.

The book starts with a strongly-argued case for 'field experience', and its relevance to both economic theory, and development policy and practitioners. In particular it should undermine erroneous stereotypes based on inappropriate theories and inaccurate observations of homogenous peasantries and farming systems. Hill provides, for both areas, background material; some empirical details of farm holdings (ridiculing much official Indian statistics), household organization, other economic activities, agricultural intensification, household mobility, migration, rural-urban and employer-employee relationships. There are contentious discussions on a number of topics, including methodology, (economic) inequality, stagnation and crisis in the countryside, poverty, agrarian hierarchy, and, the main focus of the book, a 'dry grain mode of production'. This she explicitly differentiates from a historical materialist concept.

Polly Hill is at her best with empirical material; she is strong on the deficiencies of official statistics and data production methods; her detailed discussion of cases based, as she makes clear, on observation, interview, and data collected by only reliable methods, are a direct confrontation of the evasions of other economic techniques of data production and interpretation. Unfortunately she does not deal with the latest formalists - the 'new institutional economics' - as opposed to the (somewhat vaguely caricatured) old formalists, substantivists, and Marxians, or with the statistical methods they employ on data she provides good grounds to suppose to be unreliable.

The major problem with her work (on Hausaland, and, I infer from its absence, the south Indian work) is the neglect or misrepresentation of local elites, agrarian merchants, the administrative bureaucracy, and the State. Surpluses were and are extracted from rural Hausaland in the forms of commodities, formerly slave and corvee labour, wage and migrant labour, tax, tribute, interest and in places rent. Present unequal access to the resources of the State are an extension of previous unequal relationships. Nevertheless her empirical material is sufficiently rich, informative, and consistent for interpretations based on alternative points of view; it is not highly summarised and abbreviated in the manner of more quantitative, but ironically less statistically valid work.

The arguments with which Hill takes issue, and on which she is usually interesting, are often rather loose caricatures of vaguely attributable positions. This is perhaps connected with her neglect of much recent related work. Hence while she avoids

outright injustice, except by innuendo or neglect, she is an inadequate guide to literature and debate. She seems to prefer shadow bouts with surrogates to direct engagement with work pertinent to her own. Possibly as a consequence her own positions remain sadly vague or inadequately established.

For example her views on economic inequality are very close at times to the 'cyclical kulakism' or multi-directional mobility of Shanin and others, emphasising the personal characteristics of the rich and the dispersal of wealth among more numerous inheritors as well as the obligations of the successful. She argues that '(R)ural inequality does not derive from the outside world though it is often enhanced by it', leaving one in serious doubt as to whether she is arguing that the changes wrought by imperialism, colonialism, and subsequently have changed the nature of inequality and poverty or not. Her mode of production remains curiously a-historical, and as much of the description shifts from past to present it is not clear whether it existed over the whole period, or only recently, and whether all these periods are characterised by economic stagnation.

However, Hill's West African work is essential material for specialists, and those interested in more general questions, and I suspect that despite lacunae her South Indian work will reward detailed attention. It is perhaps enough to be such an assiduous researcher and proactive skilful writer; nevertheless one hopes for a more satisfactory engagement with the literature.

R.W. PALMER-JONES

LEON SWARTZBERG, Jr., *The North Indian Peasant Goes to Market*, Delhi etc.: Motilal Banarisdass 1979. xiii, 156pp., Map, Bibliography, Index. Rs. 50.

The fieldwork behind this volume stems from a project at Columbia University in the early 1960s 'to conduct research in India on the adaptation of long-established agricultural communities to changes in the national economy, and the effects of these changes on traditional cultures'. Swartzberg's contribution to this project was to do research in a single-caste village near Patna, Bihar, during the period January 1964 to June 1965. Chapter 3 of this volume, 'The Roys: A History of an Indian Entrepreneurial Family' first appeared as the author's Master's thesis at Columbia in 1967. He was awarded his doctorate for the complete work in 1969 and it was published by Motilal Banarisdass 10 years later 'in order to make its findings more available to more people'. There is no evidence of any attempt at textual revision during the 10-year interval

between its submission as a thesis and its publication in book form; the most recent entries in the bibliography date from 1968.

Swartzberg, heavily influenced by Neale, pursues the thesis that the Indian village contains two economies - 'market' and 'household' - in a not overwhelmingly innovative re-hash of the old 'traditional' versus 'modern' debate in peasant studies. Theoretically the book is weak and parochial. Even in the mid-1960s the field of economic anthropology was not confined to Malinowski and Polanyi and in any publication from 1979 I would have expected that the authors would have taken cognizance of the strides that had taken place. At the very least I would have expected the author to address himself to the issues of his day. Thus I was surprised that he showed no awareness of Skinner's important expansion of central-place theory into the study of Asian anthropology (which took up three issues of the *Journal of Asian Studies* in 1964-1965) or of the massive literature from India on what they designate as 'Rurban' relations, both of which are germane to the work at hand. The book was, then, something of a disappointment.

This is not to say, however, that it is entirely without utility. While not living up to the promise of its title, Swartzberg makes interesting contributions in unexpected areas. Thus I was pleasantly surprised by his diachronic approach and his command of historical documents which anticipated developments in anthropological methodology only now coming to fruition. Also unexpected in a book of this title were his observations on the relationship between affinity and labour migration. While the neologisms he produced to describe the movement of excess male labour to sister's husband's village lack felicity (e.g. gyno-consanguine-local), the relations that he has discovered would seem to indicate a profitable new area of study for Indologists. Finally, as the analysis of a single-caste village, this book adds to the range of village studies available to us.

On a technical level the publishers deserve commendation. This excellent edition is without the niggling typographical errors which so frequently bedevil South Asian publications. With the exception of the cursory index, the editorial standards are high with only minor discrepancies of presentation intruding on the text.

STEVEN SEIDENBERG

## BOOK REVIEWS

NADIA ABU ZAHRA, *Sidi Ameer: A Tunisian Village*, London: Ithaca Press 1982 [St. Antony's College Middle East Monographs no.15]. xvii, 204pp., Appendixes, Select Bibliography, Index. fl.50.

This is a skilled and sensitive study of a village in the Tunisian Sahel, which makes a major contribution to our understanding of Tunisian politics and society and evokes speculative thought of comparisons throughout the Arab world. One can only regret that it has been necessary to wait so long for the fruits of research conducted from 1965-68 to be publicly available and wish that the author were in a position to provide a much more extended review of changes over the past fifteen years.

The subject of the study is the village of Sidi Ameer, an inland settlement between Monastir and Sousse in an area of dry-farming dominated by olive trees which serve both as crop-producers and as savings banks, being bought and sold according to the ebb and flow of personal fortunes as well as having a degree of sanctity attached to them. Monastir was the birthplace of President Bourguiba and the region around Sidi Ameer has produced the political élite of independent Tunisia.

According to tradition, Sidi Ameer was the first man to settle where the village now stands, taking his place and drawing a ring around him with his rosary. He lived, as far as can be established, at the end of the Hafsid and into the Muradid dynasties at a time when political and economic circumstances throughout North Africa were propitious to belief in *walis*. Tradition attaches to him the attributes of strength and *baraka* common to all *walis*. Both the *zawiya* and the village of Sidi Ameer were constructed at the same time and the *zawiya* became a religious centre for the whole of the Sahel, and, with diminished force, remains so at the present time (even when the cultural committee of the Socialist Destour Party has taken over the organisation of the annual festival).

The village, characteristically, is defined in descent and space - the descendants of Sidi Ameer living in the *Zawiya* quarter, which is almost entirely exclusive to them, socially and geographically distinct from the four *'arshs* of the Ramada quarter. Before independence and the dissolution of the *habus* which followed, the descendants of Sidi Ameer did not need to work, being made financially independent by the *habus* dues, and the *Zawiya* women were veiled and secluded, as befitted those of higher social status who did not need to work in the fields.

The core of Dr Abu Zahra's anthropological study is in her account of the *'arsh* (patrilineal descent group) and the *dar*, which refers to the extended family (or in the first instance to

the elementary family of husband and wife) as well as to the house. Where *dar* changes into *'arsh* is not always clear, and social duties in the two groups differ only in the greater degree of obligation in the *dar*. By describing, with acute observation and proper scholarly discipline, the relationships and customs amongst both the Zawiya and the Ramada people, Dr Zahra provides a vivid account of the life of the village which is indispensable reading for anyone interested in the Maghreb. She depicts a demanding society, one where everything is known and where the main motives for conduct are self-interest and the pursuit of prestige, held together by rigorously defined obligations for the exchange of visits and gifts, softened by the *hadra* every Thursday at the *zawiya* and enlivened by the festivities that surround marriage (although obligatory gift exchanges in the form of money collections form part of the celebration). Spontaneity and indulgence do not figure large (although today, a bride who was found not to be a virgin would be kept by her husband if he was a 'good man' and her parents would return part of the marriage payment secretly). But the community which results is held together with strong bonds; in Dr Abu Zahra's succinct words, 'people compete with each other for social prestige, and this reinforces and emphasizes the common values according to which this prestige is assessed'.

The final chapters are both fascinating and tantalizing, since they are concerned with the impact of the modern state - tantalizing because they date from the period of Ahmed ben Salah's ill-fated reforms, largely abandoned in 1969, and because they describe processes then beginning which have perhaps continued and perhaps been reversed in subsequent years. Economic development has changed the structure of the village; not only have the Zawiya men lost the *habus* revenues but the Ramada men have been able to take advantage of new educational and employment opportunities (in, for example, the hotel industry). On the other hand, whereas the head of the Neo-Destour party branch before Independence was from the Ramada faction, at the time of writing the head of the government Socialist Destour Party was from the Zawiya faction and from the same *'arsh* as the sheikh of the village. Not surprisingly the possible foundation of a branch of the National Union of Tunisian Women in the village was not enthusiastically welcomed even by the most modern-minded men (who regretted that in their village there were no women sufficiently qualified), but the head of the Party branch recognised the wave of the future and was calculating how to ensure that his brother's daughter would be head of the branch when its foundation could no longer be resisted.

How strong the waves of modernisation have been and how resistant or resilient tradition has proved requires further detailed research. In the meantime the publication of Dr Abu Zahra's work provides invaluable insights into a specific community and has important implications for the study of any traditional society under the impact of modernisation.

J. VAN BAAL, *Jan Verschuieren's Description of Yéi-nan Culture*, The Hague: Martinus Nijhoff 1982 [Verhandelingen van het Koninklijk Instituut voor Taal-, Land- en Volkenkunde 99]. xiii, 105pp., Illustrations, Bibliography. No price given.

J.H.M.C. BOELAARS, *Head-hunters About Themselves: An Ethnographic Report from Irian Jaya, Indonesia*, The Hague: Martinus Nijhoff 1981 [Verhandelingen van het Koninklijk Instituut voor Taal-, Land- en Volkenkunde 92]. xv, 285pp., Appendixes, Illustrations, Maps. Dfl. 90.00.

It is always a pleasure and something of a relief to review books which one can recommend to other people as being books which are worth reading, as much by theorists (who cannot, of course, work without social facts) as by those who want to find out what their fellow men in other parts of the world, and in this case in such a part of it as Irian Jaya, are about. This is one such occasion.

*Jan Verschuieren's Description of Yéi-nan Culture* consists of ten chapters, nine of which deal with such stock anthropological topics as 'Territorial, Clan and Moiety Organization', 'Kinship, Marriage and Conjugal Life', 'Initiation' and such like. Chapter 10 consists of an Epilogue by Professor Jan van Baal who has put together this book from papers left by Father Verschuieren when he died. They were in chaos, and were sorted out by Father Hoeboer, a friend and colleague of Verschuieren's, who also typed out those which he concluded might be of ethnographical interest and let Professor van Baal have a copy. This book is in the nature of a repayment of the debt which van Baal felt to Verschuieren for his friendship and for the contribution which he made (in letters) to *Dema*, van Baal's impressive ethnography of Marind-anim life.

Verschuieren was a missionary of the Sacred Heart, and this was the congregation, also, to which Boelaars was attached and through which he worked from 1951 to 1968 in the southern lowlands of Irian Jaya. *Head-hunters About Themselves* is about the Jaqaj (pronounced Ya'hry) which means 'human being'. Boelaars' book is more substantial than Verschuieren's: it runs to 273 pages of text, and contains twenty fine plates which complement the text well, five informative maps, and four appendixes, one of which is a glossary, which is very useful. No glossary accompanies Verschuieren's ethnography which is a pity for it is thereby much harder to read than it would otherwise be. Both books appear in the *Verhandelingen* of the Koninklijk Instituut in Leiden and, as usual, they are excellently produced and reasonably priced.

There is not the space to go into any of the issues which the data contained in these books raise, but one point can be made: this is that in the commentaries to Verschuieren's text, some of the questions which Professor van Baal raises have an oddly old-fashioned ring. Thus even though a scholar of his

eminence will obviously be aware of the arguments (if they can be called that) of *Le Totémisme aujourd'hui*, there is still, in Chapter 4 of Verschueren, a discussion of how far Yéi-nan society is totemic. 'Totemic', however, is not an epithet which is usefully applied to societies, so that a discussion of whether or not a society is this or that much totemic tends not to increase our understanding of the society in question (and of other societies which might serve as comparative examples) very much. The world-wide use of, for instance, animals and to a lesser degree plants in symbolic classification seems to be an imaginative device to which all men tend, but the uses to which the device is put are socially very different. No significant correlations, moreover, would seem to attach to the uses to which the device is put. It is, indeed, an empty anthropological construct.

A slightly odd note is also struck in some of van Baal's Epilogue to Verschueren. He writes, for example, that 'the great significance given to this symbolism [sc. the sexual symbolism inherent in the headhunting ritual] in Yéi-nan culture as a whole . . . confirm[s] the theory that, fundamentally, the males suffer from strong feelings of inferiority vis-à-vis their females and feel they must compensate their inferiority by acts of violence.' It is to be supposed that few would find this a satisfactory view nor would it, of course, be possible to decide one way or another, if only because it is a proposition for which it would be difficult to conceive evidence in its favour or to its detriment.

Boelaars' book is the more complete (understandably, as it was written as a monograph) and although Verschueren, through van Baal, provides an interesting account about what he intended to be a description of the original way of life ('culture') of the Yéi-nan, Boelaars' book is the fuller, and thus the better, book. It also relies heavily upon indigenous accounts (as the sub-title says) and it is indeed 'amazing that the author has managed to collect such widely varied information on numerous topics which, from a native point of view, it must have been highly unattractive to discuss.' Boelaars has let people talk about themselves, and their accounts make fascinating reading - and also make a good case for the methodological premise of all the best social anthropology that one should find out what the people whose life is being described do and what they say about what they do, in the first instance at least. This is not only worthwhile in itself, it also leads to the possibility of real comparisons being made, i.e., things which we have not put together on the basis of faulty reasoning or of old habits of mind but which we put together because of the intrinsic natures of the social facts under consideration being compared, and it prevents the construction, complementarily, of such false categories as totemism.

One can always find fault with the work of others if one wants to and van Baal raises points of criticism both about Verschueren's book and about Boelaars' but it would be churlish

to bring these up and to point to other possible drawbacks in these accounts when the value of the books far outweighs anything of the kind. Both are important contributions to the indispensable stuff of social anthropology - ethnographical description - and it is to be hoped (as van Baal hopes) that they will stimulate others to work in this area where 'cultural diversity has assumed such extreme proportions' and which renders this 'paradisiacal country', in this regard, second to none in the world. As van Baal says, once again we have to thank the Institute for publishing these fine books which continue the long and distinguished tradition of ethnography written by intelligent and sensitive missionaries. Social anthropologists have much to thank such men for, even though most, like this reviewer, can be supposed to be out of sympathy with their aims at whatever level of liberality the way of achieving those aims may be pitched - though it could never be said that men like Verschuieren and Boelaars are at all the same as the imbecile portrayed in *The Sins of Rachel Cade* or as, for instance, the American fundamentalists in Venezuela among the Auca, whose aims are altogether more sinister and whose activities should be stopped immediately.

ANDREW DUFF-COOPER

PETER C. REYNOLDS, *On the Evolution of Human Behavior: The Argument from Animals to Man*, Berkeley etc.: University of California Press 1981. xi, 259pp., References, Index, Illustrations. £20.00.

P.C. Reynolds' work is an argument on the grandest possible scale that seeks to demonstrate that human society, including the social traits that were thought to be purely the stuff of humanity, is the end result of instinctual behaviour that has evolved alongside and integrated with learned behaviour. This thesis, all 272 pages including extensive notes and references, is couched in exceedingly concise terms that, at first reading, may offend the sensitive reader as gratuitous jargon but, when armed with the supplied glossary, the elegance of the language and argument becomes apparent.

Although written in a scholarly and detailed manner, Reynolds has thoughtfully avoided alienating the general reader by providing two chapters that give a useful (albeit occasionally slippery) foothold on the field of ethology. The first chapter is a useful historical review of eighteenth- and nineteenth-century thought on the structure and derivation of human societies and behaviour, while the second chapter gives a critique of current theories and lays the foundation for the ensuing argument.

Chapter Three looks at inter-individual relationships in man and primates and attempts to elucidate the former through the

identification of homologous behaviour and/or patterns of behaviour found among the latter. The result of this exercise in speculative though carefully constructed analogy is that social attachments in man can be explained as 'stabilized approach-avoidance conflicts' that have their basis in the instincts of aggression and fear and exhibited as antagonism. Chapter Four demonstrates quite forcibly the relationship between human conceptual thought and primate cognition. Reynolds argues that man's ability to modify external objects to a predetermined form as seen by the mind's eye is merely the substitution of this new level of instrumentality into the existing cognitive ability of apes to manipulate their bodies in an iconic manner. The difference in conceptualization among man and other primates, then, is more of a matter of scale and development than a difference in kind.

In Chapter Five Reynolds argues that object exchange networks in man are similar to primate grooming behaviour and that the former can be viewed as an outgrowth of the latter by substituting material objects as the medium of social exchange to produce the material exchange systems found in human society. In Chapter Six Reynolds proposes, in a rather complicated and convoluted fashion, that language is a new 'evolutionary emergent' but that this emergence was due to a reorganization of existing primate abilities rather than the development of newly-acquired capabilities. Chapter Seven is an essay attempting to apply the conclusions reached in earlier chapters to what is known archaeologically about early hominids and to identify the hominid behavioural traits that reflect continuity or discontinuity with those of other primates. Reynolds reaches the ultimate conclusion that learning does not replace instinct but rather instinctive patterning is needed for the acquisition and maintenance of human culture, and that human behavioural and/or cultural patterning can be seen as homologous with 'the simplest instrumental acts' of monkeys.

*On the Evolution of Human Behaviour* is a thought-provoking volume that challenges many a long-held belief regarding our sacrosanct uniqueness. Due to the nature of the available evidence, the strength of the thesis is dependent on the elegance of the logic, and elegant it certainly is. Both newcomers to the field of ethology and experienced ethologists will find Reynolds' book extremely useful and a source of controversy that will take years to thrash out.

JOHN DUMONT

SALLY HUMPHREYS, *The Family, Women and Death: Comparative Studies*, London etc.: Routledge & Kegan Paul 1983. xiv, 176pp., Bibliography, Indexes. £15.00.

Sally Humphreys has now established herself as one of the most original English-language writers on ancient history. Much of her inspiration (as the title of her previous collection of essays, *Anthropology and the Greeks* [1978] proclaims) derives from a long involvement with social anthropology. In fact the credit paid to anthropology may be more flattering than is strictly warranted, for all Mrs Humphreys' essays are properly historical pieces whose worth resides in their classical scholarship. If they are also 'anthropological', this is perhaps as much to do with a general revival of interest within ancient history itself in the exploration of Greek civilization as an alien culture with its own particular complex of values and institutions, as with any set of theories, methods or 'discoveries' about society which anthropology might be deemed to contribute.

The present set of essays might well have been entitled *Public and Private in Ancient Greece*, for it is the inter-relationship (by no means a static one) between the private world of the citizen and the public domain of the *polis*, explored in the context of family, of women, and of death, which gives this collection its unity. What was the relationship between the *oikos*, the 'household', and the *polis*, 'the state', in classical Athens, both in terms of social structure, and, importantly, in terms of the values which appear to have accrued to each and on whose conflict so much of Athenian tragedy dwelt? What was the relationship, at the political level, between private interests and loyalties and those of the community? How were women, paradigms of the private world, contained and accounted for in the public realm? What, indeed, was the role of family and of kinship in ancient Athens, and how best can such a question be approached? And of death, in one sense that most private of states, how was it publically celebrated? What were the connections maintained between the deceased and the community? With what community were they maintained, the family or the state, and in honouring the deceased, what was honoured - the individual or his place in society, the living or the dead?

These are the sorts of questions with which Mrs Humphreys deals, and they are thematic. That said, however, it must be admitted that many of the essays are very much occasional pieces. All have been published or are about to be published elsewhere. Two were actually written as Introductions to volumes of collected essays edited by Mrs Humphreys. All are relatively short. With the notable exception of 'Family Tombs and Tomb-Cult in Ancient Athens', which is a substantive piece of original research on an outstanding topic, the essays tend to provide a synthetic account of the problems tackled and to propose and suggest the manner in which they might be investigated, rather

than bringing them to a final conclusion. But they are none the less valuable for that, and it may be that these essays will provide the stimulus for a new and important programme of research in the ancient world.

ROGER JUST

PHILIP MAYER (ed.), *Black Villagers in an Industrial Society: Anthropological Perspectives on Labour Migration in South Africa*, Cape Town: Oxford University Press 1980. xiii, 340pp., Bibliography, Tables, Index. £13.95.

The problem of labour migration in southern Africa has always been crucial because of the changes and disruptive consequences it has brought about in village life. In one way or another anthropologists have paid attention to this problem, although their focus has tended to concentrate on those people who went out to work and little attention has been paid to the people left behind. This book is meant to redress the balance.

*Black Villagers in an Industrial Society* is concerned with Blacks living in villages where the influence of industrialization ineluctably exercises its impact. It is the result of intensive fieldwork done between 1976 and 1979 by a team of anthropologists led by Professor Philip Mayer who is known for his devotion to the study of social change. Here the authors are trying to portray, from different theoretical approaches, the impact exercised by labour migration on different aspects of the villagers' lives in Lesotho, Ciskei and Transkei.

In the first chapter Professor Mayer portrays the historical background of the present struggle between Black and White. The implementation of the policy of apartheid and the demand for labour stimulated labour migration and divided Blacks into those who went to work and those who stayed at home. Labour migration also resulted in contact with different ways of life. Too much contact with the outside world, however, meant inevitable disruption within the villages. Throughout the chapter it is shown that belief in ancestors played an important role in the resistance to this disruption.

The role played by this belief in ancestors comes to the fore in MacAllister's chapter on 'Work, Homestead and the *Shades*'. Here we are told that the Gcaleka, in order to cope with the ineluctable disruption caused by labour migration, interpret it as a rite of passage: the ancestors are told of the departure of their child to go to work and are asked to protect him. The boy is strongly advised not to misbehave and not to waste his earnings since they belong to the ancestors. On his return the ancestors are ritually told of the event. Girls also see labour migration as a rite of passage: they will encourage

their boyfriends to go, and are reluctant to marry one who has not been through the process.

The impact of labour migration on marriage and family life is dealt with by Manona. His research shows that as a result of the long absences at work and the brief visits on leave, elopement has become the most frequent form of marriage, and that infidelity and divorce have become more common.

O'Connell, in his chapter on 'Xesibe Reds, Rascals and Gentlemen at Home and at Work', shows the complex interaction between the classes among the Xesibe of Transkei in relation to the problem of migratory labour. He establishes that among Xesibe villagers there is a conflict in integrating migration prestige and class, since the traditional criteria of status no longer apply.

McNamara studies the migrants during their stay in hostels. He shows that ethnicity plays an important role in forming groups for socialisation purposes, and in the possibility of promotion.

The merit of this book lies in the fact that a team of anthropologists has come together to examine closely the effects of migratory labour on Black villagers' lives. The conclusion is that the effects are disruptive. Although it was not the authors' intention to provide any solution to the problem, they nevertheless set forth premises which governments could take into account. Because it is the first attempt to look closely at 'those left behind', *Black Villagers in an Industrial Society* may well be considered a pioneering work, one that would strongly encourage further research extended to the whole of southern Africa.

E. P. GWEMBE

BEN WHITAKER (ed.), *Teaching about Prejudice*, Minority Rights Group Report No.59, July 1983. 24pp., Appendix, Bibliography. £1.20.

This Report was instigated by the Conference on Teaching about Prejudice and Stereotyping held at Cumberland Lodge in 1983 and organised by the Royal Anthropological Institute in conjunction with the Minority Rights Group. It consists of two papers from the conference, a summary of the conference and further contributions commissioned afterwards. The Report's composition gives it a somewhat lopsided feel as the contributions are of differing length, the conference-based material being more expansive than the retrospective works.

Michael Banton and William Stubbs were the major contributors to the conference and their papers are reproduced in full. Banton explores the philosophies behind the various educational programmes which have been introduced in Britain to overcome racial prejudice and assesses the aspirations in this field since the 1950s. He

finds wanting the terminology used to describe such educational programmes - a terminology which reflects the ideological standpoint of its proponents - and points out the resultant difficulties with the programmes themselves. He suggests that less emphasis should be placed upon trying to formulate a positive approach which he feels is impossible to do in what he describes as a transitional phase. Rather he favours an approach which clearly sets out the negative features which we should seek to avoid. 'We can agree on the need to diminish error and combat prejudice.'

William Stubbs, the Chief Education Officer for the Inner London Education Authority, presents two examples of how material on prejudice is being introduced into schools in his area. He emphasises that children should be exposed to de-stereotyping policies at an early age and that what Jonathan Benthall, in his summary of the conference, describes as 'whole school policies' should be adopted. This means that incumbents of positions throughout the school hierarchy 'from cleaner to head teacher' should be involved in the task of combatting racism.

Jonathan Benthall notes that there was no great discussion at the conference as to whether 'race' constitutes a special category of difference, more divisive than others, or whether it is just one of a number of different forms that can generate prejudice. For me this is a crucial question and the Report suffers somewhat from not having dwelt upon its consideration. What is clear from the papers assembled is that racial prejudice and stereotyping are the major concerns of the contributors.

Nadine Peppard details recent developments in race relations training and calls for greater professionalisation and sophistication in this field of endeavour. David Hicks emphasises the enduring parochial nature of the British population as a whole and highlights the enormity of the task that is needed if ingrained attitudes are to be changed. Anthony Rampton emphasises the general view of the contributors that teachers can do a great deal to help provide for the educational needs and promote the educational achievement of children from ethnic minority groups. He advocates the introduction of more Local Education Authority advisers on teaching ethnic minorities not only in areas of great concentration of population but also in areas where there are few members of ethnic minorities.

Alan Little dissents from the general view that the curriculum and the material taught is of greatest importance in controlling racist attitudes and beliefs. He sees the presence of more teachers from ethnic minorities as doing more to counteract racialism and create the conditions of equal opportunity than explicit campaigns to tackle prejudice head on. However, Trevor Hall points out that prejudice is not the monopoly of white people and Jonathan Benthall takes Little to task, pointing out that many Asian teachers are anxious about 'black' prejudice against Asians. More senior 'black' teachers would not, he feels, necessarily improve ethnic relations in a multi-ethnic school.

Tessa Blackstone's article is the only contribution to deal specifically with prejudice and stereotyping of a non-racist nature. She discusses the recent concern about equality of opportunity for

both girls and boys in the education system. She feels that gender stereotypes need to be challenged at an early age if the clear trend towards under-achievement for girls towards the end of their secondary education is to be reversed. The gender association of certain subjects can then be broken down in secondary school, career guidance can broaden the horizons of pupils beyond the traditional limits, and parents can be encouraged to consider a wider range of opportunities for their children.

The prevailing message of the Report is expressed by Ben Whitaker in his Introduction where he exhorts the reader not to regard prejudice as something which other people possess and to recognise that negative stereotyping is operative not only in the more talked-about areas of race and sex. He points out that a start has been made in educating the young against racial prejudice but he feels that similar education among adults is long overdue. Such education should not be limited to those groups in society apparently most affected by any particular type of prejudice or stereotyping. All groups in society should be exposed to such ideas. Anthropologists should not regard themselves above such self-examination and enlightenment, for as Banton points out, many of the misconceptions about racial types which endure have found legitimation in the work of our anthropological forbears.

P. L. HARDING

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THE STRUCTURALISM OF LÉVI-STRAUSS:  
PROBLEMS AND PROSPECTS

*Introduction*

Lévi-Strauss's structuralism is characterized by a certain ambiguity. On the one hand a growing number of anthropologists has come to acknowledge the merits of this structuralism: old data could be analysed in new contexts which often made it possible to relate phenomena in unexpected ways. On the other hand the limitations of this approach have also become increasingly clear. Structuralism now faces the task of transcending these shortcomings. As these failings are not primarily due to careless practice but to the hard core of its basic assumptions, this deadlock can be broken only to the extent that structuralists are willing to change cherished ideas.

In this essay I shall make a few suggestions for a possible way out of this impasse. To prepare the ground I shall first give an outline of the basic presuppositions of Lévi-Strauss's structuralism. But at the same time the limitations of his approach also come into view, which means that certain principles to which Lévi-Strauss is committed will have to be questioned. Here I shall focus on the dangers of his reductionism and on the problems of the relation between structure and process. Finally I shall indicate a few broadly-conceived directives for future research.

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I would like to thank Jan de Wolf for translating this article into English.

*The General Framework of Structuralism*

Lévi-Strauss has not only opened up new avenues which changed our perspective on specific areas of anthropological knowledge, but he has also laid the foundation for a new idea of what cultural anthropology could mean as a science through the elaboration of a certain combination of basic presuppositions about the nature of socio-cultural reality.<sup>1</sup> Ardener (1971:450) goes so far as to call this change an epistemological break with the anthropological past. The most important interdependent principles are: (1) naturalism; (2) the difference and the asymmetry between conscious and unconscious as well as between appearance and reality; (3) holism related to relationism; and (4) the priority accorded to synchrony as compared to diachrony.

Central to Lévi-Strauss's thought is his idea that man must be interpreted as a part of nature, i.e. in terms of biological and physical factors, because these determine man. This determination occurs by way of the unconscious functioning of human thought. The principles of human thought are rooted in his nature, in the physio-chemical conditions of the brain. Therefore the creation of culture is no more than the realisation of a potential system which already exists in nature. It is precisely because the human mind obeys the laws of nature that exchange - the universal source of social life - and attribution of meaning become possible. So through this tie with nature, symbolism, exchange and signification belong indissolubly and permanently together (see Simonis 1974:374).

Related to this naturalism is Lévi-Strauss's idea that reality is layered. Although he sometimes gives the impression that there are various levels (Lévi-Strauss 1973:30), he mostly recognizes two strata, variously designated as the distinction between experience and reality, appearance (empirical reality) and reality (real reality), meaning and grammar, conscious and unconscious. Although Lévi-Strauss fails to indicate the exact

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<sup>1</sup> With regard to kinship one can point to the role of 'exchange', which results from an inner need to reciprocate, and to the importance of the sibling relationship between brother and sister. Lévi-Strauss has contributed in an important way to the theory of systems of classification through his demonstration that categorizations which at first sight appear to be irrational are characterized by an internal logic of their own. In his studies of myth he has made it clear that myths are pre-eminently a medium for reflecting on the conditions of existence and for giving a meaning to it. He has also demonstrated that this attribution of meaning is characterized by a universal, basic pattern.

relationship between these levels of reality - in particular the relative autonomy of these levels *vis-à-vis* each other remains vague - so much is clear that the level of unconscious reality in some way directs conscious reality. Thus according to Lévi-Strauss one should turn away from the 'evidence' of consciousness and the certainties of experience in order to reach that 'reality' which science is seeking. Conscious meaning can always be reduced: the discovery of meaning is secondary. The most essential scientific activity consists of the discovery of the mechanisms of thought (Lévi-Strauss 1958:75).

If ... unconscious activity of the mind consists in imposing forms upon content, and if these forms are fundamentally the same for all minds - ancient and modern, primitive and civilized (as the study of the symbolic function, expressed in language, so strikingly indicates) - it is necessary and sufficient to grasp the unconscious structure underlying each institution and each custom, in order to obtain a principle valid for other institutions and other customs, provided of course that the analysis is carried far enough (Lévi-Strauss 1958:28).

So the assumption that reality can be known by the senses is mistaken. Reality never appears on the surface; it remains hidden beneath empirical data and directs the latter. This unconsciously active structure of thought - the sum total of mechanisms and conditions which determines the activity of consciousness - can be discovered according to Lévi-Strauss by means of a comparative analysis of institutions and ideas. In other words, Lévi-Strauss's method consists of the analysis of cultural products, with the revelation of processes of thought as its aim. Yet one should proceed carefully, as these cultural products do not represent processes of thought in their pure form, because they are the result of a process of interaction between socio-cultural reality and unconscious principles of categorizing thought. Thus, although structures of thought are universal, they nevertheless continually produce other structures, in the sense of orderings.

Through this attempt to mediate between what might be called reason and senses - or rationalism and empiricism (Lévi-Strauss 1955:50; 1971:618) - order becomes the central problem of structuralism. In this context emphasis is put less on the recording of individual cultures or their constituent parts than on the determination of the limitations which the brain imposes on the experience of the senses. This emphasis is closely connected with Lévi-Strauss's rejection of Durkheim's thesis that cultural classifications are the outcome of social conditions. In structuralism there is no place for sociological determinism: after all, socio-cultural orderings are the result of the activity of unassailable laws of thought. More specifically, Lévi-Strauss is of the opinion that the brain functions by means

of a mechanism of opposition and correlation, also designated as the principle of reciprocity and bipolarity, or as the unconscious; that is,

... the system of postulates and axioms required to establish a code which allows the least unfaithful translation possible of the other's into 'ours' and *vice versa*, the set of conditions in which we can best understand ourselves.... Ultimately, *pensée sauvage* as I intend it is *me* putting myself in *their* place and by *them* being put by me in *my* place (Lévi-Strauss 1970:62).

Thus this universal *pensée sauvage* forms the foundation of cross-cultural comparison.

His belief in the structuring activity of the human mind and the structuredness of reality clearly leads Lévi-Strauss to maintain that phenomena present themselves as structures. This view represents an atomistic or 'Aristotelian' concept of science, which sees reality as an aggregate of separate entities and which pays insufficient attention to the mutual relationships between phenomena. Lévi-Strauss bases himself on a 'Gestalt perspective' or 'Galilean' outlook (Lévi-Strauss 1958: 332). Two inter-related principles play an important role in this Gestalt perspective: (1) the sum total is assigned priority over its constituent part: 'the unity of the whole is more "essential" than each of its parts' (Lévi-Strauss 1950:xxxviii; see also 1973:14); and (2) the manner of explanation must be relational and integrative. This latter view has been elevated to a most important methodological rule. Relations are the only object of analysis for science (Lévi-Strauss 1968:175). Phenomena must be explained on the basis of their inter-relationships. It is the relationships which link phenomena to each other that are basic, not the phenomena themselves (Lévi-Strauss 1949a:196).

The methodological principle that relations are more important than elements causes Lévi-Strauss to emphasize synchrony. In his view, it is above all the relations which exist at a given point in time which determine the meaning of an element. He finds support for this idea in Saussure's thesis that 'facts in the synchronic order are structured and facts in the diachronic order are disconnected events' (cited in Ricoeur 1970:62). Taken together, this implies that Lévi-Strauss sees the relationship between structure - in the sense of ordering - and event (*événement*) as asymmetric. Change, events and history are all subordinated to structure. Structure - as preceding reality - makes some sense of events, which thus become integrated in this way into structure, or into a pre-existing order. Changes in orderings are caused by external factors such as raids, wars, migrations, demographic fluctuations and adaptations to new environments.

In view of the core assumptions outlined in the preceding part it

should cause no surprise that Lévi-Strauss relates cultural anthropology especially to linguistics, psychology and history.

As language is pre-eminently the medium which can reveal the activities of the brain, linguistics is of exemplary significance for the structuralism of Lévi-Strauss. Lévi-Strauss believes the linguistic system to be the prototype of various socio-cultural institutions. He views cultural anthropology, like linguistics, as a branch of semiology, the science of sign systems which studies 'the life of signs in the heart of social life' (Lévi-Strauss 1973:18). Culture is a system of codes through which man exchanges messages. All human forms of socio-cultural activity - be it kinship systems, systems of classifications, myths, culinary habits, or fashion - are in a formal sense 'codes' or systems of symbols. Objects and techniques are always connected with ideas, and therefore with signs. Apart from their context they appear simply as 'facts'. However, if the anthropologist placed them in that general inventory of societies which anthropology is trying to construct, they look quite different, and represent the choices which each society has made when faced with a number of different possibilities. 'Consequently, then, even the simplest techniques of any primitive society have hidden in them the character of a system, analyzable in terms of a more general system' (Lévi-Strauss 1973:20). Society is one big machine generating communication on different levels (Lévi-Strauss as cited in Tax 1953:323; see also Scholte 1966:1196). On this basis one may expect homologies between these socio-cultural regularities and the rules which govern language. Moreover one should not forget that all these codes are expressions and products of the human mind. The activity of the human mind, according to Lévi-Strauss, obeys the same laws in all fields. Thus everywhere the same basic pattern can be discovered.

As the explanation of phenomena consists of the indication of their logical order, anthropology is pre-eminently the study of 'anthropo-logics' (Lévi-Strauss 1962c:137). These anthropo-logics in their turn function within a 'psycho-logic' (Lévi-Strauss 1962b:75). The brain possesses a universal structure, but it processes material which differs in accordance with the technological environment and the historical era (Lévi-Strauss 1972:76). With regard to the basic explanation of its research objects, cultural anthropology can be related to cognition-psychology and ultimately perhaps to neuro-physiology.

On various occasions Lévi-Strauss has talked about the relation between cultural anthropology and the science of history (1949b, 1952, 1960, 1962b, 1964b). According to him these two disciplines have a special relationship with each other: 'the one unfolds the spectrum of human societies in time, the other in space' (Lévi-Strauss 1962b:339, my translation). The difference between cultural anthropology and history must be sought, on the one hand in the opposition individualizing/generalizing, and on the other in the opposition conscious/unconscious. Whereas history mainly emphasizes conscious actions and events which are individualized according to space and time, anthropology has to

stress the unconscious aspects of actions and events. While doing this, cultural anthropology attempts to reach the universal of which the observed is only an expression: its aim is the discovery of invariant characteristics underneath the visible variety of observed phenomena (Lévi-Strauss 1966:126-127). It tries to gain insight into the latent presence of potentialities. Anthropology and history 'have undertaken the same journey on the same road in the same direction; only their orientation is different.... A true two-faced Janus, it is the solidarity of the two disciplines that makes it possible to keep the whole road in sight' (Lévi-Strauss 1958:32).

### Problems

#### (a) Reductionism

The thesis that sociocultural phenomena must not be viewed as phenomena *sui generis* or as the results of conscious human action, but as expressions of determining causes which have a primarily bio-psychological or even neuro-physiological character, leads to an unconditional surrender to a rationality which is rooted in nature. The most important consequence of this surrender has been a reductionism which manifests itself in various ways: the exclusion of conscious signification, explanations which lack emotional motives and the neglect of socio-cultural variables.

A clear indication of a reductionist method is already implied in the continual reference to language as a model for the analysis of culture. As a consequence, Lévi-Strauss shows insufficient awareness of the fact that cultural elements - in contrast to phonemes, for example - possess a rich polyvalent meaning and content which is not in any way completely determined by their position in the more inclusive socio-cultural system. It is even a question whether language and culture are analogous:

The point is that 'cultural rules' are basically concerned with specifying appropriate messages and social contexts; and 'linguistic rules' are basically concerned with the conversion of messages into verbal form. We cannot lightly or confidently assume that the formal organization of these two realms is sufficiently similar to make wholesale anthropological borrowing of linguistic concepts, formalisms or methods productive (Keesing 1972:315).

In other words, the assumption that language as well as culture are products of the human mind does not necessarily imply that the one system can be used profitably to throw light on the other. It could be that the differences which are the result of their different functions and contexts completely overshadow such an

assumed common substratum. However this may be, the reductionism which is implied in the analogy becomes intensified through the connection of culture to the grammatical level of language. The basic problem for Lévi-Strauss is not "what do symbols mean?" but "how do they mean?" (Sperber 1975:51). As a consequence Lévi-Strauss pays insufficient attention to semantics and pragmatics:

The structuralist is no more interested in the semantic and pragmatic role of language than he is in the purpose and praxis of historical events. He is primarily concerned with the logic or code which is said to make both history and language possible and intelligible.... In semiotic terms, the structuralist is interested in *homo significans*, not *homo faber*, nor *homo symbolicum* (Scholte 1979:44).

Lévi-Strauss's firm conviction that final explanations can be found in unconsciously active innate determinants also implies that intermediate variables are pushed into a corner. Put differently, his predilection for 'the unconscious' leads to a search for final explanations, and the relegation to the background of specific factors which distinguish one person or group of people from another person or group. He is only too quick to forget to incorporate all kinds of factors into his explanatory model. It must be obvious that this results in numerous ambiguities and contradictions. A good example can be found in Lévi-Strauss's analysis of myths. He defines a myth as an orally-transmitted, problem-oriented story, and therefore as a dialogue which man has with his environment in order to make disagreeable experiences and problems into a subject of discourse, which thus finally become acceptable to him. At the same time however, he describes myth as thought which reflects upon itself. In this case he speaks of myths which operate in men's minds without their being aware of the fact (Lévi-Strauss 1964a:20). Myths are, in this way, liberated from their dependence on concreteness and therefore become detached from everyday socio-cultural reality. They present an image of the world which is already implied in the '*architecture de l'esprit*' (Lévi-Strauss 1964a:346), which means that they reveal the unconscious activity of thought. Although one could object that Lévi-Strauss has in mind different levels on which myths can be analysed, the question remains how the relationship between these levels ought to be seen. To say that one level refers to the conscious while another refers to the unconscious makes matters too simple, for disagreeable psycho- and socio-logical (and logical) problems also often show a distorted or disguised character.

A consequence of his focus on unconscious structures of thought is that no consideration is given to the possibility that the social, historical and demographic situation of a people may be of decisive importance for the structure of myth. The

question ought to be raised whether it is possible to exclude the influence of the situational context, or to view this context as always subjected to the principles of thought. Is it not possible that structural similarities in the myths of peoples separated by vast distances are caused by the structure of primitive societies, which is determined by historical, social and material conditions rather than a *pensée sauvage* (see Godelier 1971)?

To the extent that Lévi-Strauss's analysis of myths becomes more comparative, this neglect of the context of myths increases. In his treatment of the Asdiwal myth Lévi-Strauss is still occupied with inter-relating various institutions, but in his *Mythologiques* this process of tracing relationships is already much less complete and less specific. Undoubtedly, in this way a part of the meaning of these myths is lost. For example, rites provide keys for the understanding of certain symbols present in the myths (Maybury-Lewis 1970:157). Among certain groups the anthropologist cannot but consider these rites, as there exists no mythology apart from ritual. Rites contribute to the wealth of mythical systems 'by drawing upon tangible signs which derive their meaning and currency from the words of articulated language, the primary constituent of myth' (Heusch 1975:371). Another reason for drawing attention to the importance of ritual is the fact that rites display very clearly the emotional and social functions of myths. Myths not only constitute a totality of cognitive classifications aiming at ordering the universe, they are also - and perhaps primarily - a medium for the evocation, channelling and repression of such feelings as hatred, anxiety, tenderness and sorrow. Thus they are goal-directed, and therefore possess a 'conative' aspect (Turner 1969:42-43; see also Fortes 1967:8-9 and Leach 1970:128). Myths also often contain a charter, as Malinowski used to call it. It must also be pointed out that the exclusion of the emotional and affective incentives of human action from the explanation cannot be related only to Lévi-Strauss's increasing preoccupation with the *how* of thought at the expense of the *what* and *why* of phenomena, but also to his epistemology: 'Actually, impulses and emotions explain nothing: they are always results, either of the power of the body or of the impotence of the mind. In both cases they are consequences, never causes' (Lévi-Strauss 1962a:103). Although Lévi-Strauss counters the criticism that he disregards the emotional aspects of man (see Fox 1967:161-162) with the argument that his comparison between myth and music, for example, shows that he does take emotions into account (see Lévi-Strauss 1971:596), the fact remains that he only perceives this emotion as a reaction to an already-conceptualized world, within a systematized and intellectualized form, such as music. The price which has to be paid for this one-sided view is the neglect of the psycho-emotional load of 'cognitive' paradoxes. Because he disregards this primarily emotional foundation, Lévi-Strauss also finds it impossible to answer the question why miracles occupy such an important position in myths. The true

character of myth is the explanation of the existing order of things by reference to a different order. This different order, however, defines the laws of everyday experience. The mythical order always contains elements which are different from and irreconcilable with the ordinary, every-day order. According priority to human cognition implies that Lévi-Strauss is confronted by the problem that myths must solve problems of thought - as a myth is, according to him, a solution of a logical problem - by means of an impossibility (Van Baal 1977:344; 1981: 164).

Perhaps this impoverished uni-dimensional image of humanity used by Lévi-Strauss is an expression of his search for harmony. It is obvious that this harmony cannot be found in the struggle between various primary springs of action, but in the activity of the human mind, which aims at the creation of order out of chaos, and which acts therefore in a harmonizing manner. It is precisely this metaphysical conviction of harmony that constitutes the final foundation of an important part of his pre-suppositions and explains why Lévi-Strauss puts a one-sided emphasis on certain aspects of phenomena in his substantive analyses, at the expense of others. For instance, in his studies of kinship hardly any attention, if at all, is paid to conflicts about contracting marriages or to attempts to withdraw from marital duties. Nor does he pay attention to the problem of the separation and fission of kin-groups. The question of total political and economic organisation - as well as differential access to and command over natural resources and means of power - is absent from his studies. His almost complete failure to take into account these relations of power, and the antagonism which they stimulate or channel, also brings him into difficulties when he has to explain social transformations.

Lévi-Strauss views cultures as self-regulating systems which are in equilibrium, or at least show a tendency towards it. In Wilden's terminology (1974:286) system stands for 'law and order', and change is the work of 'outside agitators'. Moreover a disturbance is only temporary. An orientation exists towards a new equilibrium which is a 'compromise' between the antecedent state of the system and the disorder which was introduced from the outside (Lévi-Strauss 1962b:92). This is indeed another indication of the priority accorded by Lévi-Strauss to structure as compared with process: an existing structure cannot be explained by reference to historical events, but only to a preceding structure. In other words, he believes that historical reality is of secondary importance *vis-à-vis* structural reality (see also Scholte 1974, 1979). Diachrony is dependent on synchrony. Historical reality is especially important insofar as 'eventualities' make it possible 'to abstract the structure which underlies the many manifestations and remains permanent throughout a succession of events' (Lévi-Strauss 1958:29).

The asymmetric relationship which Lévi-Strauss claims to exist between history and anthropology is equally clear from his

correlation of history with the conscious and anthropology with the unconscious. Now, the conscious is an epiphenomenon of the unconscious. The unconscious provides the grammar and determines the possibilities available for the conscious. Thus we have come to the problem of the relation between structure and process, between order and change which can be found on the cognitive-psychological as well as on the macro-sociological level.

(b) Structure and process

Although Lévi-Strauss's choice for structure at the expense of process can be explained by reference to Saussure's structuralist model of language, this does not of course justify such a choice. In fact it is impossible to do this in the light of recent developments in linguistics and psychology, the two disciplines most relevant here. An important insight which will not be given up so easily and which was gained especially in cognitive psychology is the acknowledgement that 'cognitive structure' should be seen not merely as a product of categorization, but also as a mechanism, constantly changing in the process of categorization. In this view earlier experiences (the past) have been processed and have resulted in a certain codification, which influences new information which has not yet been codified. Constant interaction occurs as new experiences become integrated. This continuing interaction leads to new results and possibly even to structural principles differing qualitatively. It is precisely this permanent duality, always and at the same time structuring and being structured, which ought to be the central issue (Piaget 1968:10). Therefore, it would seem to be dogmatic to accord priority to either of the two. Significantly, at a later stage Lévi-Strauss started increasingly to use the concept of transformation, perhaps also influenced by the generative grammar as developed by Chomsky.<sup>2</sup> He now sees structures as 'generative matrices' which continually cause new orders to come into being through successive transformations (Lévi-Strauss 1971:33, 561), but these continue to belong to one and the same type, because they are being generated according to the same basic rules. In other words he does not renounce his thesis that a limited number of unchangeable principles of ordering do exist, forcing one to the conclusion that Lévi-Strauss does not know how to deal with the concepts of biological and socio-cultural evolution. It is perhaps relevant in this context to refer to the 'Finale' of *L'Homme Nu* (1971). In this passage Lévi-Strauss evokes the ultimate end of all life. For him human existence is only an episode in the development of nature, and human interference itself is just a part of this.

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<sup>2</sup> In Lévi-Strauss's own work one also sees a growing interest in 'structural history' (Lévi-Strauss 1975:71-80).

The final destiny of life is the ultimate destruction implied in the second law of thermodynamics, which states that entropy always increases in time.

At the macro-sociological level Lévi-Strauss's point of view makes it difficult to explain societal transformations, although the incorporation of primitive societies in more inclusive systems is one of the most prominent societal phenomena:

Numerous primitive and peasant societies, the traditional subject-matter of our discipline, if not wiped out altogether or conceptually interred in the 'ethnographic present' have been transformed into or (re-) discovered as components of earlier or present-day encompassing global social formations (Pouwer 1981:2).

This is acknowledged by Lévi-Strauss himself:

... those societies which today we call 'underdeveloped' are not such through their own doing, and one would be wrong to conceive of them as exterior to Western development or indifferent to it. In truth, they are the very societies whose direct or indirect destruction between the sixteenth and the nineteenth centuries have made possible the development of the Western World (Lévi-Strauss 1973:315).<sup>3</sup>

In an attempt to indicate the causes of societal development, Lévi-Strauss mentions the difference between peoples with history and those without it. Of course every community has its own history, but the reaction of societies to this fact differs greatly. Lévi-Strauss proposes a dichotomy between Western and primitive concepts of history. The Western concept is of a history which is cumulative and proceeds strongly in one direction. The historical process becomes internalized and becomes the impetus of development. The primitive concept, by contrast, is of a stationary, less cumulative history which does not follow one main direction (Lévi-Strauss 1973:391). In societies which are characterized by the latter concept, history is only accepted as form without content; although past and future do exist, their only meaning is contained in the fact that

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<sup>3</sup> This perspective also has consequences for evolutionism. 'Now instead of linking the units of the scheme through a *succession* of time, one had rather conceive them as *simultaneous* constituents of articulated, segmented global systems. Evolution should be conceived as a shift, a transformation from one *global system* to another' (Pouwer 1981:2). See also especially Friedman 1979:9-19 and Ekholm and Friedman 1980:61-77, on whose argument the statements of Pouwer are based.

they are mirror images (Lévi-Strauss 1962b:341). The difference between cumulative history and stationary or fluctuating history is the difference between the categories of time which are being used. Cumulative history is characterized by irreversible time, stationary history by reversible time.

Lévi-Strauss compares this difference in concepts of time to the difference between the functioning of clocks or mechanical machines and that of steam engines or thermodynamic machines (Charbonnier 1961:35-47). In the 'clockwork model' one notices the regular occurrence of cyclical processes, which start again once they have returned to their point of departure. Thus primitive societies which are characterized by the clockwork model - by no means all primitive societies (Charbonnier 1961:39) - appear as static societies with no history. In the 'steam-engine model' the relative temperatures of the constituent parts are different. The functioning of steam engines is based on a difference in potential between their parts. In societies this difference manifests itself in different kinds of social hierarchies. In these societies Lévi-Strauss sees a connection between - on the one hand - the development and consumption of energy, and - on the other hand - great tensions caused by strong social contrasts. These contrasts tend to become equalized, only for new contrasts then to arise in a different guise (Charbonnier 1961:46).

However, this exposition of Lévi-Strauss does not touch on a basic problem. In his statements which relate anthropology to linguistics and psychology we find a clear explanatory mechanism, viz. structures of thought. Such an explanatory mechanism is absent from the 'macro-sociological' statements of Lévi-Strauss concerning the connection of anthropology to the science of history. Lévi-Strauss does not answer the important question of how different concepts of time in various societies as well as their associated characteristics could come into being. Both possibilities are given in the simultaneity of *diachrony* and *synchrony*, of process and structure, but why does one society choose reversible time and another irreversible time? Why is society A a 'clockwork society' and society B a 'steam-engine society'? Why and how does the transition from the one type of time to the other occur in any particular society? Lévi-Strauss merely establishes that these two types of time can be distinguished. For an answer to these questions he should have considered technological and economic development, but apart from a few references to Marx he remains silent on this issue. It should be mentioned, however, that Lévi-Strauss has his reasons for letting this question rest. First of all, he maintains that his aim is the outline of a theory of 'collective representations' or 'suprastructures'. This implies that he is passing over or paying insufficient attention to more important phenomena (Lévi-

Strauss 1962b:155).<sup>4</sup> Moreover he believes that it is the task of the science of history - assisted by demography, technology, historical geography and ethnography - to develop the study of real infra-structures (Lévi-Strauss 1962b:174).

### Prospects

The argument so far has brought us to the point where we have to ask what is the task and position of anthropology if on the one hand the basic explanation of its subject-matter has to be found in psychology, and on the other hand the techno-economical aspects of reality have to be studied by the historical sciences. What is left over is merely the study of the consequences - supra-structures - of causes which are the subject-matter of other disciplines. Moreover Lévi-Strauss's reductionist method does not do sufficient justice to these causes, for the structuralism of Lévi-Strauss in its present form has become increasingly the 'negation of all anthropology' (Simonis 1968:344; see also Diamond 1974, Scholte 1979). Lévi-Strauss (1962b:283) himself remarks that the ultimate aim of the human sciences is not the construction but the solution of man. Leaving aside man as a consciously acting and choosing being - because he is the mere concomitant of a determined natural phenomenon - results unavoidably in an impoverishing reductionism no longer dealing with basic anthropological questions concerning what, how and why. One can only escape this reductionism if sufficient attention is given to the different levels of human existence. In my opinion these levels have to a certain extent an existence *sui generis*. In other words knowledge of unconsciously acting structures of thought is perhaps a necessary condition for gaining knowledge about cultural phenomena, but not a sufficient condition. I would prefer to say that there has to be compatibility between the physiological, psychological and socio-cultural levels of human existence. Each of these levels

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<sup>4</sup> More than once Lévi-Strauss has warned against idealistic interpretations. Only societal transformations cause ideological transformations, but the reverse is not true: 'the idea which people conceive of the relations between nature and culture depends on the way in which their own social relations are subject to change' (Lévi-Strauss 1962b:155, my translation; see also 1962b:173). His own work, however, is not entirely consistent with this view. Idealistic arguments can be found in various places, especially in *Le Totémisme Aujourd'hui* (1962a: 130, 142).

is characterized by its own laws, which have to be inter-related.

In theory Lévi-Strauss would agree completely with this point of view, but his research has become increasingly limited to the level of the unconsciously acting principles of thought. This reduces the scope and explanatory power of his structuralism quite unnecessarily. Therefore I would advocate first of all a return to the pre-*mythologiques* stage of his studies, which showed his twofold aim in substantive research: on the one hand the discovery and description of different patterns of socio-cultural phenomena, and on the other hand the demonstration that these 'systems of signs' are a manifestation of the human mind, adapted to specific environments. This twofold aim can be achieved especially if research is oriented towards concrete institutions and ideas. Only if myths, classifications, systems of kinship, forms of art etc., are seen in a wider socio-cultural and politico-economic context is it possible to make connections between the *ordre conçu* and *ordre vécu*. The aim should be what Lévi-Strauss himself has called the construction of an *ordre des ordres*: a description of the formal properties of the whole, made of parts which each correspond to a definite structural level (Lévi-Strauss 1958:365). Lévi-Strauss assumes that all these different parts are inter-related through transformations, which must be annihilated in order to discover a logically ideal homological relationship between the various structural levels (Lévi-Strauss 1958:366). Such a holistic procedure gives greater possibilities in mapping similarities and differences, i.e. relations of transformation. This idea of totality can also be found in the structural-Marxist concept of 'social formation': '... a complex internally structured totality of various layers and levels inter-related in all sorts of relations of determination' (Glucksmann 1974:106). This structural Marxism in particular offers many points of contact for further development of structuralism, not only because of the study of the interaction between various sub-systems (Althusser and Balibar 1970; Godelier 1973, 1975; Meillassoux 1964), but also because of a different view of the relation structure-process. The result is that the problem area is approached from two angles which contrast, but which are at the same time complementary.<sup>5</sup>

Apart from entering into a dialogue with structural Marxism, the structuralism of Lévi-Strauss will also have to come closer to bio-genetic or, more generally, evolutionary structuralism. Here I have in mind especially the work of American anthropologists such as Brady, D'Aquili, Laughlin,

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<sup>5</sup> For a scheme of similarities and differences see Foster-Carter 1978, Pouver 1981:17.

Rubinstein and Stephens.<sup>6</sup> Their research is a development of Jean Piaget's constructive structuralism. Structure and development of thought are not investigated primarily through a comparative analysis of culture products, but much more directly through empirical cognitive-psychological research, involving experiments when possible.

In general, evolutionary structuralist thinkers may be said to hold to the proposition that the explanation of observed phenomena must be made by reference to ontologically real, knowable but rarely observable structures that are systematic in function, pan-human or pan-societal universals, and usually, but not always, unconscious to the actors. In addition, the evolutionary structuralist approach to these structures is developmental, biologically grounded, and neurophysiological or cognitive in attributing the locus of structure (Rubinstein 1981:11).

The basic principle behind this research is the hierarchy of structural levels (Rubinstein 1980, 1981). A good example of this view is the scheme proposed by Laughlin and Brady in which four levels are distinguished. These levels, which function in the human organism, when seen as acting in a social system, are:

(1) the neural infrastructure, the central and peripheral nervous systems, structure and function; (2) the cognitive infrastructure, the mechanisms for processing perceptual material; (3) the societal infrastructure, the mechanisms for organizing individual cognitive infrastructures, such as ritual, institutionalization, bureaucratization, etc.; (4) the surface structure, behavioral expressions of symbolic and meaningful information in the culture pool, including economic, political, social, ideational content, etc. (Laughlin and Brady 1978:4).

The advantages of this approach are a much more explicitly stated research methodology and a reduced dependence on language as a model for cultural analysis. In my opinion this is an important positive development, in view of the problematic relation between culture, language and thought.

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<sup>6</sup> See D'Aquili, E. (1972); D'Aquili, E., C.D. Laughlin, and J. McManus (1979); Laughlin, C.D. and E. D'Aquili (1974); Laughlin, C.D. and I. Brady (1978); Rubinstein, R.A. (1980); Rubinstein, R.A. (1981); Rubinstein, R.A. and C.D. Laughlin (1977).

Considering the above-mentioned problems in the structuralism of Lévi-Strauss - namely reductionism and emphasis on synchrony - developments in these related scientific orientations will have to be integrated into it, if the self-imposed barriers surrounding this paradigm are to be brought down. This should not be too difficult because 'orthodox structuralism', structural Marxism and biogenetic structuralism constitute a set of contrasts, a system of similarities and differences. The similarities can be found in the basic postulates, while the differences are mainly of a lower order: they are limited partly to some principles and partly to research priorities. The structuralism of Lévi-Strauss itself, however, is also in need of further extension and reformulation. Here I have in mind the systematization of the conceptual apparatus and the formulation of an explicit research methodology, especially the unequivocal statement of the rules of argumentation, through which scientific proof can be established. Having regard to the start which several anthropologists have made in this direction (Pouwer 1974; Rossi 1974) we may expect that structuralism will continue to become a paradigm bearing scientific fruits for a long time to come.

A,DE RUIJTER

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## ANDAMANESE SEX ROLES

Differentiation in male and female roles has frequently contributed to the demonstration of the inferior status of women, or of their consignment to the low-prestige family role of child-rearing. We should like to discuss here sex roles among the Andamanese, which present a situation different from that of other hunting and gathering societies in India and South-East Asia and may be related to the historical isolation of their islands. Our analysis is based primarily on the data of E.H.Man (1883) and Radcliffe-Brown (1933), the two principal field-workers among the Andamanese. They did not make any special analysis of women's roles, but offered only general remarks on male-female relations and what Man called 'their marked equality, affection, and self-respect'. They both note a clear-cut sexual division of labour as found among other hunters and gatherers, and a generalized female involvement in ritual and political activities.

### *Economic Activities*

Precise information is lacking as far as the economic role of women is concerned. According to Murdock (1968), hunting contributes 20% of the diet, fishing 30% and gathering 50%. Men and women, according to these figures, contribute equally to the subsistence of the group. The dietary study carried out by Bose for the Onge of Little Andaman during January 1964 (Bose 1964) also deserves mention. Meehan (1977) presents her data in terms of weight: pig and turtle represent 472 kg. and 24 kg. of edible flesh respectively; gathering products account for 259 kg., shellfish for 3 kg. and fish for 179 kg. Although she does not mention it, the fishing was probably done mainly by women as, according to Bose's chart,

pig hunting - a male occupation - was carried out almost daily. This study thus confirms the approximately equal contribution of men and women in obtaining food.

As in all hunting and gathering societies, men are the sole hunters of the group. We may note, however, that hunting depends on the season; it is especially important from May to August, but almost non-existent from March to May (during the north-east monsoon), during which period the men help the women in collecting tubers. Ethnographic accounts report the sharing of game between men and women within the group; after a successful hunt, pigs are cut up and partly cooked, and lumps of meat are then divided among all the families present, who finish the preparation alone. We have no way of knowing the extent of this sharing or the amounts of meat consumed by the women subsequently. However, all authors report the existence of large feasts after a hunt, where meat is divided up and eaten in large quantities by the whole group, and where dancing follows. These huge meals are then followed for several days by a diet of fruit and vegetables. This would imply that, whatever kind of food is available, is shared by the whole group, at least at the evening meals.

In the case of fishing, the men go out into the open sea in canoes in search of dugong, turtle and the larger fish, and also catch small fish inshore with their bows and arrows. Women fish collectively on the coast, using small hand-nets or even their bare hands, mostly catching lobsters, crabs, shellfish and turtles grounded on the beach. Large-scale collective fishing by all members of the community, irrespective of sex or of age (except for the very young and the very old), also occurs when the opportunity arises. The whole shoal may then be netted. As for the sharing of fish, Sarkar (1974) mentions that among the Onge of Little Andaman women divide their catch among their respective families, while the catches of individual men are retained by them. On the other hand, the catches of organised collective fishing by both men and women are distributed equally by all the families of the local group. Sarkar writes that 'both males and females play their assigned roles and none of their roles is less important in relation to one another as one supplements the other....' When men are unlucky in sea fishing, women provide their coastal catches. According to Sarkar, the women's contribution to fishing is considered significant, and they enjoy a certain prestige and position. Thus it seems that all able-bodied adults procure and profit from food without discrimination. Each sex specializes in a certain kind of labour and shares resources according to availability.

Man and Radcliffe Brown both mention exchange transactions of meat and fish between coast-dwellers and jungle-dwellers; wax, clay artefacts and decorated bones, all manufactured by women, are exchanged also. Women are excluded from any decisions concerning hunting and sea-fishing, while on the other hand it may well be that men are excluded from any decisions concerning gathering.

### *Manufacturing Techniques*

Men and women also have assigned and clearly defined roles for manufacturing hunting equipment and other implements. Generally, men manufacture all their hunting and fishing equipment (canoes, weapons, cords, lines and turtle nets), while women make their own fishing-nets, slings, sleeping-mats, basket-work, screens and torches, and prepare cords for bags, necklaces, arrow fastenings, bracelets, etc. Both sexes use a cyrena shell to make cords and lines. Another common implement is the stone hammer, which men use for flattening arrow-heads and women for making bone necklaces. Both sexes are equally involved in making pottery.

Women are also in charge of preparing the various clay washes and waxes needed for ritual purposes, mainly *tala-og* (white clay mixed with water), *koi-ob* (a mixture of oxide of iron with turtle- or pig-fat) and *kangata-buj* (red wax). This last item is also manufactured by men for use in protecting the string fastenings of arrows, spears and harpoons. These washes are prepared by women for the body decorations used during ceremonies, and they also make many other objects, such as necklaces, trays, paddles, bows, etc. As far as house-building is concerned, permanent encampments and temporary homes for periods of a few months (as for example in a period of mourning) are erected by men, while shelters for short visits and temporary rests are built by women.

### *Political Activities*

Elderly persons of both sexes take communal decisions, and hold similar positions within the tribe. Radcliffe-Brown relates that women seem to have a good deal of influence over any decisions concerning the encampment, and over quarrels between individuals and local groups. Man states that the headmen are male, and that they act merely as influential guides for organising hunting expeditions and local group meetings. Both the headman and his wife enjoy respect, and the wife rules over all the unmarried women of the camp; even when she becomes a widow, her respectable and influential position stays the same. In North Andaman, for instance, feuds between neighbouring local groups were quite frequent. At the end of a feud there would be a special peace-making ceremony, which Radcliffe-Brown describes as follows:

One or two of the women of the one group would be sent to interview the women of the other group to see if they were willing to forget the past and make friends. It seems that *it was largely the rancour of the women over their slain relatives* that kept the feud alive, the men of the two parties being willing to make friends much more readily than the women (1933; our emphasis).

His experience suggests that women, although sent by the men to negotiate peace, were not merely mediators, but had real decision-

making power and exerted a strong influence on the men as to when the fighting should cease; it was they, in fact, who counted bodies and demanded vengeance.

### *Ritual Activities*

Women are never excluded from any of the rituals described by Man and Radcliffe-Brown. Certain roles assumed by them in these ceremonies suggest that they are as important as men to them, and they participate fully in the symbolic activities. Women play an especially important role in two of these activities - scarification and body decoration.

#### 1. Scarification

Young girls and boys are submitted to scarification from their early childhood. The operation is repeated at intervals during childhood until the whole body is scarified. Women make the incisions with a small flake of quartz and are the main practitioners. The sole exception is the scarifying performed on the backs of young boys during their initiation ceremonies by an elder man using a pig arrow; these are connected especially with hunting.

#### 2. Clay decoration and patterns

These decorations are used during the extremely elaborate ceremonies which occur at intervals during the life-cycle. These ceremonies include initiation at puberty for both boys and girls; marriage; the end of mourning; communal peace-making; and special occasions where paintings and dancing are required, such as meetings of different local groups, the death of a relative, before burial, after successful hunts followed by big feasts, and during illness. The initiation ceremonies are among the most elaborate and comprise several steps, among which the most prominent are the turtle-eating ceremony (mainly for coast-dwellers), the pig-eating ceremony, and the honey-eating ceremony. These relate to food prohibitions that start with the initiation period (for children of around eight years of age), corresponding with the onset of a period of fasting. Young girls and boys have to observe certain restrictions, which are re-stated at each new ceremony. For example, the turtle ceremony enables them to eat turtle - previously forbidden - but other foods remain prohibited. In connection with each of these there is a minor ceremony, so that one after the other, all food prohibitions are removed until the man or woman becomes free to eat anything (this being at marriage for a man, and at the time of first pregnancy for a woman).

During and after each of these ceremonies, clay patterns and decorations are made on the bodies of both men and women. Two forms of decoration can be distinguished:

a) A smear of common clay is used for initiates, warriors, homicides, and sick people, and also for participants in the orgiastic meals that follow a successful hunt, due to the fear that animal

smells may attract evil spirits. These periods correspond to danger. People are called *kimil*, which refers to a state of danger, and they are covered with this ordinary clay which alleviates the 'smell' of the *kimil* and keeps evil spirits away. This clay does not require special patterns and is generally applied by each man and woman on his or her own body.

b) Elaborate patterns made of fine white clay and red ochre are used for the decoration of men's and women's bodies before the final phase of these ceremonies, which consist of dancing and singing. These patterns correspond to the end of a ritually taboo condition; initiates, mourners, etc., formerly in a *kimil* condition, are removed from danger. As Leach (1965) points out, this decoration serves as an agent of transformation ending the *kimil* condition, which is, for him, an ambiguous state, in view of the application of ordinary clay. These decorations are made only by women.

These same patterns are found again when, at the end of the mourning period, the bones of the deceased relatives are dug up, ornamented and painted, and used as cures for sickness or as presents to relatives or members of other local groups. Once again, women have the monopoly of this symbolic representation.

Thus it seems that women are not only included in the ritual human cycle but are directly concerned in more everyday activities. Since we know little about Andamanese values, our purpose is not so much to prove that the Andamanese situation reflects one of male-female equality, but to show that, as far as we can tell from the available evidence, women create links of cooperation between persons, families and even local groups. By their specific economic, ritual and political functions, they contribute largely, and are indeed essential to the economic and social life of the group.

Many essays have been written on sex roles, and particularly on universal male domination. We will not take up this discussion here, except to stress two points. First, as Briggs has noted (1974), concepts such as domination, and equality and inequality, should be avoided until valid criteria are established. Are they to refer to a range of specialised knowledge, to control over the means of production or reproduction, or to personal domination? Among the Andamanese, both male and female roles are essential to the whole group and mutually supportive, even where differentiated. Every member of the group controls food production and the manufacture of goods, and every member is equally responsible for ritual cooperation and peace. Differentiation does not give way, in the Andamanese society, to demonstrations of superiority or prestige, at least not explicitly.

Secondly, public and family spheres are often said to be universal opposites, with women confined to child-rearing, and men taking care of political affairs and extra-communal problems. We have just seen, however, that among elders, decision-making about production, distribution, settlements, disputes, ritual feasts, wars, etc., is often collective. Men individually make decisions about the activities for which they are responsible, as do women.

At the ritual, political and ideological levels women, by virtue of the fact that they transcend mere mediation between men, participate in public life; and apparently this cooperation does not seem to lead to any conflicts between the sexes. The place of women in Andamanese society thus reflects the relations of egalitarianism, cooperation and solidarity that hunting and gathering societies require. Avoidance of conflicts, recognition of individual prestige on the basis of knowledge rather than power, and the sharing principle are values recognized by all, and necessary to the continuity of the society.

Leacock, in a recent article (1978), has attempted to place women's status in a historical perspective, and has argued that the tendency to attribute to pre-class societies concepts such as equality, power and property - all characteristic of class societies - becomes equivocal where such concepts are applied to band societies, where decisions are taken by all members of the group, and where there is no dichotomy between public and private spheres. She has also pointed out that women's control generally declines with the advent of trade. In the light of this, we shall now examine the extent to which sex roles among the Andamanese may be a consequence of their historical isolation.

The position of the Andamanese female seems rather different from that of all other hunting and gathering societies in South and South-east Asia. Although among the Birhor, Paliyans, Veddas, Negritos, Aetas, etc., men do not seem to exercise control over women's labour and reproduction, and there is no rigorous separation of men and women; nowhere is there to be found such a specific inclusion of women in political as well as ritual life as among the Andamanese.

Among the Birhor of Chota Nagpur (India) ceremonial knowledge is a male preserve, scarification being done only on male children, and by male elders (Williams 1974). Roy earlier (1925) mentioned that all Birhor exclude females (except for girls who have not yet reached the age of puberty) from their spirit-huts. Women are not allowed to eat the heads of animals captured in the hunt or sacrificed to the spirits; ordinarily men alone are entitled to offer sacrifices to the spirits and only they have personal relations with them. There is one exception, however, where a woman may have to offer sacrifices to her own *manita* spirits, which are transmitted through the female line. Only in this case may she eat the head of a sacrificed animal, which she shares with her husband, although it is her husband who actually performs the sacrifice.

Concerning the Paliyans of south India Gardner (1972) reports a sexual egalitarianism, members of both sexes being in contact with individual supernatural beings. He also reports the exchange of clothes between men and women during dances, which he describes as an expression of sexual complementarity. However, he describes what is a situation of minimal economic cooperation between the sexes, in which a husband and wife will sometimes feed only themselves for several months.

Among the Veddas, the Seligmans reported (1911) that most ceremonies and invocations were connected with hunting, and that men were the officiants at them; women did rock paintings, but these do not seem to have been connected with a precise ritual activity. Anthropologists working with many Indian groups have commented on the strict seclusion of women during polluting periods such as childbirth and menses. A parturient Birhor woman has a new doorway made to her confinement room, and for a certain number of days after delivery, during which contact with her is generally taboo, she must use only this entrance door. Food prohibitions are quite important during pregnancy and remain so after delivery, but they apply only to women. This is rather in contrast to the Andamanese, where husbands attend their wives when they are giving birth, and share the same food prohibitions.

Evans reports (1937) that face paintings, scarification and nose-piercing are common among the Negritos of Malaysia. He states that both sexes ornament themselves, but unfortunately gives no further details here. Shamans are all male, and designs on combs (worn during dances by women), on birth bamboos (worn by women during pregnancy) and on blowpipes (engraved to be sure that the game will be not only struck but killed) are engraved only by men. Schebesta, in a note concerning the Semang of Patalung, Thailand (1925), mentions that the head of the tribe was a woman, but his information is unreliable as he never saw the camp, but only met a few men in a neighbouring village. Among the Penan, and among similar groups in the Philippines, no precise data is available on female and male social roles, but only on the traditional sexual division of labour. In general, ethnographic accounts note relative equality between men and women in these societies, yet female control of production seems to be exercised in the interests of individuals rather than the collectivity, and there is no evidence for cooperation between the sexes with regard to any social level.

On the whole, differences between the Andaman Islands and similar groups in South and South-east Asia are great. The Andamanese are characterized by elaborate and complex rituals and mythology, by corporate entities, by elaborate technology (S-shaped bows, pottery, outrigger canoes), and by cooperation and solidarity at all levels in the male-female relationships we have just described. What is most striking is that the Andamanese, perhaps due to their isolation, had no trading relationships - not even silent barter - with outsiders prior to annexation, as far as we know. This isolation may be a reason for these differences; trade may have the effect of undermining cooperation between the sexes. And it is significant that hunting and gathering societies in India, Malaysia, Indonesia and the Philippines have been trading with agriculturalists for a long time. Tamil literature of the first seven centuries AD describes tribal cultures with precision - both swiddening and hunting and gathering types - and also gives evidence for trade in forest products. In 1681, Robert Knox reported the Veddas trading with the Sinhalese kingdom, exchanging forest products for iron and arrow-points (cited

in Seligman and Seligman 1911). Needham (1954) reports that the nomadic Penan of the interior of northwestern Borneo conclude blood-pacts with the long-house groups of the settled Kayan. By these pacts the Penan secure immunity from attack, while the Kayan are able to procure forest products from the Penan. Silent trade is reported from the first century between on the one hand, the Negritos of the Malay Peninsula and the Aetas of the Philippines, and on the other, Chinese, Arab and Malay traders. The Semang traded wax, resin, gum and other products for salt, beads, cloth, metal articles and weapons. Archaeological work in the Philippines (Hutterer 1974) has revealed the antiquity of trade between the nomadic tribes of the interior and the Chinese. This trade seems to have shifted over time to a symbiotic exchange of the produce of agricultural populations, especially iron, for forest products.

There is no doubt that the loosely organised groups composed of independent nuclear families is a recent phenomenon, a consequence of the dependence of hunting and gathering societies on the capitalist economy, and in some cases, of their involvement in sporadic agriculture; but it is certain that barter and a relative degree of specialisation have preceded the development of modern capitalism. In Carreon's translation of a Spanish manuscript - itself transcribed from the original Visayan of around AD 1200 - we learn that the Negritos of the Panay Islands traded various forest products and engaged in both agriculture and hunting and gathering (Carreon 1957).

It appears, therefore, that the interaction between hunting and agricultural populations is of great antiquity, introducing an element of specialization among the hunters, and making them to a certain extent dependent on agriculturalists. As Leacock points out (1978), processes concerning specialization and exchange may contribute to the delineation of a public domain in which men become more important economically and politically, their activities being seen as more prestigious, while there is a parallel and general decline in female control. In other words, although the advent of trade does not really seem to undermine the egalitarian ideal of hunting and gathering societies, it may in some way undermine cooperative relations between the sexes. As for the Andaman Islanders, exchanges between coast-dwellers and jungledwellers, mostly of fish, meat, washes of clay and ornamented articles, did exist, but these never seem to have extended beyond the islands. Iron was not originally used for tools or arrows, though in more recent centuries some came to be obtained from shipwrecks. Thus many hunting and gathering societies in India and South-east Asia would seem to have developed symbiotic relations with agriculturalists, who have affected the formers' social organisation through their contact, while others, like the Andamanese, have retreated instead into hostile isolation. It is these two responses - cultural transformation and isolation - that have variously arisen from such contacts in these areas.

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ULYSSES SANTAMARIA

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KETU MYTHS AND THE STATUS OF WOMEN:  
A STRUCTURAL INTERPRETATION OF  
SOME YORUBA MYTHS OF ORIGIN

Modern literature on the status of Yoruba women in south-western Nigeria has corrected the view that they were oppressed by throwing into relief areas in which they were prominent. Awe has drawn attention to the prominent part women like the Iyalode played in traditional Yoruba politics (1977; 1979), while Atanda (1979) and Babayemi (1979) have stressed the significant roles of women in the palace organisation of Oyo. Karanja (1980) has studied Yoruba market women, showing the economic strength that made them powerful. Finally, in drawing attention to the role of women as mothers and as occupiers of the innermost and sacrosanct space within Yoruba domains, Callaway has demonstrated the importance of Yoruba women to many central features of Yoruba society (1978; 1980). In this article some Yoruba myths will be discussed and analysed, in order to throw into relief this prominence of Yoruba women.

The Ketu<sup>1</sup> share with other Yoruba sub-groups those myths which attempt to explain their origin. These fall into two categories, those of creation and those of migration. The former have a metaphysical quality, and attempt to explain how the whole world originated from the Creator *Olodumare* who chose Ile-Ife as the spot for the creation of the universe. It is this type of myth, and what it says about women, that will form the focus of my attention.

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<sup>1</sup> The Ketu of western Yorubaland, a sub-group of the Yoruba people of south-western Nigeria, are located astride the Dahomey-Nigeria boundary. The town of Ketu from which the name derives was the capital of the ancient kingdom of that name (Parrinder 1956), much of which now lies in the People's Republic of Benin (Dahomey).

The first myth of creation can be described very briefly. The Supreme Being (*Oloдумаре*) resides in the heavens with his divinities (*орisha*). Below this spiritual universe was a great void and a great deal of water. It occurred to *Oloдумаре* to create another universe peopled by mankind. He gave an order to the arch-divinity *Orisha-nla*<sup>2</sup> to descend and begin the work of creation. *Oloдумаре* gave *Orisha-nla* a five-toed hen, a chameleon, a small quantity of loose earth in a snail shell, and a chain with which to descend into the void. *Orisha-nla* left the presence of *Oloдумаре* determined to carry out this task to the best of his ability. On his way he became thirsty and helped himself to a copious amount of palm-wine. He became intoxicated and fell into a deep sleep. When he did not return to the heavens at the appointed time, *Oloдумаре* sent down *Orisha-nla*'s younger brother, *Oduduwa*, to find out if all was well with him. *Oduduwa* came upon the drunken *Orisha-nla*, took stock of the situation, and quietly proceeded to carry out the work of creation. He descended into the void by the chain, threw the loose earth onto the water, and released the five-toed hen upon it. The hen scratched the earth and spread it to the end of the world. Then *Oduduwa* let down the chameleon to test the firmness of the earth. The chameleon had been chosen for this test because of its extreme carefulness and the delicacy of its movements. It gave its approval, and the spot where the creation took place was called 'spreading' (*Ife*) in commemoration of the event.

*Orisha-nla* appeared not long after, to discover that his task had been accomplished by his younger brother who had, in so doing, usurped his right as the creator of the earth. He quarrelled with *Oduduwa* for his impertinence. The quarrel was reported to *Oloдумаре*, who effected a reconciliation between the estranged brothers. In compensation, he commissioned *Orisha-nla* to carry out the task of moulding the human figure. He confirmed *Oduduwa*'s right to own and rule the earth on his behalf from the sacred city of *Ife*. *Oduduwa* later gave birth to several children, who moved away from *Ife* to establish kingdoms of their own. His second child, a female, gave birth to *Alaketu*, whose offspring are the *Ketu*.

A second myth was collected by Ellis (1894). It asserted that the Yoruba originated from incestuous pairings among the divinities. In this myth, *Oduduwa* is presented as a female, the earth goddess.<sup>3</sup> *Orisha-nla*, the arch-divinity, belonged to the

<sup>2</sup> *Orisha-nla* is the arch-divinity and the most important spiritual being after *Oloдумаре*. He may be conceived as the refraction of the creative power of the Supreme Being since he is said to be the Divinity assigned to mould the shapes of living things.

<sup>3</sup> There is disagreement over the sex of *Oduduwa*. Ajisafe (1924:10) asserts that he was the husband of *Omonide*, mother of *Alaketu*, etc. Parrinder (1956) recorded a *Ketu* tradition which claimed *Oduduwa* to be female. Idowu (1962:24-7) says that the historical personality of *Oduduwa*, the deified ancestor of the Yoruba, was

heavens. He married *Oduduwa* and they had two children, the 'land' (*Aganju*), and the 'water' (*Yemaja*). These engaged in sibling marriage and gave birth to the 'air' (*Orungan*), that is, the region between the solid earth and the edge of the heavens. *Orungan* brought the incestuous relationships to a head when he ravished his mother *Yemaja*, who in an attempt to escape further humiliation purposely fell and burst open, whereupon a number of minor divinities emerged from her gaping body.

A third myth of creation shows the link between these two. It also bears out a message of the myths, which emphasize the important position of women in Yoruba society. This third myth recounts the great escapades of a virtuous Yoruba woman of great beauty, *Moremi*. She succeeded in freeing her people from the attacks of a neighbouring group called the Igbos (these have nothing to do with the Igbo, a major, but separate sociolinguistic group). On previous raids the Ife warriors, all male, would not defend themselves against their attackers, who dressed as if they were super-human. *Moremi* made a vow to the river goddess *Esinmerin* that if the goddess would help her to overcome the attackers, she would sacrifice her dearest possession to her. At the next Igbo raid she allowed herself to be captured, and being a woman of great beauty, she was given as wife to the King of the Igbos, into whose favours she endeared herself. During her captivity, she discovered that her dreaded enemies were human beings in a special type of dress made of grass, so that they resembled spirits. She escaped, went back to her people, and revealed this secret to them. The attackers were met at the next incursion with lighted torches, and the Ife victory was decisive. In fulfilment of her vow *Moremi* made great sacrifices to the river goddess, who refused all, demanding instead *Moremi*'s only child, *Oluorogbo*. *Moremi* complied and her loss was mourned by all her people. *Oluorogbo* rose up and later ascended to heaven on a rope.

As myths, these stories cannot, of course, be taken as an accurate record of historical events. Among the conservative Yoruba, they served as charters for social actions which aimed at accommodating new experiences with existing traditional practices (Simpson 1937: 5). Thus they projected the current social outlook and hopes into the past, in order to accord them the authority of the ancestors. This sanction was vital to the acceptance of any new idea for a people who believed that their social order and values were ancient, unchanging and continuous, having been handed down from one generation to the next. On the other hand, for a people struggling to blur, if not erase, the memory of a recent traumatic experience, the myths may also serve as a means of counterfeiting history. Such stories become vital to the group's attempts to re-unify after a period of internal division, by endeavouring to put an end to the recurring dialectic of segmentary fission and fusion, through the building-up of an elaborate genealogy specifying the common

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grafted onto that of the earth goddess *Oduduwa*, whose cults flourish at Ado and Imeho in western Yorubaland.

origin of all sub-groups. The implication is that the desire to unite is of ancestral origin. In the words of Fortes, such myths of origin 'become intelligible when it is realized that they are nothing more than formulations of the contemporary scheme of political and ceremonial relationships' (1945:23).

In the structural tradition, however, myths have been shown to express the contradictions in the basic premises of culture. In doing this, they show the gap between what cultural institutions try to achieve and what they succeed in achieving. Although such myths are related to given empirical facts and reality, they are not a straight reflection of reality. One must analyse them to identify the contradictions they mediate, if one is to get at the message they convey. It is in this light that I shall now look more closely at the Yoruba myths I have already described, following the example of Leach's analysis (1969) of the story of Creation in the Book of Genesis.

The antinomies of 'heaven' and 'earth', 'good' and 'bad', 'strong' and 'weak' are built into the structure of each of these myths. In the first myth there is a clear distinction between the deity and man. Thus *Olodumare*, in heaven, delegated the arch-divinity to create men below. These sets of oppositions imply one other, 'above' and 'below'. The link between them comes in the divinities *Orisha-nla* and *Oduduwa*, who symbolize opposition in their final act of separation, with one going up to heaven and the other becoming ruler on earth. Yet *Oduduwa* on earth is heaven-bound, having originated there, while the arch-divinity is earthbound, having been assigned the task of moulding the figure of the men who inhabit the earth.

The characters of the arch-divinity and *Oduduwa* develop the theme of opposition. Thus the 'good' and 'strong-willed' *Oduduwa* resists the temptation to drink, and succeeds in accomplishing the task of the 'bad' and 'weak' *Orisha-nla*. In the second story, the 'bad' *Orungan* commits incest with his 'good' mother (the only permitted incest among the divinities being that between brother and sister), who demonstrates a considerable measure of strength by attempting to escape. In the third story, the 'good' and 'strong' *Moremi* finds a solution to the problem of her people, while the sacred, but 'weak' and cowardly men of Ife fail to defend the people. The sets of oppositions which emerge from these relationships may be rendered as:

|        |       |
|--------|-------|
| heaven | earth |
| good   | bad   |
| strong | weak  |
| above  | below |

When we add sexual opposition to these, on the merits of the characters in the stories, we must assign woman to the column of strength and man to that of weakness. This anticipates the suggestion that underlies this article, that Ketu women are powerful and that the instances of overt expression of male dominance are a mechanism of male defence against the perceived power of women.

At the level of discrimination in sexual relations, the analysis offers a more interesting insight. Most societies recognize some rule of incest and exogamy, however this may be defined. In the second myth, although the divinities regard sibling incest as 'normal', they view child-parent incest differently, and the attempted escape of *Yemaja* expresses disapproval of it. In the story of *Moremi*, the marriage of the heroine to the Igbo king, no matter how brief, exemplifies exogamous marriage or alliance, as against the practice of marriage within the group. The contrasting point of interest is that while the incestuous assault of *Yemaja* by *Orungan* leads to the former's death, the exogamous liaison of *Moremi* with the Igbo chief results in the preservation of *Moremi*'s people. This is an example of transformation where the end-result of the second set is a reversal of the first. On the one hand, coercive endogamy leads to death; on the other, exogamy (albeit temporary) leads to life.

In the myths about *Moremi* and the incestuous dealings of the divinities, women play very active and positive parts. Added to the fact that there is no complete agreement on the sex of *Oduduwa* who is, in most areas of Yorubaland, regarded as female, the imbuing of women with such characteristics as strength, life, fertility and courage is clear. The same myths associate the opposing characteristics of weakness, death and lack of courage with men.

There are three dominant themes in these myths:

- 1) they emphasize the insistence on marriage within the group, so as to keep the blood of the group pure;
- 2) they speak of the change necessitated by practical political considerations, which suggests the wisdom of creating a peaceful link with neighbouring groups through exogamous marriage;
- 3) they speak of the powerlessness of Ketu men, who realize they are dependent on women to effect these extra-group alliances. This powerlessness is masked by the ideology of male dominance, and by the socialization of women, which encourages them to accept such dominance.

Yoruba tradition insists on tribal endogamy, so as to keep the blood of the group pure. Although this practice has created a sense of exclusiveness and unity within the group in relation to others, the *Moremi* myth shows the price of maintaining such exclusiveness. To conservative elements within the society, the proposal to allow intermarriage with neighbouring groups was regarded as a complete break with tradition. The myth of *Moremi*, and the incest of the divinities and their consequences, provide the ideological underpinning and charter for the change. In so doing, they help the ordinary Ketu male accept the new situation as traditionally ordained, thus avoiding the conflict that may accompany social change.

The compromise in tradition implied in the transfer of females has had another effect on the relations between the sexes in this society. For the Yoruba man, claiming superiority over the Yoruba female, the realization that he depends on females for survival has weakened his feeling of dominance. These myths not only portray this contradiction, which is inherent in the patrilineally-

oriented world of the Yoruba, but also present a permanent paradox running through all relations between the sexes.

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## BOOK REVIEWS.

RIK PINXTEN, INGRID van DOOREN and FRANK HARVEY, *The Anthropology of Space*, Philadelphia: University of Pennsylvania Press 1983. xi, 179pp., Appendixes, Bibliography, Index. £24.40.

One of the things which ethnographers are always on the lookout for (it may safely be presumed) are new methods which will help them to make sense, or more sense, of the data which they have. *Anthropology of Space* is candid about it being, in one guise, an aid to fieldwork, a kind of field manual. It is based on ten months' fieldwork in 1976-77, and consists of six chapters, an Introduction, and a Foreword by Oswald Werner, of Northwestern University, whose student Pinxten once was.

According to Professor Werner, 'this book is bound to be controversial'. Perhaps one gets blasé at Oxford, but it is hard, frankly, to understand why Werner should think this to be so. To start with, the book attempts to bridge the gap between what is called 'armchair' anthropology and fieldwork. It is true that nowadays fieldwork is generally considered to be a *sine qua non* of anyone aspiring to be a professional social anthropologist. But fieldwork does not guarantee a job, and it often seems to be the case that fieldwork and the data collected by fieldworkers are not considered to be the most important criteria by which to judge the quality of a person's work. In the days when fieldwork was the exception rather than the rule for most social anthropologists, moreover, the relationship between those who stayed at home and those who went to the field (or, rather, found themselves in exotic surroundings because of their other activities) was very close indeed. And even today, the work of theoreticians and synthesizers and of fieldworkers

is practically inseparable.

Pinxten is refreshingly and directly committed to social facts. He writes that 'it is taken as a point of departure that no semantic element whatsoever is known, prior to thorough empirical analysis and actual painstaking field procedures having been carried out'. And later he expresses the view, with which one can only concur, that 'certain percepts and the linguistic expression of these percepts might be universal or they might be limited to a particular cultural or linguistic community; but only detailed and painstaking empirical analysis can decide this question in each case.'

*Anthropology of Space* contains a deal of ethnography about Navajo conceptions of space. Chapter 1, 'The Natural Philosophy of Navajo Language and World View', is 32 pages long and contains fairly detailed information about this area of Navajo ideology. The information presented is a composite of statements of a number of 'consultants', listed in Appendix A, who are acknowledged in the text. (The main consultant, Frank Harvey, is one of the authors of the book.) Later chapters, especially the semantic analyses which Pinxten carries out, rest on this data, which were collected by 'the methodologically rather vague...classical ethnographic methods'.

A major finding of this chapter is that everything in the Navajo world moves, that is to say, the Navajo cosmos is composed of processes and events. The concept of 'movement' is difficult: it cannot be understood as actual movement or displacement. Rather, movement (dynamic) is much less specific and more general - 'persisting through eventual change'. The Navajo view of 'being', 'becoming', and 'growing', is also dynamic, so that existing is more a series of events than states or 'situational persistences through time'. This idea is (for the non-specialist) an interesting suggestion. It seems to this reviewer, though, to be based on rather thin evidence - 'probably incomplete', in Pinxten's words - in spite of their impeccable derivation. The idea, further, does not take account of the only exception to the general rule of change: 'the dead body of a person is the one and only thing that is still.'

Chapter 3, 'Linguistic Material on Navajo Spatial Meaning', is the longest in the book. It runs to 72 pages. The way data are analysed is as follows: a Navajo term is translated into an English expression. A description is then given in English of the meaning, the use, the 'nonverbal correlates', etc., of the term. Examples are supplied in Navajo sentences with English translations.

So far, one takes it, there is nothing very remarkable about this procedure. *The Northern States of Fiji*, for instance, is replete with such exercises, and so is *Do Kamo*. And *Kédang* relies heavily upon a similar, though less explicit, technique. Such approaches to linguistic material are often very illuminating. The final step in the analysis is what is original: the 'components' of each term are detailed. 'Components' refers to the 'list of relevant entries of the UFOR, making up a character-

ization of the meaning of the particular Navajo term'. The UFOR is the Universal Frame of Reference, the analytical tool used throughout *Anthropology of Space*.

This reviewer's impression is that the UFOR, which derives from work which Pinxten did for his doctorate, is meant in many ways to be the most important contribution which this book aims to make to social anthropology. The UFOR, it is contended, transcends individual or cultural bias, and it aims to set out, in Werner's words, the 'maximum complete set of spatial discriminations that human beings are capable of making'. 'The sophisticated, highly refined methods and techniques of semantic analysis' inherent in 'The Device' (i.e. the UFOR) are set out in a long Appendix B. The kind of semantics which Pinxten embraces he characterises as 'post-Quine and anti-Katz'.

'Space' is divided into three different spaces which are represented in semantic representations: physical space, socio-geographical space, and cosmological space. The list of entries under the three categories totals 67. The entries under socio-geographical space and cosmological space are amended (i.e. truncated) lists of the 45 entries under physical space.

The entries represent a 'spatial phenomenon' in 'the most neutral and culture-free (or "pre-culturalized") way possible .... The UFOR maps out all spatial differentiations with which man-the-knower has to deal and which are subject to cognition because of their physical prominence and/or because of the biological constitution of people induces these spatial characteristics (e.g. the structure of the body induces the distinction between three spatial dimensions).' So it would appear that the entries are only neutral and culture-free to the extent that what is physically prominent and what 'the biological constitution of people' consists of can be argued to be definable in ways which have these characteristics. This reviewer's feeling is that this problem of defining the assumptions upon which the UFOR is based probably (and expectably) vitiates its claims to be neutral.

Not that this fact is particularly important for the prosecution of fieldwork. Pinxten lists a number of things to do with spatial conceptions which might usefully be raised in discussions in the field. However, 'a genuinely universalist pretension is typical.' This pretension is indeed most noticeable in the formal language and in the notations which are adopted in most of the book. Not that there is anything necessarily wrong with formalism, of course, although an assessment of the usefulness of a specific technique must depend upon the results which it procures.

One example is entry 122, 'Cardinal Points, Cardinal Directions', which is applicable with certain provisos to entries 222 and 322 ('Cardinal Points/Cardinal Directions in Sociogeographical and Cosmological Space') also: 'Cardinal directions (and cardinal points) are recognized, provided human beings organize their actions, their ways of moving, their constructions, or anything in the actional world in such a way that

they are located in space as analogous to the different positions of the sun in the sky, from rising to zenith to setting places. The fourth direction is analogous to the one "where the sun never comes". This analysis is based upon the view that 'the different positions of the sun during a diurnal period can be used to construct a neutral framework concerning the cardinal directions: at least these directions are natural in the sense that they are not manmade and are perceived and acknowledged by human beings in all different cultures.'

One such culture is that of the Balinese. On Lombok, the three positions of the sun are perceived and acknowledged by the people. Associations are indeed made between the positions of the rising sun and of the setting sun. The cardinal points are also constantly referred to, directly and indirectly, by analogy, but unfortunately the 'fourth position' is approximately north (*kaja/kaler*) in Balinese ideology. There is also no position where the sun never comes in Balinese ideology, apart from 'hell', the realm of *aśurya*. *Aśurya* and Pinxten's fourth position are quite different, I think. But in any case one does not need the UFOR to help one to ascertain these crucial points in Balinese thought.

Let us take 'Left/Right', entry 119: 'phenomena (with spatial relevance)x and y are recognized to be on the left or right side of one another (x is to the left of y, and then y is to the right of x), provided they are perceived or acted upon as distinct from each other, and provided they are handled as being in a spatial relationship to one another that is analogous to that of the dominant or preferred hand (or the other way around, depending on the cultural information available).' Pinxten then notes that 'the universal fact of the neurological difference of one hemisphere over the other...may suffice for the UFOR, but one should allow for culturally different interpretations of this fact.'

And so on. This reviewer must be candid and admit that he will stick with the classical ethnographical methods, although they are methodologically rather vague, in preference to this kind of formulation which, like so much theory, tends to make matters worse rather than better in trying to get to grips with one's data.

This preference is, of course, a matter of taste. It is his taste, also, which allows the reviewer to report to readers of *JASO* that *Anthropology of Space* is a serious book, whose leading author is both serious and concerned. Chapter 5, 'Applications in the Teaching of Mathematics and the Sciences', takes the view that the results of teaching young Navajo in a way which presupposes the 'pre-school knowledge' which Western children, according to such writers as Piaget and Bruner, possess, are ineffectual and totally inappropriate. That is, the ineffectualness and inappropriateness derive from 'the fundamental and poorly understood lack of commensurability of Western and Navajo knowledge systems'. This situation can be remedied, Pinxten thinks, by exploring and utilising in teaching

the differences and the similarities between Navajo and Western concepts. This approach would be to the benefit of the Navajo: 'nobody deserves this second-rate treatment in a democratic society', i.e. living in a divided world, partly Navajo and partly Western. It would be epistemologically useful: 'it is fascinating and theoretically rewarding to try to work out alternatives to the historically and culturally specific outlook that predominates nowadays.' Finally, there is the evolutionary reason: 'as long as science cannot pretend to have valid answers to all basic questions (as is the case in our contemporary situation...), it is foolish to exterminate all other, so-called primitive, pre-scientific, or otherwise foreign approaches to world questions', even if one does not go quite so far as Joseph Needham went in 1956.

To a non-specialist, this book seems to be one which would appeal to anyone interested in such peoples as the Navajo; it presents a methodological approach which is interesting and challenging, even for those who decide not to adopt it; it raises questions which might be useful in field research; and it contains an element which will reassure anyone who thinks that questions which have a political content are not necessarily to be eschewed in the work of social anthropologists merely because it aims to be scholarly. *Anthropology of Space* is a work of serious scholarship which raises questions which are interesting, which might be useful, and which are morally significant.

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JERRY W. LEACH and EDMUND LEACH (eds.), *The Kula: New Perspectives on Massim Exchange*, Cambridge etc.: Cambridge University Press 1983. xi, 538pp., Bibliography, Illustrations, Maps, Index. £47.50/\$79.00.

Since the publication of *Argonauts of the Western Pacific* in 1922, Malinowski's description of the *kula* has become part of the culture that social anthropologists share. This volume of twenty-two essays grew out of a 1978 conference organized for the purpose of reassessing Malinowski's study and constructing a comprehensive description of the contemporary *kula* exchange system. Fifteen of the twenty contributors have conducted recent fieldwork in the Massim - the cultural region encompassing islands east of Mainland Papua New Guinea - and it is primarily their presentation of new ethnographic material that updates Malinowski's *Argonauts*. Accordingly, the papers are

grouped by geographical location, beginning with the Trobriand *kula* area and proceeding, like the famous shell necklaces, clockwise around the ring. Two ethnographic sections deal with island communities bordering this area. In addition, several papers that consider *kula* exchange from a theoretical perspective supplement the new ethnography. The volume concludes with a stimulating essay in which Edmund Leach questions whether 'a synthetic totality THE KULA can be pieced together from components derived from reports from different islands, even when ...the islands are immediately adjacent.'

In an introduction that critically reviews the received interpretations of *kula* exchange, Jerry Leach, the principal editor, notes one of the persistent questions of *kula* analysis: how does the *kula* work? The greatest strength of this volume lies in supplying information fundamental to understanding this question. Campbell's two papers on *kula* in Vakuta, southernmost of the Trobriands, offer important details of *kula* transactions. Campbell explicates the shell classification system - that is, the criteria by which Vakutans place armshells and necklaces into ranked categories. The discussion, moreover, outlines a number of topics elaborated in other papers: the initiation and maintenance of *keda*, the 'paths' which link partners in the exchange of shell valuables and furnish participants with a conceptual model of the exchange system; the strategies men may exercise in diverting shells from one *keda* to another; and the political processes entailed in building a *kula* career. Munn, for example, concentrates on modes of persuasion and strategy in her analysis of Gawan *kula* as an ongoing process of influence-building in which control over the movement of shells generates 'a symbolic value attribute of actor identity' - *butu* or 'fame'. Munn, Campbell and Damon illustrate their papers with actual case histories of transactions, developing an overall picture quite different from Malinowski's portrayal of partners fixed in a lifelong relationship. Men dissolve partnerships, establish new *keda* or reinstate old relationships in response to diverse opportunities for enhancing their 'names'. Indeed, keeping a *kula* relationship 'alive' is problematic, not given, as every repayment of a debt potentially ends the *keda* partnership.

The strategies that actors employ in 'playing' *kula* imply that, contrary to Malinowski, valuables do stop in their circulation around the ring; transactions, like *keda*, begin and end with someone. *Kitoms*, shells over which individuals exercise absolute proprietary rights, enable men to create breaks in the cycle of indebtedness. Malinowski, who never spoke of *kitoms*, offered *kula* as an example of a 'new type of ownership', shells being held in trust and only temporarily. The majority of the authors in this volume, however, demonstrate how individually-owned *kitoms* variously work in Massim societies. Damon, for example, in interpreting the claim of his Muyuwan informants that every shell is someone's *kitom*, proposes that: 'At a certain level of abstraction the *kula* ring consists of individual owners of *kitoms* exchanging these articles back and

forth.' *Kitoms* account not only for how one begins a cycle in the *kula* unencumbered by past debt, but also, according to Damon, for the principle of equivalence that ideally guides all exchanges. Damon furthermore discusses the convertibility of *kitoms* in paying debts for canoes and pigs as well as for the 'work' a woman provides her husband during her lifetime. Similarly, Thune and Macintyre, in papers dealing with the southern *kula* area, emphasize the importance of *kitoms* in a wide range of contexts. Thune describes the paramount significance of *kitoms* in inter-matrilineage exchanges in the Duau area of the Normanby Islands, while Macintyre reports five major exchanges involving *kitoms* on Tubetube, including mortuary, compensation and land payments. The arrangement of papers thus makes it clear that as one moves into the southern Massim, *kula* becomes less differentiated conceptually and practically from other exchanges. Indeed, Thune notes that no word in the Loboda language refers to *kula* alone. Tubetube people likewise regard *kune (kula)* as only one way of gaining wealth and prestige to use within the community. *Kitom* convertibility, then, provides a key to understanding both how other exchanges function as sources for new *kula* shells and what motivates individuals to place shells in and remove them from circulation. More generally, the new ethnography corrects the view of *kula* from Kiriwina where (as Weiner observes in a paper that examines *kula* in relation to intra-community exchange) it constitutes an almost autonomous realm of activity.

While the papers that treat *kula* and related exchanges as specific forms of transaction comprise the heart of the volume, several contributions investigate the symbolism and meaning of *kula*. Tambiah analyzes *kula* mythology in order to reveal a Trobriand code for male and female values. Young's exegesis of the Kasabwaybwayreta myth touches upon similar themes, including the opposition between stasis and mobility that characterizes male/female relations and Massim ideology in general. Munn's analysis of the symbolism of influence relates this opposition directly to the movement of shells. Nevertheless, the volume on the whole is less satisfying with regard to symbolism, offering preliminary suggestions (such as Scoditti's claim that '*Kula* expeditions are at one level symbolic representations of the life cycle and behaviour of a butterfly'), rather than systematic interpretations. Likewise, though with some exceptions (Irwin, Berde), the papers are generally weak both in locating *kula* exchange in a precise historical context and in assessing the impact on *kula* of political and technological changes in the colonial and post-colonial periods. (However, papers by four of the contributors in a recent issue of the *Journal of Pacific History* partially remedy this shortcoming.)

The sections dealing with Massim communities not directly involved in *kula* exchange are quite valuable. Young and Chowning present materials from D'Entrecasteaux societies on exchange institutions analogous to *kula* in political and economic functions. Four papers discuss systems of exchange found in the

Louisiade Archipelago societies of the southeastern Massim. Liep takes up the question of 'Rossel Island money' in light of his field experience with Rossel (Yela) exchanges. Berde, Battaglia and Lepowsky all discuss the overall patterning of exchange in the archipelago, stressing the critical role of mortuary feasts in organizing virtually all production and exchange. The political aspects of sponsoring mortuary ceremonies blur the distinction between 'internal' (intra-community) and 'external' (inter-community) exchange that characterizes to various degrees *kula* areas to the north and west. In fact, both Battaglia and Berde suggest that this merging of internal and external realms is the definitive feature of Louisiade exchanges. The inclusion of D'Entrecasteaux and Louisiade material - some being made available for the first time - thus provides a comparative context for the new ethnography. In this context, the distinctive features of *kula* exchange are highlighted while at the same time the features *kula* shares with other forms of exchange are identified.

Taken together, the papers in this volume define the Massim culture area through the analysis of recurrent ideological themes and cognate political and economic processes. Excellent maps, an index of place names and a separately published bibliography (Martha Macintyre, ed.) with over 600 entries on *kula* support the effort. The volume thus provides a basis for fine-grained comparative analyses of *kula* exchange within the Massim, rather than replacing Malinowski's model with a new alternative. Indeed, the new ethnography reveals significant differences around the ring over the use of shells in non-*kula* contexts, the gender identifications attributed to armshells and necklaces, the right to participate in *kula* exchange (J. Leach) and informants' statements about the criteria of shell classification. E. Leach rightly argues in his conclusion that 'a synthetic totality THE KULA' cannot be constructed out of these variations. Nonetheless, as Leach recognizes, analytical comparisons of the principles underlying different forms of exchange are both possible and needed. A. Strathern presents such a comparison of *kula* with other exchange systems operating in Papua New Guinea societies (Tolai, Siassi, Enga, Melpa). On a different scale, Firth attempts to view *kula* as 'a set of macro-economic relations involving price-making mechanisms'. Gregory, in contrast, maintains a strict dichotomy in his comparison of the respective principles governing *kula* exchange and capitalist commodity exchange. Finally, E. Leach calls for a comparative approach to *kula* exchange in line with Lévi-Strauss' categories of 'restricted' and 'generalised' exchange. Ultimately, the comparative exercises made possible by and tentatively offered in this volume will diminish much of the novelty that Malinowski ascribed to *kula*. The result, however, should be a sharpened appreciation for the historical and cultural specificity of *kula* exchange.

ROBERT J. FOSTER

PAT HOLDEN (ed.), *Women's Religious Experience*, London & Canberra: Croom Helm and Totowa, New Jersey: Barnes & Noble 1983. vii, 192 pp., Index. £13.95 (Cloth), £6.95 (Paper).

This collection of papers, based on a series of seminars organized by the Oxford University Women's Studies Committee, addresses the question of 'how women perceive themselves and their roles within varying religious systems'. It is thus not so much about religious experience in the Jamesian sense as about female experiences of male-dominated religious or symbolic systems. Quoting from the last paper of the volume, the editor states that a common theme is that 'women do not challenge the dominant model but rather elaborate on an element of it.... Religion may appear to repress women and to justify their subservience to men. The papers show that women do not generally challenge this; neither, however, do they necessarily share the view.' The papers are arranged in pairs; two are on Victorian England (Spiritualism and Theosophy), two on the Mediterranean (village Greece and Turkish towns), two on India (ancient texts and contemporary village life), two on Judaism, and two on Africa (West Africa and the Nyole of Uganda).

Vieda Skultans tells the amusing but sad story of the relationship between Spiritualist mediums (typically younger women in precarious financial circumstances) and psychical researchers (typically well-to-do, middle-aged men). What intrigued me in the account of Spiritualism (which Skultans is reluctant to see as a 'racket') was that because of its quasi-scientific and quasi-religious character it could be used by the men as a device for acting out their erotic fantasies rather openly without doing damage to their reputation, or to that of the medium, unless someone showed the poor taste of exposing the materializations as frauds. If Spiritualism 'may appear to repress women and to justify their subservience to men', the paper shows that the mediums did not generally challenge this, and that Skultans does not necessarily share the view; on the contrary, one of her conclusions is that Spiritualism 'provided ideal career opportunities for women', even though she goes on to show that in the long run the prospects for a medium were about as promising as those of an ordinary prostitute.

Diana Burfield uses a biographical approach in her well-written and informative account of the role of a few women in the Theosophical Society, and she places that movement in the historical context of other similar movements of 'advanced thought' in the 1880s. In passing she makes the observation that '1888 was as exciting and tumultuous a year as 1968, and considerably closer to being a genuinely revolutionary situation', and one is struck by the resemblance between the two periods in terms of the political and ideological ecumenism of the 'New' social and philosophical thinking, which allowed for all sorts of blends of socialism, anarchism, oriental mysticism and the occult.

Lucy Rushton's paper is a series of ethnographic observations on the daily life of women in a Macedonian village and on the way they perceive their position with respect to official theology. Concepts like 'honour' and 'shame' are not invoked (and indeed these women appear refreshingly 'shameless'), but some concerted analytic effort would not necessarily have detracted from the value of the author's observations.

Analytic competence is demonstrated in Nancy Tapper's paper. By looking at the structure and content of two kinds of women's gatherings, one secular and one religious, and by placing them in their total context of the social and religious life of the Turkish provincial town, Tapper shows why the contradiction between male dominance and the highly valued status of motherhood cannot be consciously confronted, let alone symbolically resolved or mediated, for the gatherings serve, among other things, both to cement and to obscure the contradiction.

In her paper on ancient Indian religious texts, Julia Leslie is concerned to find out what religious opportunities were open to women at various periods, and whether the difference in sex made a difference in respect to access to religious or philosophical knowledge. Leslie has looked for women in the texts and has found some. We are not told whether the character and varying frequency of female appearances in the texts are related to what went on in the society of the different periods.

Given that, in contemporary Hinduism, religious worship is determined by the degree of purity of the individual, and given that women are considered relatively impure in relation to men, why is it the case in a village in central India that women worship the gods on a greater number of occasions than do men? This is the question which Catherine Thompson confronts, and indeed answers, in her paper. She argues for a more nuanced view of purity and impurity, one which is bound up with ideas of power and powers. Aspects of female sexuality are certainly polluting (menstruation, childbirth), but 'a woman's mature sexuality is...made auspicious once she becomes a wife. As a wife she is under the control of a man.' So part of the answer lies in the 'Turkish' contradiction of valued motherhood and male domination. But unlike their Turkish sisters, the Indian women are not symbolically paralyzed by the contradiction; they may actually mediate it themselves, Thompson argues, precisely because of the nature of the ideas of purity and impurity.

The two Jewish papers, by Julia Neuberger and Jonathan Webber, are both apologetic for official Judaism with respect to the position of women. Rabbi Neuberger points out that women are not excluded but only exempted from taking part in public religious life, so if they do not participate, it is largely their own fault. Webber argues that it is perfectly feasible for a woman to be both orthodox and a public figure, for the traditional Jewish ideology, codified in the law, offers women 'both the cultural security and, at the same time, the

cultural freedom not to feel it necessary to rise up against it in their voluntary participation in communal life'. Such is the power of ideology.

Elisabeth Tonkin deals with masking in West Africa. She points out that a mask is not just the physical object; it also includes the carrier of the mask and the action that he performs. Masks are connected with power, and a mask event 'frequently defines, creates or enacts "maleness" as the most socially marked state'. 'Masks then are a focus for pondering on sexual divisions and also for bounding or neutralising the powers of women which, simultaneously, they present.' The female element which Tonkin elaborates on is that if women are excluded by mask events, they should consequently be ideologically free to reject the male message of the mask.

In her paper on explanations of misfortune among the Nyole, Susan Whyte deals with a 'classical' African system of representing physical disorders in the idiom of social relationships. But in some cases women suffer misfortunes which are caused by totally unrelated, 'little' or 'foreign' spirits. This Whyte takes to be an indication of the existence of a 'counterpart' female model which represents women as individual persons, not defined primarily by their dependence on men, as wives and daughters. But the point is that this 'counterpart' model is in itself determined by the dominant male model, and this leads to an important discussion of the nature of the relationship between male and female models, a discussion which, as the editor pointed out in the Introduction, is actually centrally relevant to the other papers as well.

The papers are of uneven quality. The best - those by Tapper, Thompson, Tonkin and Whyte - are fine anthropological analyses which focus on rather restricted phenomena in order to make general points about female reactions to male-dominated symbolic systems. They may thus be seen as falling within the tradition of 'women's studies' in social anthropology which was inaugurated in the mid 70s by Shirley Ardener's volume *Perceiving Women*. But unlike that volume, the editor of the present one has not sought to provide a more or less unifying theoretical framework for the different contributions. Even the problematic character of the term 'religious experience', though it is briefly acknowledged, is glossed over by reference to the diversity of the material. Pat Holden feels that the papers contain 'distinct indications of new approaches to the study of women and religion which should provide important pointers to future studies', but she leaves it to the reader to find out what they are and ends her Introduction with the ascertainment that 'there is an immense and rich variety of female religious experience'.

On the technical production of the book, the less said the better.

ABDELALI BENTAHILA, *Language Attitudes Among Arabic-French Bilinguals in Morocco*, Clevedon: Multilingual Matters 1983 [Multilingual Matters 4]. x, 168pp., Bibliography, Indexes. £9.90.

This study makes a valuable contribution to the growing social science literature during the last two decades concerning the complex linguistic problems faced by the newly-independent nation-states of Asia and Africa. The issues involve determining the choices and functions of language(s) toward national integration both from the ideological and practical viewpoints. Formation of appropriate policies and their implementation as part of language planning becomes a major task and requires the collaboration of linguists, educationists, administrators, and other experts. The task is even more formidable and the solutions more difficult to come by if the new nation-states are multilingual.

Since attitudes towards language(s) play a major role in the formulation and implementation of national language policies and in their acceptance by the population at large, a systematic study of language attitudes in any specific nation-state is a prerequisite for adequately developing its language policies and planning for the fruitful and consensual use of language(s) in all domains. This study fulfils such a task for Morocco.

The linguistic situation in Morocco is complex because of the existence of four languages: Berber with its several widely differing and not always mutually comprehensible varieties; Moroccan Arabic learned by all Arabic speakers as their first language; Classical Arabic which is no-one's first language; and French, the language of the colonial rulers. Berber has no written form, and Berber speakers adopt Berber-Arabic bilingualism routinely to facilitate everyday exchange and communication. Classical Arabic was, and continues to be, the language of the traditional education system, while French has been the main language of instruction in the three types of school established by the French. Bentahila has chosen to study only one aspect of this complex linguistic situation, but one that is very significant. He explores the attitudes of Arabic-French bilinguals concerning a variety of language-related issues. These bilinguals were selected because they represent 'the majority of the educated younger generation' and thus are in a position to influence the future of national integration and progress in Morocco. Bentahila's methodology involves using various types of questionnaires and tests administered to many randomly selected samples of Arabic-French bilinguals.

After an introduction which outlines the language situation in Morocco, chapter two briefly presents the various theoretical and analytical approaches used by linguists and other social scientists to study bilingualism. Chapters three to six constitute the core of the book and explore several aspects of the Arabic-French bilingual community and its language attitudes.

They all have a similar organization. In each, the introductory section discusses some key issues based on a brief survey of the relevant literature, and is followed by a section describing the various tests administered by the author and their purpose. The results of the tests are analysed and discussed, and conclusions are drawn. The final chapter summarizes the major findings of the research.

Chapter three is devoted to testing the bilingual individual's attitudes towards his language, his code-switching abilities between Arabic and French, and the relationship between his language use and his world-view. The analysis of the results reveals some interesting trends. While Moroccan Arabic is seen as the easiest to learn and the most practical in everyday life, French is seen as most modern and practical in such domains as education and administration. Classical Arabic is seen as beautiful and rich. Most respondents seem to value each language for its own sake, despite the overall bias towards French. Although the bilingual speakers frequently code-switch between Arabic and French, many view such a practice negatively and few admit to doing it themselves. As for the bilingual's world-view, his 'attitude to the world varies, depending on which language he is using'. Bentahila believes that his tests constitute evidence to support the view that 'language, as one part of the society's culture, will as such naturally be linked with other aspects of that culture'. Thus, 'French language is closely linked to other things French, and in turning to the language [the bilingual] also turns to the culture.'

Chapter four is primarily concerned with discovering the types of factors which may affect the choice of Arabic or French by the bilinguals, and with discussing them in the context of attitudes examined in the previous chapter. It appears that the bilingual's choice of language in any particular situation is not arbitrary, but is influenced by many factors, the most crucial of which are interlocutor, setting, topic discussed, and speaker's intent and mood. Overall, Arabic seems to be the language of the home while in the public domain French is preferred in communicating with educated individuals in prestige professions, and Arabic is used with strangers and low-level professionals. Informal situations generally seem conducive to a mixture of Arabic and French, in contrast to formal ones. Topics such as philosophy and religion evoke the use of Arabic, which is also used for such activities as telling jokes, insulting, greeting - or when speakers are tired or angry. This last fact demonstrates the speakers' emotional attachment to their native language. French is used to discuss science, technology, industry, social sciences, etc. It is also used for courting, and for maintaining social distance and impersonal relations. Books, newspapers, radio broadcasts and films in French are more popular and are preferred to those in Arabic though in practice both languages seem to be even in this respect. In the domain of education, there is an overall preference for French. In general, this preference is based on practical considerations.

However, Bentahila notes that each language is associated with different domains, has its own role in the bilingual's speech behaviour, and cannot be excluded in social interaction. He cautions language planners to be aware of this reality.

Chapter five is aimed at discovering the Moroccan bilinguals' attitudes toward those who speak French or Arabic, and those who code-switch between them. The results of the match-guised tests reveal attitudes similar to those overtly expressed by the bilingual respondents. For instance, those who mix two languages are perceived as inferior in status, personality, and manner of speaking. It appears that the Moroccan bilinguals' judgments of a person are quite radically influenced by what he speaks, and that they may gain significantly different impressions of a person's character, status and level of education according to whether he is using French, Arabic, or a mixture of the two'. While a French speaker is perceived more favourably than an Arabic speaker, test results also show a bias against those who speak accented French.

Chapter six starts with a general discussion of the problems involved in language planning in Morocco. It is argued that since Classical Arabic is no-one's mother tongue, ideas of replacing French by Classical Arabic amount to 'replacing one non-native language with another'. This is indeed a significant observation. The drawbacks commonly associated with Arabic, such as its unwieldy writing system, its lack of punctuation, problems of printing, its inadequacy as a mode of scientific writing, and its inability in fulfilling the needs of contemporary Morocco, are viewed by Bentahila as not so much due to the nature of the language itself, but rather as emanating from the Arabic speakers' attitudes towards their language. According to him, part of the blame also lies in the ways in which Arabic has traditionally been taught, making it a static language not to be polluted by borrowing from other languages. Bentahila believes that with proper planning Arabic can indeed become a national language.

As in other chapters, the results of various tests designed to discover Moroccan bilinguals' attitudes towards the present and future language situations are analysed and discussed. Bentahila admits that although the bilinguals' views on the issues pertaining to language planning are important, they certainly are not representative of the Moroccan population as a whole.

Moroccan bilinguals recognise that the advantages of bilingualism outweigh the disadvantages. Most of them feel that French should be used as a medium of instruction at primary, secondary, or higher education. This appears in conflict with the existing ideals of the policy-makers. Most also prefer French for such subjects as mathematics and science and do not favour the use of Arabic in its place.

The emotional conflict Moroccan bilinguals face between ideology and practicality is quite obvious from these results. While they think that ideally Arabic should be the language used

for all purposes including education in science, they realize the immense practical advantages of French. Bentahila forcefully argues that language planners in Morocco should not sacrifice practical values for the sake of ideals. He feels that the task of 'running the country and leading it into the next century' is likely to fall on the educated bilinguals, and 'their feelings about the present and possible future language situation must not be ignored'. At the same time Bentahila makes several practical suggestions for persuading Moroccan bilinguals that Arabic is an adequate replacement for French. He feels however, that the question of Arabization is tied to the co-existence of two divergent varieties of Arabic - Classical and Moroccan - and also to the very negative attitudes towards the latter which is viewed as a language of the ignorant and illiterate people. Both these problems need to be resolved before Arabization can be successful.

This study should go a long way in convincing language planners and policy-makers, especially those in Morocco, that attitudes towards various languages involved in policy decisions play a key role in the success or failure of any language planning. Bentahila is not only familiar with the existing literature on language planning, but has also given considerable thought to the specific problems of language planning in Morocco. The study is methodologically sound and the results are significant. Thus not only should its conclusions prove useful to language planners in Morocco, but it could be used as a model by language planners in other Asian and African nations to discover the nature of language attitudes in their respective countries in guiding their policy decisions.

MAHADEV L. APTE

SCOTT WHITEFORD, *Workers from the North: Plantations, Bolivian Labor and the City in Northwest Argentina*, Austin: University of Texas Press 1981 [Latin American Monographs no. 54]. xi, 155pp., Bibliography, Index, Maps, Tables. £16.25.

Whiteford's book sets out to describe and analyse the employment structure of Bolivian migrants in N.W. Argentina and its implications for the personal and social lives of the migrants themselves. In this it represents a useful contribution to the growing literature linking the macro-level of Third-World capitalist development to the micro-level of individual lives, of daily decision-making and the unrelenting struggle simply to 'make ends meet'. One senses throughout the author's recognition

of the essentially political nature of his ostensibly sociological field. Capital dictates terms to labour and it is the task of the researcher to put flesh onto the bare bones of that axiom by discovering the social relations that allow the process to continue.

The book's best section is undoubtedly chapters 2 to 5. Here, Whiteford takes us through three descending levels of his complex subject using direct and clear prose. Chapter 2 places the 'sucking-in' of Bolivian labour to Argentina's northwestern provinces within the general framework of colonial and post-colonial economic development. The involvement of Argentina's indigenous Andean and Chaco populations in this story reminds the reader that migration in this area is not solely a trans-national affair.

In chapter 3 the focus is narrowed to the sugar zone itself and the complex labour requirements of an area in which varying ecological conditions mean that different crops in different places need intense labour inputs at different times of the year. The traditional image of *golondrina* labour gradually moving south with the southern summer is replaced with a much less simple picture of an almost frenetic scramble for work as different crops ripen and employers enter the labour market in intense but limited bursts. Much of the book, indeed, is devoted to the flow of employment information as a vital economic resource among migrants. Relationships through which advanced knowledge of employment opportunities is communicated are salient parts of migrant social structure.

Chapters 4 and 5 deal with the methods of recruitment to the sugar harvest itself and the allocation of work and payments within the plantations. The relationship between the *contratista* (agent), *mayordomo* (foreman), and *zafretero* (harvest worker) and the nuances of clientism are clearly explained. The *cuarta* system of subcontracting work within kin or friendship networks, however, does not seem to be given sufficient attention. As Bromley and Gerry (1979) are cited in both text and bibliography, it seems odd that the mechanisms by which the sugar companies obtain workers rather than employees (cf. Birkbeck in that volume) are not further pursued.

The remainder of the book is largely devoted to the strategies developed by migrant workers for maximising the opportunity to earn wages so as to introduce an element of security into their lives. The exposition of the role of different migration patterns and the central importance of the city in the lives of migrants shows the results of extensive data collection and careful analysis. The influence of Ortiz's work on peasant decision-making is clearly present in the elaboration of the concept of the 'strategy of least vulnerability' and in the general tone of the analysis.

However, two conceptual areas - kinship and ethnicity - are not given the detailed treatment they deserve as instrumental factors in economic life. Throughout the book Whiteford refers to 'the family' or 'the extended family'. Unfortunately no

serious attempt is made to examine the nature of the kinship bonds that are evidently of great importance to migrant life. The norms of marriage, child rearing, siblinghood and kindred association are never fully explored, yet 'family' links are mentioned time and again with reference to economic activity. Similarly the role of money within kin groups is clearly central to 'family' co-operation in a context where husbands, wives and children are all potential or actual members of the labour force.

Ethnicity is dealt with in a similarly unproblematic fashion. While the antagonism between unionised Argentine labour and non-unionised (and even illegal) Bolivian labour is mentioned, the stigma attached to Bolivian identity in modern Argentina is skated over. The 'fiesta' complex with its bringing together of solidary functions on the one hand and instrumental contacts on the other suggests a more complex role for ethnicity than Whiteford allows. The combination of ethnic stigma, scarce employment and the fact that many present-day 'Bolivian' migrants are Argentine-born would point to the existence of a crucial ethnic boundary. The maintenance and function of regional identity and general 'Bolivian-ness' could have been the subject of an illuminating chapter.

Scott Whiteford's book will be of interest not only to social scientists working in the field of migration but also to anyone with an interest in the mechanism of surplus labour exploitation. Not least, it is a contribution to the knowledge of modern Argentina, whose European immigrants have tended to attract the bulk of the historical and sociological attention paid to that country.

CHARLES DAVISON

ERNEST CASHMORE, *Black Sportsmen*, London etc.: Routledge and Kegan Paul 1982. xiv, 222pp., Bibliography, Index. £5.95.  
ERNEST CASHMORE and BARRY TROYNA (eds.), *Black Youth in Crisis*, London etc.: George Allen and Unwin 1982. 176pp., Bibliography, Index. £4.95.

Black teenagers squaring up to lines of riot police; gangs of black youths lurking in the shadows waiting to rob the unsuspecting passer-by; the dreadlocked mystic verbally abusing others in the street: such are the images of black youth which have developed in Britain over the last decade. Very few positive images of black youth exist despite the attention lavished upon this section of the population by the media and others during recent years. Indeed Cashmore and Troyna state

in their Introduction to *Black Youth in Crisis* that the term 'black youth' has 'become synonymous with young black males of West Indian descent'.

Social scientists are not altogether free from blame for the creation of this image, Cashmore included. There has been a tendency for scholars to concentrate upon the more extreme manifestations of black youth culture. In these two books, however, the author and contributors go some way to redressing the balance. In *Black Sportsmen* the ever-increasing contribution of black youth to the sporting world in British society is recorded. The contributors to *Black Youth in Crisis* attempt to provide a more sensible approach in looking at black youth than that which concentrates on extremes of behaviour, an approach that does not generalise at the expense of accuracy. Neither work entirely gets away from the view of black youth as male and of West Indian origin, but a start is made.

Cashmore addresses himself to the historical, social and psychological reasons for large numbers of blacks entering sport and the high quality of their achievement in Britain today. The book comprises twelve chapters which include nine profiles, including full-page pictures, of leading black British sportsmen. These profiles are the result of personal interviews with the author and provide the framework around which Cashmore weaves his argument. The profiles offer an interesting insight into the development, not only of sporting careers but also about perceptions of being 'black'. The profiles are of successful black sportsmen, and Cashmore's observations about the obstacles facing black youth in the educational and occupational world, as well as their family backgrounds, are based on the experiences of the successful. However, Cashmore recognises that these sportsmen are but a small proportion of the blacks that are taking up sport, and he takes care to outline the costs as well as the benefits for black males entering sport.

Cashmore rejects the notion that black males are born sportsmen or have natural ability. He sees sporting endeavour as offering black males an avenue of social advancement where other avenues are blocked. Black children are seen to fail at school as a result of lack of parental interest and the negative expectations of teachers who channel black children into sporting activities at the expense of their academic education. Despite the potential benefits of sporting activity Cashmore concludes that,

Sport conceals deep, structured inequalities and for all the positive benefits it yields, it remains a source of hope for blacks only as long as these inequalities remain.

While Cashmore's message is clear, his methodology is suspect. He relies heavily on the statements of his informants without presenting any other substantial empirical data to support his claims. He does not present the parents' or the teachers' view

of things, for example.

In *Black Sportsmen* Cashmore does not tackle the question of why certain sports, and even certain events and particular activities within the same sporting area, attract black youth more than other sports. This is not necessary to his argument. He does not attempt to define what he means by the term 'black'. He leaves this to our common-sense notions, and to a certain extent this works in the context of the book. The contributors to *Black Youth in Crisis* cannot afford this luxury. Rex points out that black youths come from different cultural environments into different cultural environments, and that responses can vary widely as a result. Responses should be linked to conditions 'on the ground'. In this way Asian youth can be brought into the picture, although Rex points out the difficulties in incorporating black females into any analysis.

Rex is joined by Fisher and Joshua in calling for more empirical research in this area. The latter contributors challenge the popular assumption, present within Cashmore's work, that there is a generation gap resulting in conflict between the black teenager and black parent. They point to the scarcity of empirical evidence to support this view of the immigrant generation as conformist in contrast to their children.

In all, *Black Youth in Crisis* contains nine chapters from a variety of contributors covering topics from Rastafarianism to the way in which social policy has exacerbated the crisis in which many black youths find themselves. However the most interesting contribution for me is that provided by Fuller. She describes the responses of a group of black school-girls in a London comprehensive school to the experience of both racial and sexual subordination. She gives the reader some insight into the way the category 'black' is being used sociologically. Quoting Miles and Panacklea she says that 'it is the unique experience of racial exclusion that is the essence of black ethnicity.' She claims that the black experience in a white-dominated society is similar to the female experience in a male-dominated society. The black girls at Torville School are conscious of both forms of domination, but surprisingly this double exclusion and domination gives rise to a positive sense of their worth. Not surprisingly they found difficulty in having this sense of worth acknowledged by others outside the group.

The recognition of the type of research needed in the future by the contributors to *Black Youth in Crisis* and epitomised by Fuller's work bodes well for ethnography. The detailed case study is unanimously recognised as the way forward. While holding reservations about Cashmore's work, I would recommend both books in that they open up new lines of inquiry, and in doing so expose many assumptions about black youth which have little empirical basis.

BILL HOLM, *Smoky-Top: The Art and Times of Willie Seaweed*, Seattle & London: University of Washington Press 1983. 174pp., Photographs, Appendix, Bibliography, Index. £21.20.

Holm has produced a book more on the times than on the life of Willie Seaweed and more an effective catalogue of his works than a monograph on the artist, his society, and the ritual objects he made. The accompanying text gives a personalistic and impressionistic account of the environment, which with the aid of excellent illustrations attempts to weave the reader into the world of Willie Seaweed.

Hitamas or Kwaxitola (Smoky-Top; anglicised name, Willie Seaweed) was, until his death in 1967, one of the most influential traditionalists who helped preserve and uphold the indigenous cultural traditions of the Canadian North-west peoples during the difficult years from the 1880s into the sixth decade of the present century. By the year of his birth, 1873, the densely populated Pacific cultures had already experienced substantial transformations as a result of contact with European merchants. However, as a result of the tortuous prolongations of the inlets jutting deep into the forest-clad mainlands, contact was largely limited to the outer coast of Vancouver Island and the smaller islands to the north-west. Nevertheless, by 1843 there was a string of seven forts pertaining to the Hudson Bay Company, each engaged in a bilateral trade, receiving furs from the local populations in exchange for woollen cloth, blankets, steel tools, muskets, and various forms of adornments. Some indigenous groups were attracted to the forts and consolidated extra-tribal groupings, better to obtain the advantages of their presence.

The introduction of new materials and better tools with which to fashion them resulted primarily in an extremely creative efflorescence of ritual objects with a concomitant exploration of style and form, which complemented fundamental indigenous values. The 19th century was also a time, however, when disease decimated large parts of the population, the small-pox epidemic of 1862 alone being responsible for the loss of perhaps one-third of the coastal population. The ravages caused by the epidemic were further augmented by the introduction of the ethnocidal laws of 1876 prohibiting the celebration of potlatch ceremonies and any form of dance. These laws held force for seventy-five years and were not repealed until the submission of a decree in 1951. One can surmise that whereas changes in territorial situation and material culture, with a complementary effect on artistic tradition, were proceeding well before the 1870s, it was only later that any concerted effort was mounted to diminish the importance of the core elements of the Pacific North-Western cultural area.

Hitamas, descended from high birth and through the traditional life passages of Kwakiutl society, was from the beginning placed in an ambiguous situation. His position as a chief,

which rested on the fulfilment of ceremonial obligations, clustered around the potlatch, and his inheritance of certain dance forms and accompanying masks from his successive marriages to the daughters of other chiefly lines, together placed him in an ambiguous situation in relation to the 1876 ordinance prohibiting the exercise of the manifestation of such standings. The consequent life of Hitamas is an exemplar of an Indian people's dignity in the face of provocation by the metropolitan society, and was in this case a life lived largely outside the boundaries of the law.

Holm provides a brief historical context in which he places the evolution of the artist's work, a work fastidiously inscribed within the traditional values of the Kwakiutl society in which he participated, and in which he fulfilled the obligations implied by his status, guiding his community to observe and uphold their complement of the same. Other than his achievements in formal oratory and dance, Hitamas was an extremely accomplished sculptor who exercised his prerogatives not only in reproducing the expression of ideological form, but in providing the apparatus on which the means of recreating the deeds of the ancestors and the stories of the spirit world depended, and therefore the gloss on which the hierarchical organisation of the society relied.

Hitamas was prolific as a sculptor and it must be said that it is to this aspect of his life that the book is dedicated. Holm has identified over 120 examples of his work which include masks, totem-poles, and wall-panels, but the contribution of the author to anthropology is that he is able to demonstrate effectively the nature of individual creativity within the constraints of plastic expression recognized by his society, and in consequence plot the boundary between individuality and its concomitant compromise with traditional form derived from the collectivity. With Hitamas, personal style is expressed in the form of strong geometrical designs and excessive formalism. This is particularly brought out in the carvings of rattles and head-dresses where his style contrasts with the generally flowing styles of his accomplices.

That Holm's book is more a catalogue of the artistic achievements of Hitamas than a biography is, moreover, confirmed by its entry as such under the Library of Congress classification number at the front, perhaps a little travesty on the part of the publishers. Ethnographic intent is minimal, though there is an ambitious if not always successful attempt to identify the person and function of the masks he has assembled. The monograph would have gained much if it had discussed the use of the ritual objects in their ceremonial integration in rites of transition connected with the dry-season and wet-season ceremonies, and their transmission through inheritance and the resulting prestige they confer on their receivers. Despite these reservations, Holm has presented a fortuitous monograph of the work of one artist which stands as a recognition and a tribute to the value of indigenous culture. Such a work implies

a growing consciousness of the indigenous cultural achievement and as such a stimulus to that tradition.

ANTHONY A. SHELTON

PARMANAND LAL and BIMAN KUMAR DAS GUPTA, *Lower Siang People: A Study in Ecology and Society*, Calcutta: Anthropological Survey of India 1979. 156pp., Bibliography, 8 Plates.

Anthropology is a well recognised activity in India, and there are several hundred professionals who are primarily social rather than biological anthropologists. However, it is the Indian sociologists who receive most of the international recognition, while the work of their tribalist colleagues is often ignored by academic journals. Part of the reason is no doubt the disappointing quality of so many of the publications, even from major organisations such as the A.S.I.

Siang is the name given to the Brahmaputra river as it pierces the Himalayas, so the tribals described here (mainly Gallong and Minyong) live about 100 miles N.W. of the Kachin. Field-work was conducted over some three months in 1968-9. Such a short period does not automatically condemn one to superficiality - Geoffrey Gorer's 1938 book on the Lepchas of Sikkim showed how much can be achieved in three months by a determined ethnographer armed with a theoretical orientation and two months' work on the language, and staying in a single village. But the present authors attempted to survey four villages (or five, or six - their statements vary), give no evidence of acquaintance with the published materials on the language, and have no clear aim apart from providing 'data'.

The reader is antagonised at once on finding that pp. 9-12 are borrowed without acknowledgement from Sachin Roy's incomparably better *Aspects of Padam - Minyong Culture* (1960: 12-16); the copying is largely verbatim, though careless. Pages 38-9 and a shorter passage on p. 82 come from the same authority. The plagiarism is probably the work of the first co-author, a geographer. Perhaps the practice is less poorly regarded in India than here - I have remarked on another example in the literature on the Byansi (adjacent to N.W. Nepal).

In so far as any attempt is made to relate the study to the previous literature it is half-hearted. The scanty bibliography omits, for instance, B.S. Guha's relevant booklet (n.d., probably 1966) on the youth dormitories of the area. The priest Magum Loya of Kabu village (p.116) is surely the Mogum Loya, one of whose chants was published in text and translation in an

admirable little book by the anthropologically untrained Tumpak Ete in 1974. No attempt is made to compare the Minyong kinship terminology with Roy's, though as the data is so lacunary (omitting *inter alia* MP, MBC, D, ZC) perhaps it would be unprofitable.

Even in the present economic climate some Indian publishers still set high standards (cf. for instance J.S. Lall (ed.), *The Himalaya: Aspects of Change*, from O.U.P. Delhi 1981). It may be unfair for native speakers to criticise the quality of non-natives' use of English, but the book's utility is much reduced by the lack of maps, glossary and index, and by the abysmal copy-editing and proof-reading. Within eight lines (p. 144) *dorrum Nisam* changes to *dorum nicam* and *silisiang* to *sllitaug*. No doubt some snippets of information are both new and reliable, but they are hard to identify in such a setting.

N. J. ALLEN

LAURENCE K. L. SIAW, *Chinese Society in Rural Malaysia: A Local History of the Chinese in Titi, Jelevu*, Kuala Lumpur, Malaysia: Oxford University Press 1983. viii, 197pp., Map, Photograph, Bibliography, Index. £17.50.

Laurence K. L. Siaw describes his book as a simplified version of a Ph. D. thesis, revised for the ordinary reader. What it offers is a chronologically arranged history of a Chinese immigrant settlement in the interior of Negri Sembilan over the period 1870-1960. Although derived from a thesis in a department of anthropology and sociology, in its published form the book might easily have been prepared as a thesis in history or political science. The only analytic idea mentioned comes from Max Weber and makes an incidental contribution to the argument. The book's particular interest is that it is an account in academically level and dispassionate tones of Chinese relationships in British Malaysia by a Chinese scholar. The activities of the Japanese occupying force during 1942-1945 are described with forthright attention to their effects on Chinese life and attitudes, particularly in relation to the cold-blooded Japanese massacre of the entire hamlet of Jelundong in March 1942. The Communists in their various transformations - from a largely Chinese political movement, to British-trained anti-Japanese guerrillas, to would-be post-war governors, to anti-British guerrillas under the suppression of the Emergency - are depicted with understanding, but without idealization. The British receive the same detached scrutiny, although Laurence Siaw's politeness combined with his determination to describe things as

he sees them produces some odd results, as in the following rather tortured sentence: 'Granted the noble motives of early British administrators in extending the Pax Britannica to troubled corners of the earth when invited to do so by warring native chiefs, the early history of Titi shows quite clearly that the imposition of British rule over Jelebu was of great economic benefit to the British themselves.' In Chinese eyes the British always played with a stacked deck, until they could play no more. The book however is marked by a large silence on one crucial topic, namely the place of the Chinese in Malay society and the Malay state. The index entry for Malays lists 'relations between the British and', 'attitudes towards life', and 'relations between the Japanese and', but does not mention Malay ties to the Chinese. The author is so discreet that if the reader had to rely on this book alone he would have no idea who replaced the British in the post-Independence government and might even get the impression that the Malays were a group of secondary importance in colonial Malaya. The book is thus incomplete. It is useful to those interested in both overseas Chinese life and in Malaysia, but it requires previous knowledge of both subjects.

R. H. BARNES

JAMES AXTELL (ed.), *The Indian Peoples of Eastern North America: A Documentary History of the Sexes*, New York and Oxford: Oxford University Press 1981. xxi, 227 pp., suggested readings. £7.50.

This is an introductory text for history students clearly intended as a supplementary text. The subjects of this work are various Indian groups who, in the last three centuries, occupied various parts of North America. These groups spread from James Bay in the north to the Gulf of Mexico in the south, the Mississippi River in the west to the Atlantic coast in the east.

Axtell focuses on the sex roles and life cycle of aboriginal North Americans. The text is divided into seven sections representing the life cycle. Each section consists of an introduction then a series of separately introduced excerpts from primary historical sources which deal with sex roles and the life cycle.

The primary historical sources used are from among the most widely cited and oft relied upon of historical sources which deal with North American Indians. The selected excerpts are generally short, clear and to the point. Axtell makes no claims of being authoritative, exhaustive, or analytical but openly admits that anthropologists may well find this work lacking.

In the author's own terms he has managed to produce an admirable supplementary text.

I don't, however, accept all of Axtell's assumptions and I do think that serious flaws mar this work. Rather than provide his audience with a wide range of primary historical sources, ten sources are repeatedly used to provide the bulk of sixty-seven excerpts. This over-emphasis on a small number of primary sources masks the wealth of published information available and distorts the authoritativeness of those sources selected. Sagard may be an excellent early 17th-century source for dealing with the Huron but so is Samuel de Champlain. I find it disconcerting that the former is repeatedly cited while the latter is ignored when both authors contributed equally valuable information to Axtell's choice of topics. When Axtell does stray from his main sources I am left with the unpleasant impression that much of what remains can be best described as filler.

The most serious problem with primary sources in this text is in its omissions. There are neither excerpts from archival sources nor references to the vast volumes of primary source material to be found in the archives of North America or Europe. The treatment of historical problems of interpretation is given light coverage. The cultural bias of early recorders is well presented but the cultural bias of the native population and its effect on the experiences of the early recorders is left unconsidered. Nor has the possibility been considered that many primary historical sources are but poor plagiarisms of earlier works on fiction concocted for the fame and fortune of the author. Any work that introduces primary historical sources to the student should clearly present the range of materials available and the labyrinth of errors to be encountered if primary sources are not treated with the utmost caution.

One final criticism worth making is, quite simply, that it is grossly misleading to suggest that historical works dealing with social and cultural questions should be exempt from anthropological criticism simply because they are historical. This neither promotes good scholarship nor an adequate understanding of either discipline's contributions to the resolution of mutual concerns.

In spite of these criticisms Axtell's book fills a gap in the student literature available on Native North Americans and this in itself reflects well on his efforts.

C.J. WHEELER

## OTHER NOTES AND NOTICES

### THE GAPP TRAINING WORKSHOPS: AN INTRODUCTION TO APPLIED ANTHROPOLOGY

#### *Introduction*

The Group for Anthropology in Policy and Practice (GAPP) is a non-official body, based in the United Kingdom, which was formed in April 1981 in response to the job famine for trained research anthropologists. According to its second newsletter:

GAPP has two main aims: to promote the active involvement of social anthropology in the making and implementation of policies, increasing the quality and quantity of that involvement; [and] to encourage the anthropological profession to take account of the experiences of this involvement in teaching and research, and [in] its main theoretical activities.<sup>1</sup>

Thus GAPP is not a job-finder or placement agency, but a forum promoting the idea of anthropologists working in non-academic situations, both among anthropologists themselves, and among their potential employers.

One of GAPP's activities towards this end - planned especially for the future - is a series of one-day workshops, held in various parts of the country. On Saturday 29 October 1983 I represented the Institute of Social Anthropology (University of Oxford) at one of these workshops, which was held at the London School of Economics, as a follow-up to a similar one of an experimental nature conducted previously at Sussex University. The registration fee was £2, though current SSRC students were able to recoup this cost, as well as travel and subsistence costs, from the SSRC.

The aim of these workshops is to simulate certain non-academic situations in which an anthropologist might work. In the one I attended, six fictional contexts were provided initially:

*Education.* The Birchester Education Authority is planning to close down a number of secondary schools. Such is the uproar when they decide to close the Sir Ezra Simon School which serves a largely Asian immigrant community that the Authority decides to conduct an investigation.

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<sup>1</sup> 'Aims, Plans, Policies', August 1982, p.1.

*Health.* A consultant in gastro-enterology is concerned about the numbers of alcoholics occupying beds in establishments provided by the Bloomsberry Inner City Health District since there is nowhere to refer the patients. A study of alcoholism treatment services in the area is required.

*Industry.* Chipo Ltd is trying to rationalise its operations in order to increase profitability but has run into problems of strikes. Because the labour force is largely immigrant the directors feel that their shop floor managers and the Union representatives are not communicating and a study by someone who knows about immigrants is required.

*Overseas Development.* The Atlantis Development Bank has put out for tender a study of energy utilisation in Zingali in order to construct an energy plan for the country. Of the six large firms which are tendering, two feel that the social aspects are crucial and are putting their proposals accordingly.

*Social Services.* In a rapidly urbanising part of rural Turnshire there have been a number of problems with adolescents. Turnshire Social Services would like to undertake some preventative work with these children and the first step is considered to be a social study of the community.

*Urban Planning.* The Ditchback Deprivation Project has been commissioned to find out the needs of the Ditchback area and how these might be met by voluntary initiatives fostered by the Community Council. Other commitments of the Council have to be borne in mind.<sup>2</sup>

It should be emphasized that, except perhaps in the development option, these were regarded not as jobs for an anthropologist specifically, but as jobs for social scientists that an anthropologist could do.

The workshop was attended by over fifty students and departmental representatives - in the ratio of perhaps five to one - who were divided into groups of about five to seven, according to choice of subject. However, presumably because of their popularity, the development and social services sections were divided into two groups, while for lack of support the industry section was dropped altogether. Otherwise, each option had just one group, under the guidance of a GAPP anthropologist experienced in that particular field. Departmental representatives were given the opportunity to circulate and observe all the groups at will, or to take part in just one group; most,

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<sup>2</sup> Extract from 'Outline of the Workshop' (workshop information sheet), GAPP, unpublished. Quoted by kind permission.

Including myself, seem to have opted for the latter, in the belief that this would be more instructive as to the methods used by the workshop, and accordingly I participated in one of the overseas development groups.

#### *Plan of the Workshop*

Each group collectively played the part of 'the anthropologist' working either alone, or as part of a team of specialists in various technical, economic, etc., fields on the social aspects of a particular problem/case study. Apart from the introduction, the day was divided into five sessions. The first was devoted to establishing the anthropological content of the project, and the problems the anthropologist was expected to research, in the context of the employer's terms of reference for the particular project. The second was devoted to methods (fieldwork, library research, use of informants, etc.), establishing a timetable and budget, recruitment of local staff, etc. The third session was concerned to draw up concrete proposals for research to present to the customer, whose 'representative' then considered the report in the fourth session. In the fifth session all the different groups came together to report on their case studies, and common problems and feelings were discussed. Evaluation forms were completed (anonymously) by all participants (including GAPP staff) before dispersal at six p.m.

#### *The Development Workshop - Formulating the Proposals*

Since in the development case there were two groups, an added element of authenticity came from the feeling that both were competing for the tender being offered by the Development Bank that was funding the project. Here, the anthropologist was the lowest-paid member of a consultancy team that included two foresters, two engineers, an economist, a co-ordinator, and a director responsible on a part-time basis for the project; the consultancy firm was one that regarded the social aspects of its development work as important, and stressed this fact to staff and customers alike.

The subject of the case study was a fictitious Third World country divided ecologically into three zones (a desert coastal strip, heavily urbanized and developed, and with most of the population; a hill area, with pastoralism and small-scale agriculture; and an inland tropical forest, little penetrated, save by a few hunting and gathering groups, and with a little subsistence agriculture); only a bare minimum of information was available, however, leaving the team with a wide range of possibilities to take into account. The project as a whole was a study of rural energy utilization and future requirements, and

the anthropologist - chosen in part as one of the few people who had studied the language - was expected to comment on the existing social constraints that might inhibit popular acceptance of changes in rural energy provision, and the future social implications of such changes.

Throughout these sessions the emphasis was very much on the customer's requirements, and on tailoring the tender proposals to meet them. This had two particular results for the anthropologist. First, there was a tendency, at least initially, for one's attention to wander into consideration of technical and economic problems - essentially the briefs of other experts - rather than concentrating on social implications. For example, rather a lot of time was devoted to the technicalities of fuel alternatives rather than simply establishing them as options. Secondly, any moral or ethical misgivings that the anthropologist might have had concerning his part in the project - which would inevitably disturb traditional ways of life - was lost in the rush to present an acceptable, competitive tender to the customer. For instance, the concern expressed for the fate of the hunting and gathering groups, faced with the possible loss of their forests through commercial exploitation for fuel and timber, was brushed aside by the GAPP expert, who frequently guillotined discussion of such irrelevancies in order to keep to the timetable. By the third session, we were discussing the prospect of over-turning existing relationships with perfect equanimity, to give but one example. (In fairness, however, it should be pointed out that the other study groups tended to discuss questions of principle rather more extensively.)

#### *The Development Workshop - Presenting the Proposals*

Equally instructive were the reactions of the 'representative' of the Development Bank to the tenders of the two groups. In real life this 'representative' was an anthropologically-trained employee of the Overseas Development Corporation. As the 'customer', he questioned the concept of the anthropologist being an important part of the team, and rejected the proposed amount of time devoted to anthropological research on the project (allowing just one month at the start, instead of the full six-month life of the project). He stressed the need for the customer's requirements to be very well-known to the consultants (who should demonstrate their awareness of this fact), and that tenders should strive to meet them. The tender report should certainly deal with potential problems that have escaped the customer's attention, but only insofar as this is of benefit to the customer and does not criticize his overall aims. Such insights might impress the customer sufficiently for him to award that particular tender, but unsympathetic comments will clearly alienate him. In short, the use of an anthropologist on such projects could only be justified by the usefulness of its results to the customer.

As an anthropologist, the 'representative' pointed out two potential conflicts: between the anthropologist's ethical position and the client's needs; and over the speed with which data has to be collected and analyzed under such conditions, when compared with pure research. Nonetheless, he emphasised the need for anthropologists to think and act positively, proving to potential customers that they have valuable special skills just as much as economic or technical experts. This he mentioned as one of the cardinal aims of the workshop. To quote again from the newsletter:

'Anthropologists must learn to sell themselves; and they must also set out to convince potential users of their capacities.'<sup>3</sup>

### *Conclusion*

On the whole the workshop was well and economically organised, and the time-table worked reasonably efficiently. It would have been useful to have had more time to correlate the findings of the various groups, but as this obviously had to be the last part of the time-table, it was perhaps inevitable that it should come under pressure. However, in the final session it emerged that not every group had felt that the anthropologist had to be treated as a dispensable, nominal figure on the research team. The social services group, for example, were able to suggest the social workers themselves as one potential difficulty in their case study, in that they might be more inclined to identify the existence of a 'problem' in relation to the adolescent girls in their care than were the girls themselves. One inference to be drawn from this may be that the most suitable practical role for the anthropologist, with his or her ideals of objectivity, open-mindedness and non-interference, is not as part of a team of researchers, but as an observer independent of both the team and the object of their research, with the task of assessing their interaction and identifying possible difficulties arising from it. In this way the anthropologist would be employing his or her unique training and attitudes without compromising the above-mentioned ideals. Persuading potential employers of this fact would be another matter, however.

Overall, the workshop provided one with a clear and dramatic demonstration of the differences between the luxuries of pure research and the pressures and compromises of applied anthropology. It can definitely be recommended to anyone thinking of following up a research degree with a non-academic, but relevant post, since it will certainly convey an idea of what such a post might entail. Every effort was made to provide a faithful

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<sup>3</sup> 'Aims, Plans, Policies', p.1.

simulation, and no effort was made to convert participants to a particular point of view; the emphasis was on letting the situation speak for itself. Nonetheless, the actual commitment of the group leaders to applied anthropology was evident, as was the amoral ethos of 'getting the job done'. It is to be hoped that these workshops will continue, not as training for applied anthropology - which in any case is not the intention - but in helping the individual to form an attitude to its merits and ethics.

R. J. PARKIN

#### RECENT RESEARCH IN THE SUDAN

In mid-November of last year (1983) a two-day seminar was held at the Institute of Social Anthropology, Oxford, on 'Recent Research in the Sudan'. It was organised by Dr Wendy James and attended by students of the social anthropology of the Sudan from Oxford and elsewhere. Dr James had recently returned from an extended stay in the Sudan where she had met a number of researchers from Europe, and had conceived the seminar as an opportunity for as many as possible of those researching into this subject to meet and discuss their work and the problems of carrying out research. This latter topic, though much discussed informally, did not, however, emerge particularly strongly in the seminar itself.

The contributions of Oxford scholars to the study of the Sudan is well known, with major studies by E.E.Evans-Pritchard, Godfrey Lienhardt, Jean Buxton and Wendy James, among others. There is still much interest in the area at the Institute, where there are a number of students with an interest in and experience of the Sudan working towards postgraduate degrees.

As well as students and scholars from Oxford, participants were also invited from Cambridge, Berlin and Rome. Though this provided an international flavour, it was a great disappointment that there were no Sudanese students present. Gabriel Jal (SOAS), the only Sudanese studying social anthropology in Britain at the time of the seminar as far as the participants were aware, was unable to attend. There was, however, an impressive representation from Berlin, which could become a centre for the social

anthropological study of the Sudan. At present five members of the Berlin Institute - two of whom were at the seminar - are working on a Sudan project called 'World and External World - Social, economic and ideological ways with present-day change by tribal groups', which is being supervised by Dr Kramer and Dr Streck.<sup>1</sup>

There is always a temptation in social anthropology - though perhaps less strongly than formerly - to classify and identify researchers by referring to the people they have studied. Refreshingly there were a number of contributions to the seminar which could not be so classified. Enrico Castelli (Rome), who has been working towards an inventory of ethnological objects in European museums and collections collected by the early travellers of the nineteenth century, reported on his progress so far and on some of his discoveries. Douglas Johnson (UCLA) (who has until recently been working as the Assistant Director for Archives, Ministry of Information and Culture, Southern Regional Government of the Sudan) spoke on the existence, provenance, and usefulness of documentary sources for the fieldworker. He pointed out that though documentary sources have been used mostly for political and administrative history they do contain a variety of information of varying quality and detail on social structure, inter-ethnic relations, health, local economy, relations between indigenous peoples and government, and other topics of interest to social anthropological fieldworkers.

Jeremy Coote (Oxford), who has carried out some fieldwork among the Agar Dinka, 'Jur' of Mvolo, and Mandari, gave a discussion paper on the future directions which the social anthropology of the Sudan (and in particular the Southern Sudan) should take. He maintained that research should become both more general (not taking any form of boundedness of 'peoples' for granted) and more specific, concentrating on individual villages (as with village studies in India) or on particular topics (such as the *luak* at War Nyang [see below]). Barbara Harrell-Bond (Oxford) reported on research she had recently carried out in Yei River District of Southern Sudan, a region suffering an emergency influx of refugees. Her research aimed to fill the need for intensive field data to inform policy-makers and aid-donors concerning the present status of refugee assistance. The study of refugees is a growing field - they are a new 'people' with a built-in comparative framework provided by their position, the interest of aid agencies, governments, etc.

The other speakers in the seminar concentrated more on 'peoples', though even with these contributions a number of new concerns emerged. John Ryle (Oxford), Andrew Mawson (Cambridge), and Irene Leverenz (Berlin) spoke on the Agar Dinka, amongst whom they had all done research. Ryle's talk attempted to answer the

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<sup>1</sup> Preliminary Reports of the Sudan Research Project, in English or with English summaries, are available in booklet form from: Institut für Ethnologie, Freie Universität, Berlin, Brümmerstrasse 52, 1000 Berlin 32.

question, 'Why are the Agar different from the other Dinka?' The Agar occupy a central position in the geography of Dinkaland - they are the people of the horizon for both western and southern Dinka, though they represent something different to each. Educated non-Agar Dinka, as well as the Agar themselves, maintain that the Agar speak a purer form of the Dinka language and they sometimes ascribe this to their comparative proximity to the mythic source of the Dinka people by the Nile. Non-Agar however tend to regard Agar tribal culture as having been adversely affected by contact with, and absorption of, non-Nilotic peoples to the south. It was Ryle's conclusion that the distinctive features of the Agar and neighbouring groups such as the Apak Atuot can be explained only by much more systematic investigation of key lineages and perhaps a comparison of the *luak* at War Nyang with other Nilotic shrines.

Mawson and Leverenz presented a joint paper on the major Agar ceremony of the rebuilding of the *luak* (cattle-byre) of God at War Nyang. Both researchers had gone separately to Agar country to make studies of more mundane matters, only to find that this eight-yearly ceremony was about to take place. After a wait of some months they were privileged to witness it and to be able to record the events leading up to and surrounding the destruction of the old *luak* and the rebuilding of the new. They did not have the time to assimilate and organise their material before the seminar and consequently presented a preliminary report. At the time of writing (February 1984) both are back in the Sudan and hoping to return to the field.

Joachim Theis (Berlin), also now back in the field, presented the most formal paper of the seminar on 'Hunting, beer and kinship - sketches of inter-ethnic relations in the Yabus Valley (Southern Funj Region)'. Theis carried out some six months' fieldwork among the Koma of the Yabus valley. His first example of inter-ethnic relations was the multi-ethnic hunting party (*mata*), in which Koma, Ganza, Uduk, Berta and Gwama cooperate. The *mata*, Theis concluded, symbolizes the unification of different ethnic groups irrespective of boundaries and distinctions. But several aspects of the *mata*, such as the distribution of the meat on an intra-ethnic scale, also express division of ethnic and territorial groups. By contrasting Berta and Oromo ways of consuming and distributing beer (the centre of social life), on the one hand, and Koma ways of doing so on the other, Theis in his second example pointed to divergent concepts of the ethics of labour and social relationships. Finally, by discussing some aspects of marriage and kinship among the Koma, Theis showed, as had other participants in the seminar, that it is problematic to consider the peoples of the Sudan as clear-cut ethnic groups.

Burkhard Schnepel, another anthropologist from Berlin (though not a member of the project mentioned above) is at present writing a thesis in Oxford about the Shilluk of the Southern Sudan. Schnepel discussed the mystery (both to anthropologists and to the Shilluk themselves) of ritual regicide in the context of other Shilluk patterns of thought, feeling and behaviour. He also made clear that in dealing with a classical anthropological theme such as

'divine kingship', one has to take into account the Western perspectives in which former interpretations (and distortions) of the ethnographic material have taken place.

Ruth Buckley (SOAS) carried out her research in 1981 and 1982 in Payawa, a Kakwa village area with a population of about 2,000. She spoke informally about her research and particularly on what she described as the negotiation of meanings and identities specifically associated with gender, but had widened her concerns to show that not only gender but also kinship, age, education, religious and political affiliations, and even association with development projects, are used by the Kakwa to enhance their positions within Payawa. Indeed, all of these 'resources' are used at different times and in different circumstances depending upon the advantage that can be obtained.

Guro Huby (Trondheim) has also carried out research among Bari-speaking people, concentrating on Bari women in Juba. Huby summarized her thesis, which is based on fieldwork carried out in 1975-76 and 1978-79. The main argument is that Bari society has retained essential features of the old Bari pastoral gerontocracy in spite of radical changes over more than a century in the political and economic situation, and in their social and natural habitats. She argued that the Bari people as a social entity is being kept intact by the process of exchange of women and bridewealth between lineages, and that the old men are retaining control over this process.

Gerd Baumann (Belfast and Oxford) briefly described his research interests in the Nuba Mountains, based on fieldwork in 1976 and 1978-79. His fieldwork had focused on economic development, cultural change, and ethnomusicological interests. Much of Nuba Mountain ethnography shows a tendency to treat Nuba groups as isolated from their surroundings. Baumann's present work is concerned to show the interaction of the Nuba of Miri with other groups of the region. Miri villagers have experience of rural development and local government, and have accepted Arabic as a second language and Islam as their religion. A book presently being prepared by Baumann will give attention to their economic and cultural participation in provincial and national life and the internal changes that such participation has brought with it.

Wendy James gave an outline of the fresh research she had begun in Juba during part of the sabbatical year (1982-83) she had spent in the Sudan. This included research into 'Juba Arabic', the language of the streets. Increasingly widely used, and now a mother-tongue for some Juba families, the language has been officially scorned and academically neglected. The second aspect of James's research was the ambivalent ethnic category 'Nubi', particularly in connection with the growth of towns in the Southern Sudan. Work also included the collection and study of a body of transcripts from the cycle of radio plays put out by the Sudan Council of Churches' studio at Juba. A remarkably rich picture of the current dilemmas and conflicts of Juba life is reflected in these plays. Dr James hopes eventually to continue this research, and to bring together the various aspects of this study of changing Southern Sudanese society.

The seminar was wide-ranging, but with so many speakers in just one and a half days there was little time for formal discussion, and no firm conclusions were reached. A recent article by a leading Sudanese anthropologist<sup>2</sup> has criticised non-Sudanese scholars by painting a vivid picture of two kinds of anthropologist: 'those students who come to do "exotic" fieldwork' and 'professional anthropologists who come as "experts" even if they have never been in the Sudan before.' Neither category, it is argued, has sufficient commitment to the country, being merely concerned 'to get their degree' or, presumably, 'to earn a fast buck'. Such a view of expatriate researchers must be taken seriously. The advantages we can sometimes have over indigenous scholars are enormous. Too often the research results to which so many Sudanese themselves have contributed never find their way back to the Sudan, and are consumed - if at all - only by the academic community in the West.

The participants in the seminar were, however, united by their concern for the Sudanese peoples they have lived with and learned from. As the country enters another difficult period in its history, it becomes even more important that students and scholars can meet in places like Oxford, London, Cambridge and Berlin to discuss the Sudan, its history, and its peoples.

JEREMY COOTE  
BURKHARD SCHNEPEL

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OXFORD UNIVERSITY ANTHROPOLOGICAL SOCIETY

The O.U.A.S., which was founded on January 28th, 1909, recently celebrated its 75th anniversary. A gathering for members and guests was held at the Museum of Modern Art in Oxford, where fittingly the RAI exhibition of early anthropological photographs - 'Observers of Man' - was currently on show.

The Society, which exists to promote the study of anthropology in all its aspects, still meets regularly during term for evening talks by invited speakers from Oxford and elsewhere.

Anyone wishing to obtain further information about the Society should contact the Hon. Sec., c/o Institute of Social Anthropology, 51 Banbury Road, Oxford.

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<sup>2</sup> Abdel Ghaffar M. Ahmed, 'The State of Anthropology in the Sudan', *Ethnos* 47 (1982), pp. 64-80.

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