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CULTS OF DEATH IN NORTHWESTERN PORTUGAL

Psalms 16:10

'For thou wilt not leave my
soul in Hell; neither wilt
thou suffer thine Holy One
to see corruption.'

Shortly after I began field work in northwestern Portugal I became aware of a set of cults the significance of which was, at first, not clear to me. It was only later that I realised that there was a pattern unifying them. These cults had to do with the opposition between physical life and death and spiritual life and death. This paper is an attempt to deal with these cults as a group so as to arrive at general conclusions about their significance.¹

The term 'cults of death' itself may be misleading. I refer mainly to three local cults not all of which are specifically related to physical death. As they centre around the life/death opposition, however, and can be understood only in that context, I refer to them by means of this term. The life/death opposition is perhaps the single most important symbolic unit in the northwestern peasant culture of Portugal. It can be observed in practically every field of action and thought, and it is to this that I attribute the great importance that these cults assume locally. Furthermore the term 'cult' may be misunderstood. Cults are not considered by the people to be separate entities of cultural action. It should be made clear that the people see what I have called 'cults' as an integral and indistinct part of their dealings with the world at large and the supernatural world in particular.²

The first cult at which we shall look is that of the Souls of Purgatory; that is the souls of the recently dead burning in Purgatory, waiting for the day on which they will be purified by fire of their earthly sins. The second is the cult of *incorrupt bodies*.

These are the bodies of people who, after burial, remain incorrupt, their bodies preserving the flesh and their clothes remaining in perfect condition. Finally, we shall deal with the cult of people who are reported not to eat or drink - and therefore not to have normal bodily functions. These people are considered to be saintly and I shall refer to them as 'non-eaters'. These cults are not at all specific to Portugal, and may in fact be found throughout the Catholic, and indeed the Christian world. Naturally I cannot claim that my arguments have validity for such a broad field as I have limited myself intentionally to northwestern Portugal.

In their recent book, *Celebration of Death: The Anthropology of Mortuary Ritual*, Huntington and Metcalf argue that the funeral rites

express the social order by differentiation between people of unequal status. This type of argument lends itself to extensions in many directions. Hertz himself connects it with the practice of preserving relics of individuals of high esteem, and hence the origin of ancestor cults. (1979:73).

In my opinion this argument, which has its value, cannot explain the internal consistency of these cults. I found then, that in order to cope with them, I had to go beyond this explanation. The attempt to do so forms the main argument of this paper.

Before examining each cult separately, I shall give a short account of burial rites in the area where I carried out fieldwork, giving particular attention to the specific practices and conceptions that will illuminate the analysis of the three cults.

In northwestern Portugal there are three very distinct types of death to which people's reaction vary accordingly.³ A person can die of old age. In this case he has had time to prepare himself for death: he has taken the Extreme Unction which, as Van Gennep rightly points out is a 'rite of incorporation into the other world' (1960:165), and his death is therefore not problematic, for the passage from the living to the dead was well delineated and ritually complete. One can, on the other hand, die of some illness: this is the second type of death. Although in this case one may have been able to take the Extreme Unction and to go through the rituals of separation from the living and incorporation into the dead, this type of death creates greater problems for it wrenches a previously able member from the midst of the living, thereby threatening the social order. To this type of death the reaction is more violent, the wailing is stronger and the dangers that surround the pre-burial period are more acute. Finally, there is violent death. It is this type of death that produces the greatest reaction on behalf of the local community. The wailing is very strong and all the practices attached to the fear that the dead person's soul may not have separated itself from the world of the living, are strongly enforced.

Immediately after any type of death all gates and doors of the house of the deceased are thrown open. The relatives abandon themselves to wailing usually keeping to an inner room, until the body

is prepared for viewing. This is the first bout of wailing and it has the effect of calling the attention of all close neighbours. These come to the house, and a group is immediately organised which consists of a few women neighbours who look after the kitchen and a man who *corre com o enterro* - literally, 'runs the burial'. This man, who is chosen by the family as a particularly trustworthy person, calls the relatives and the undertaker, buys the food and prepares the house for the sudden surge of people after the second bout of wailing. This takes place when the coffin is brought to the house and the body is exposed in it. The family then emerges dressed as if it were the middle of winter, even though it may be summer. In this way they feel they are protecting themselves against the bad influences generated by this death. They sit wailing, accompanied by their relatives and visitors who, towards evening, start coming to the house. The visitors go to the foot of the coffin and sprinkle the dead person with holy water in a practice which, as Hertz rightly emphasized, has the function of pushing away the evil spirits (1960:83 and fn. p.151). Indeed, if the person died at home, the sprinkling with holy water and the recitation of *Ladainhas* (long prayers designed to scare away the Devil) would have already begun when he or she entered his or her last moments. It is believed that, if we could see through the eyes of a dying person who had not been sprinkled with holy water, we would not be able to see the light of day because the devils around him are so numerous that they obscure light.

Following the rite with the holy water, each visitor greets the members of the household individually. The visitor then leaves money for a mass, and writes his name down on a list. This practice too can clearly be seen as a rite of separation, for each mass that is said for a dead person helps him or her to be released from Purgatory sooner. Therefore, by offering a mass, visitors are helping with the process of transition of the deceased from the world of the living into the world of the dead. During all this time, prior to the mass on the seventh day after the death, nobody ever pronounces the name of the dead person. They call him *o falecido* - the 'deceased'. Indeed, before the burial, they even call him *o falecido sobre terra* - literally 'the deceased above the earth'. Only close relatives keep this practice for the duration of the whole year of mourning.

As has often been pointed out, death as a social phenomenon is a process and not a complete event that takes place at the time of physical death. There is a local conception which illustrates this very well which is the belief in *remédios da desempata* - literally, 'medicine of the deciding game'. The metaphor implies that there is a draw, as in a game, between life and death. When a person is very sick and is approaching death, the locals believe that there is a point at which the doctors give him or her this medicine. If the person has life in him or her, he or she will survive; if, on the other hand, the person is fated to die, death will immediately ensue. This conception is made even more evident in the visions of the Procession of the Dead, which afflict some locals. These processions consist of people who have recently died, accompanying

a coffin inside which lies the soul of the member of the parish who is next going to die. The coffin is followed by those who will die immediately after that. These visions, however, are only seen by a few *voyants* who cannot disclose the names of those in the procession. Similarly there is a belief that the soul of a person who dies far away from home or during sleep, wanders around at night bidding farewell to those who remain alive, and gently touching them.

Here we see clearly that death is a process that develops before the person's actual physical death. This conception - that the soul is separated from the body and that its movements have to be directed - is evident, too, not only in the practice of not calling a person's name (for fear the soul answers the call) but also in a series of other practices. No fire should be taken out of the house where the corpse lies, for the soul would follow it. This soul would then be *penada*, literally, 'in pains' or 'in sorrow' and it would haunt the community. The priest must accompany the funeral procession otherwise the soul will go astray. The close relatives, however, do not go in the funeral procession for, should they go, the soul would be so sad that it would not go to the grave. When the coffin is being lowered, most people shift about, otherwise the soul would find them and remain with them. After the coffin is in the grave most people throw a bit of earth into the pit because, without this rite of separation, the soul would remain 'in pains' for it would follow them. It must be noted that all these rituals have two symbolic functions: by pushing the soul away, the people are not only protecting themselves from its bad influences, but also helping it on its way to Salvation.

The danger that the soul should remain 'in pains' is specially great if the deceased has suffered violent death, in particular if he was murdered. The soul of a murdered person will sit on its homicide's back till it breaks. The only way to avoid this is to plant a cross on the spot where death occurred. Even under normal circumstances, however, the soul of the recently deceased is dangerous, for its calls to other souls. When a funeral procession passes by, people who are lying down, even if they are sick, get up, otherwise the soul of the deceased would collect theirs. Souls 'in pains' are very specifically feared, for they are those for whom the rites of separation were not fully operative and, as Van Gennep says, 'these dead without hearth and home sometimes have an intense desire for vengeance. Thus funeral rites also have a long-term utility; they help to dispose of eternal enemies of the survivors' (1960:161).

Before entering more fully into the discussion of the Souls of Purgatory and the cult related to them, I would like to speak of the practices related to the body after its first burial. Unlike Greece (cf. Alexiou 1974) where the process of secondary disposal involves quite clearly defined rituals, in northwestern Portugal this practice tends to be rather understated. My opinion is that it is in an overt state of decadence due to the repressive legislation that was passed in the middle of the nineteenth century as an attempt by the state to curb most of the traditional burial

practices (e.g., the Maria da Fonte uprising). The State has allocated a minimum period of five years during which nobody is allowed to open a grave. As a rule, however, this law is not strictly kept and usually the disinterment takes place after three to four years. The grave digger is given orders to go ahead by the family at least one member of which would be present at the time. The grave is opened and the bones are cleaned. If there is a bone-house they are deposited there; alternatively they are put into plastic bags which are tightly sealed and deposited in a corner of the grave. The justification for the disinterment is that the grave may be needed for some other member of the family, but my experience is that this is done even when no member is predictably approaching death. This acceleration of the process is due to the fact that secondary disposal marks the complete separation of the deceased from the living. After this ceremony the grave is no longer looked after by the relatives with the same intensive care. Some rites are nevertheless always performed by the family until the memory of the deceased dies, which usually happens at the level of the person's great-grandchildren. These rites take place during the days of All Saints and All Souls, and although they are specifically directed at the last occupant of the grave, they apply equally to all its previous occupants. With secondary disposal the period of liminality is finally over, for it is only then that the relatives know whether the body has corrupted or not. We shall soon see the relevance of this.

In order to grasp the meaning of the cult of the Souls of Purgatory we need to understand the local eschatological beliefs. These are in theory the same as those of the Catholic Church. In practice, however, they differ slightly. Locally most people are believed to die with *culpas* - sins, wrong doings, guilts, debts: this word can have all these meanings. Thus most people have to spend a greater or lesser amount of time in Purgatory according to the number of their *culpas*. The problem, however, is that, although Hell is often referred to, it is never really claimed that a certain person has probably gone there. The only instances I have come across of people being reported to have gone to Hell, were those in which souls that were in Purgatory were specifically sent to Hell by means of exorcisms, because they were bothering the living. Local informants were hesitant when questioned on this, and I have never encountered the idea that people go directly to Hell. However, everybody goes to Purgatory where there are two kinds of souls, those 'in pains' or 'in sorrows', and others. In theory, everybody in Purgatory is 'in pains'. In fact, however, some of the souls are redeemed and go to Heaven while others, for reasons which I shall explain, remain in Purgatory until they are either liberated or sent to Hell by the living. The latter are more properly referred to as *penados* - 'those in pains' - and they are the ones to haunt human beings.

There are two main reasons why souls remain 'in pains': the first and most important of these is that the soul has left *culpas*, debts, in this world. The word 'debts', understood in a wide sense, is perhaps the best translation for *culpas* here. For example, if a person has made a promise to a saint and has died before being

able to fulfil it, this will be used as an argument for his being 'in pains'. Another very common and important cause is if, at the time of death, the relatives discover that there is an absence of money that they cannot explain. This either means that the person left business deals incomplete or that money is hidden in a secret place. The most commonly experienced 'debt', however, is that when the deceased, during his lifetime, secretly moved the landmarks that divide the plots of the various families. As there are no land maps of the area, people are very fearful of this. The soul of a person who has done this remains 'in pains' and hovers around the area of the crime until someone moves the landmark back to its original place.

The second reason for being 'in pains' is if some ritual aspect of one's burial was not properly effected, for example, if the ground where a person was murdered or suffered sudden death has not been sanctified by the placement of a cross. Other instances have also already been mentioned such as when fire is taken from the house, or when the deceased is not purified with holy water. I have never heard of an antidote to these latter problems. I presume that they are possibilities that people consider but which are seldom, if ever, actually used to apply to specific situations. What lies in common between these explanations is that, either for material or economic reasons or for supernatural and ritual reasons, the soul of the person 'in pains' has not been able to go through the process of separation effectively. One therefore understands the rigour with which people follow these rites and avoid dying with these *culpas* unpaid.

I shall now discuss the three cults that I have mentioned at the outset of this paper. The cult of the Souls of Purgatory, in the minds of the people, only refers to those souls in Purgatory that are not 'in pains'. These souls are going to be freed by the action of St. Michael who, much against the Devil's wishes, redeems them from Purgatory due to the grace of God, mainly through the intercession of the Virgin, St. Anthony, and Christ. When these souls go to Heaven they are thankful to those who prayed for them, and they therefore ask the saints to intercede with God for the salvation of the faithful. This is the theological and popular basis of the cult of the Souls of Purgatory.

The Souls of Purgatory then are in a liminal position between life and death and they therefore assume both a sacred and a frightening character. Because of their transience they are used as mediators between this life and the next: between the powerlessness and blindness of this fallen world, and the powerfulness and omniscience of the world to come.

I shall now deal with the cult of incorrupt bodies. As we have seen, after death, a person is allowed to rest for a period of three to five years. Usually, after that time, all that remains of the deceased is a skeleton. In some cases, however, and for reasons that do not really concern us here, some bodies do not corrupt.⁴ Descriptions I have heard of the discovery of incorrupt bodies are very similar in kind: the coffin is opened and both the person's clothing and body are perfectly intact. There is, accompanying this unexpected

vision of perfection, a smell of sanctity which, in the numerous and various accounts I have read, is always described as the smell of a flower, either rose, violet or jasmine. Most of the bodies react quite badly in coming in contact with the air, and many disintegrate shortly afterwards. This, however, is not always the case.

When an incorrupt body is discovered, the people as a rule claim it is that of a saint. Whilst the authorities and priests are interested in burying it immediately, usually they are prevented from doing so by the people. This pattern has repeated itself in all recent cases that I have studied. While the body is above ground, the priest practices a ritual of 'lifting of excommunication', that consists mainly of whipping the corpse. This detail greatly surprised me until a priest guided me to a passage of the *Rituale Romanum* (tit.III, cap.4⁰) in which the priest is told to whip the corpse of an excommunicated person while saying prayers so as to unify him or her with the body of the Church, thus allowing his or her soul to go to Heaven. What the priests in this area do is perhaps very similar to all those rites of integration described by Van Gennep when he says

Whipping is an important rite in many ceremonies ... and is equivalent to the New Guinea rite of hitting the person over the head with a club to incorporate him into the totem clan, the family, or the world of the dead. (1960:174)

The Portuguese priests' actions can be understood if we take into consideration the theory that, when a body is discovered to be incorrupt, this is either because it is that of a great sinner or that of a saint. If the body belonged to a great sinner, then it would corrupt after being spiritually reincorporated into the Mother Church, the community of the living. As a rule, when this ritual has been completed, the priests and the authorities manage to rebury the corpse. If, however, when the body is again disinterred after five years, it is still intact, the pressure exerted by the people to keep it above ground is too great and the priests and authorities are forced to compromise.

I have studied eight cases where this cult is very much alive, but I have references to at least twelve other similar cases in northwestern Portugal alone. Of the cases I studied two have been known for more than five centuries, whilst the other ones were all disinterred in the twentieth century. As I have previously pointed out, cases of incorrupt bodies are fairly frequent in this area. In the small county of Ponte da Barca where I worked, I have come across at least four cases of people that are known to be incorrupt underground and two that are exposed in an aura of sanctity. Now this raises a problem: why should some be prayed to whilst others are not?

There are two main answers to this problem: the first has to do with the personal characteristics of the deceased. Their lives have to conform to a pattern of sanctity. If they were known to have led morally dubious lives they are then perhaps forgotten when they are reburied. Some bodies then are forgotten because

people's attention is directed to other cases that have a greater appeal. This leads us to the second answer: the sociological setting behind the cult is very significant. If two cases of a similar type are found, the one which was discovered last tends to be forgotten.

It is necessary to give a further note concerning the incorrupt bodies of sinners. Apart from the practice of flagellation of incorrupt bodies by priests, in the assumption that they belong to sinners, as was described above, I have never heard it claimed that an incorrupt body belongs to a sinner. When, in northwestern Portugal, an incorrupt body is discovered and a cult does not develop, this is due to a kind of social amnesia, as I noted above, and not to the belief that the body was that of a sinner or of someone excommunicated. The most widely accepted conception is that incorrupt bodies belong to saints and not to sinners. Some local informants have even told me that they do not know of sinners' bodies remaining incorrupt. I presume therefore that the practice of flagellation has its roots not in the people (who in fact resent it), but in the Church hierarchy. I do not want to discuss at length the problem of the incorruption of sinners here, since it is largely outside the concerns of this paper.⁵ My opinion, however, is that the symbolic structure of this conception is very different from that which ascribes saintliness to incorrupt bodies. The body of a sinner is incorrupt because he left behind so many *culpae*, 'debts', that he could not even decay: in other words, he could not go through the most basic process of separation from the living. This is why, as soon as he is incorporated with the dead by means of the lifting of the excommunication, his body immediately shrivels up and decays. He has to be seen as an extreme case of a soul 'in pains'. As with the other souls 'in pains', the rites of separation were ineffectual, only with these sinners, they were more so.

Of the eight cases of incorrupt bodies I studied in detail, three belonged to women. All of these are reported to have been unmarried and in fact claimed to have been virgins. Purity, endurance, and love are the qualities that are constantly ascribed to these women. In this they are very similar to the Virgin Mary. As in her case, incorruption is seen as a particular outcome of purity. Mgr. Poble (quoted in Winch and Bennett 1950:73-4) argues 'The incorruptibility of Our Lady's raised body may also be inferred from her perpetual virginity. There is an inseparable causal connection between *incorruptio virginalis* and *incorruptio corporalis* - the one if the fruitage of the other'.

The male saints are characterised in a somewhat different fashion. In their case purity is not such a focal point, although two of them were priests and the others were reported to be of the most unimpeachable morality. There is a similar stress on endurance to pain and suffering, and especially a great stress on their material generosity. This difference is rather significant for it clarifies the difference between male and female roles: whilst women are mainly subject to corruption in their sexual behaviour, men are subject to corruption mostly in their social and economic behaviour. On the other hand, the common stress on endurance to pain is connected with these saints' lack of concern for the base

needs of their fallen condition, and therefore their predominant interest in spiritual life.

Before going into the next and last cult I will try to clarify the issue of what makes incorrupt bodies saintly and why the locals award them special attention. Like the Souls of Purgatory, these incorrupt bodies are ambiguous cases. Although they cannot be said to be in a transient state, as the Souls in Purgatory are, they certainly are in a position of liminality for they lie somewhere between the dead and the living. Dead people normally do not have a body, as this decays a short while after death, unlike the living who are characterised by their possession of a body. As having a body is a characteristic of being alive, incorrupt bodies can metaphorically be said to be simultaneously dead and alive. When we take the lead suggested by Mgr. Poble in the quote above, where he related *incorruptio virginalis* to *incorruptio corporalis*, we understand the symbolic mechanism behind this cult.

I shall now give a brief account of the cult of 'non-eaters'. These are people who, due to their 'saintliness', can live in a state of permanent and total fasting. They do not eat or drink, or so it is assumed. I have heard locally of three cases, but I know only of one case in any depth. This was a girl who, at the age of fourteen, had been sexually persecuted by several men. To avoid them she jumped out of a window, and as a result was crippled for life. Whilst she was bed-ridden all that she ingested was the Host every Sunday. She was reported to have 're-lived' the experience of Christ's Passion every Friday night and as a result of this she was said to have 'an almost supernatural endurance to pain'. She died at the age of 51. We find here characteristics similar to the ones of the previously discussed cult. This woman was a virgin and it was in order to remain 'pure' that she embarked upon this life of saintliness and great suffering. It is worth noting that the other two cases of non-eaters that were encountered in northwestern Portugal were also women and both of them are reported to have been sexually 'pure'.

The Bavarian case of Theresa Newmann, reported by Vita Sackville-West in *The Eagle and the Dove* is similar in most points to the one described above. But why should these women be considered saintly; what in them explains people's readiness to believe in their healing power? Once more we are confronted with an ambiguity of definition that leads to a situation of liminality. These women are alive, but every Friday they 'die with Christ', for it is emphatically stated that they physically suffer Christ's Passion. At the same time they do not participate in those body functions that characterise human needs in this fallen world: they do not need to eat or drink and they are sexually 'pure'. They also obviously do not have bowel movements. These people are alive, but they truly behave as if they were dead. It is, I believe, in this ambiguity that one must look for the symbolic significance of this cult.

In all cases referred to above, the relation between the saints and the people is very close. A parish or a county feels unified in the common possession of a saint. People are proud of 'their' saints, and the offerings which they give the saint are payments

in gratitude for the success that they have had in the 'favours' and 'miracles' that they have asked the saint to perform. Their harvest does not look promising, someone is sick, the son is in the army, etc., all these situations correspond to direct needs which the saint helps to fulfil. The saint therefore has an immense economic significance. This significance rests on a symbolic structure and it is this which needs explaining.

The three cults discussed above all deal with entities that lie between two worlds. These are, therefore, in a liminal situation. But we have a further complication involved here, and that is that life and death are used in two different senses, the physical and the spiritual. This interplay of meaning in the use of these concepts is central to Christianity. Cobb, a theologian has said, 'It will be clear that the New Testament and Christian antithesis is not that of the Old Testament and Judaism, between this world and the next, but between two kinds of life both here and there' (1915-30:19). The body, as the centre of physical life, is at the same time, due to its needs and demands, the centre of spiritual death, and it is with the corruption of the physical body that spiritual purification in Purgatory can take place. Physical death may lead man back into spiritual life. Physical life and spiritual life are then used as metaphors for each other in certain circumstances, whilst in others they are seen to be opposites. Although they use life and death in two different senses, Christians seldom explicate the specific meaning it assumes in each instance. If we attempt to do this, however, we find that there is an immense contradiction involved in the use of the same term with these two different meanings. What characterises physical life is that it occurs in this world, whilst physical death leads one into the world beyond. Thus, spiritual life and death are characterised by occurrence both here and in the beyond. The two concepts therefore do not correspond. We see, therefore, that what characterises physical life is the presence of a body and its needs, whilst that which characterises physical death is the absence of the body and its needs. Conversely, spiritual life is characterised by the negation of the body, the stress being on the state of the person's soul, whilst death is characterised by the indulgence in the satisfaction of the bodily needs and desires.

By this argument I do not mean to imply that every single characteristic of the physical life/death opposition is negated in the spiritual life/death opposition. Rather there is a contradiction at play and this contradiction requires a mediation. Lévi-Strauss said: 'the purpose of myth is to provide a logical model capable of overcoming a contradiction' (1963:229). I would contend that these cults can be looked at in a manner similar to myths. They are, therefore, seen as attempts to mediate between the basic contradictions that are found in the local worldview. The Portuguese peasant, like all Christians, lives in a fallen world, one of hardship and despair, one of continual approachment to death. Yet he believes that there is a state of perennial life that can be achieved already in this world. To mediate between this conflict he takes recourse to entities which, because they are not clearly classifiable as dead or alive, can be used as mediators.

We now reach the point where we have to consider the position of Christ and the Virgin Mary in relation to this life/death symbolism. In the 1950's the Assumption of Our Lady became a dogma of the Catholic Faith. This belief, which had been widespread in the Christian Church since the 6th century, states that the Virgin died but that her body did not corrupt, for she was taken to Heaven in bodily form. The Virgin, thus, became with Christ one of the two only people who are believed to have been actually resurrected. They died, yet they are alive for their death did not imply the corruption of their bodies. In this, as we can see, they are similar to the case of the incorrupt bodies. But there is a great distinction between these two cases for Christ and the Virgin are *incorruptible*, while the saints are merely *incorrupt*. Christ and the Virgin were immaculately conceived, that is, they never participated of spiritual death, for they did not inherit the original sin. If their bodies did not corrupt this was because, through their heightened spiritual life, they managed to overcome the corruption of death. Christ and the Virgin lived a physical life and suffered a physical death but, because they were resurrected almost immediately after their death, they are alive in Heaven and their bodies are united with their souls. Once again we find a situation of ambiguity but here life has definitely won. This is an instance of what Lévi-Strauss would call a *helicoidal* mediation. Jesus and Mary, who are characterised by the forces of life (both spiritual and physical), confronted the evil forces of death (once more spiritual and physical); they suffered human death (here, however, only physical), and therefore managed to achieve spiritual life for all mankind, that is, the eventual abolition of evil.

If we look at the cults of non-eaters and incorrupt bodies we find that they effect a similar type of mediation. They too are characterised by forces of life (i.e. their arduous strife to achieve spirituality) and they confront the evil forces of death (both physical and spiritual). Through their death, however, (virtual in the case of incorrupt bodies and metaphorical in the case of non-eaters) they manage to bring about a victory of purity over the forces of death. This mediation is, however, no longer *helicoidal* in that it does not abolish the original polarity of forces, as the mediation of Christ and the Virgin had done, but merely postpones the action of the forces of death.

This second type of mediation is not specific to these two cults for it can be said to extend to the cult of all saints, since they all mediate between the spiritual life that they acquire and the physical death to which they are subjected. This idea of mediation is not mine and, in fact, the Church itself phrases the position of Christ, the Virgin and the Saints in a very similar fashion. As far as Christ is concerned it is enough to quote from Paul's Epistle to the Romans (5:18-19) where he says, 'For as by one man's disobedience [that is Adam's] many were made sinners, so by the obedience of one shall many be made righteous.' It is, however, when I categorise the Virgin's mediation together with Christ's that I am going against Catholic Theology. But even here I am accompanied by most non-Catholic theologians who openly objected

to the establishment of the dogma of the Assumption on the basis that it leads inevitably to an equivalence between Christ and the Virgin. When we hear Salazar speaking on this subject we see that the problem is very real. He says of Mary:

She acted as a mediatrix with the Mediator. The work of our salvation was so wrought. The Virgin expressed to her son the desires of the human race; but the Son, deferring to the Mother, received these, and again presented to the Father the desires both of His mother and of His own; but the Father granted what was wished first to the Son, then to the Mother.' (Winch and Bennett, 1950:107).

It is obvious then why Winch and Bennett complain that 'the general tendency of Mariology has been to parallel the Blessed Virgin with her Son, so that every prerogative that belongs to Him should in some measure be imputed to her also' (1950:107). For the north-western Portuguese people the dogma of the Assumption and its related conceptions are so deeply ingrained that I can incur no grave mistake in interpreting their cult of the Virgin as a form of mediation that is, in its symbolic bases, similar to the mediation of Christ.

As we saw when we looked at the broad symbolic structures of the three cults, however, the mediation performed by the saints (including incorrupt bodies and non-eaters) is a weaker version of the mediations of Christ and the Virgin. The mediation effected by means of the Souls of Purgatory is weaker still. The latter pray for us after our prayers have saved them. But, as their liminality is based on a transition and is not permanent, their mediation, too, is short-lived.

To summarise I would like to point out that, in looking at three cults related to death, we have to place them within the broader framework of the mediation between life and death in Christian theology. This mediation was interpreted as having been effected mainly by means of an analogy between physical and spiritual life and death. We found three types of mediation at play:

1) a *helicoidal* type, where the result is the complete abolition of the original conflict, i.e. that of Christ and the Virgin;

2) a *non-helicoidal* one, which was nevertheless *permanent* in its effects, i.e. the saints;

3) finally, a *non-helicoidal* one that was based on the *temporary* state of liminality of the Souls of Purgatory, and therefore the weakest of the three.

JOÃO DE PINA-CABRAL

FOOTNOTES

1. All of the data used in this paper refer to the northwestern Portuguese Province of Minho.
2. A mistake often made is the separation of strictly Catholic Religion from the local folk religion. This distinction misses the very important point that these people do not see any intrinsic difference between those beliefs which the Church opposes and those which it fosters.
3. I am not considering here a fourth type of death, that of children, because it does not play any part in relation to these cults. Children are 'pure' yet they are not saintly for they have not had time to confront this 'Fallen World' and to 'conquer' it. Significantly enough I have never come across any cult to the incorrupt body of a child.
4. The scientific reasons for incorruption are to be found either in the nature of the earth or of the coffin. Locally the coffins are often lined with lead. This practice may be the basis for the frequency with which incorrupt bodies appear.
5. The belief that incorrupt bodies are those of sinners is indeed frequent throughout the whole of Southern Europe. In Greece (cf. Campbell 1964:357), although there are incorrupt bodies of established saints, the most common practice is to classify cases of incorruption as related to sinners. In Southern Portugal a similar practice is found. A clear example is the 'Capela dos Ossos' in the Church of St. Francis in Evora where two incorrupt bodies have been hanging on the wall for many years as a *memento* of spiritual death.

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THE WELL-SPRINGS OF ACTION: AN ENQUIRY INTO 'HUMAN NATURE'

I

Richard Broxton Onians' (1951) book, *The Origins of European Thought about the Body, the Mind, the Soul, the World, Time and Fate*, is as exhaustive as the title suggests. Its value rests in enabling us to perceive the dim outlines of a theory of human powers which was present in the minds of the peoples of western Europe before the dawn of history. The phenomenology and osteology with which Onians supplemented the account, further enable us to locate the physiological processes on which the theory must have been based. It has been lost. Today we possess only fragments. And yet, we repeatedly make recourse to the theory in our behaviours and speech as if we knew its substance.

The hand is placed upon the chest when one pledges allegiance to one's country. To indicate assent, one nods one's head. Someone who is over-sexed is called 'horny'. In a Catholic church, one touches one's forehead and one genuflects before the altar. We associate the symbol of a skull and crossbones with death. We ascribe to ourselves the capacity of appreciating the 'aesthetics' of an object, and speak of the inspiration we receive from a speech.

These are but 'shreds and patches', but at one point they were connected. The theory rested on a primordial disjunction between fluid and air; between the liquid or liquefiable substances contained in the brain, the cerebro-spinal column, the genitals and joints, and the breath. In Latin, this opposition was expressed as one between the *genius* and the *animus*.

The *animus*, or 'breath-soul', was associated with the lungs, the seat of consciousness. This association lingers in the attitude one assumes when pledging allegiance. It was in the lungs that thoughts took shape in the form of words which were conceived of

not as sound-images but as breath-images. One of the earliest theories of perception held that 'breaths' emanate from objects in the world to combine with the 'breath' which emanates from the eyes to produce consciousness. The term aesthetic arose out of this association.

The *genius*, a kind of 'life-principle' associated with the unconscious and the sacred, was disseminated throughout various parts of the body. One propitiated it by massaging the forehead. One beseeched another's genius by clasping his knees. Onians quotes Pliny the Elder:

In a man's knees there is a certain sanctity observed by the nations. It is these that suppliant touch ... perhaps because in them is the life (*vitalis*). For in the joint of each knee ... there is in front a certain bulging cavity, on the piercing of which, as of the throat, the spirit flows away (p. 181).

The *genius* was the executive power in man. The semen and 'marrow' with which it was associated was 'the stuff of life and strength', just as the breath was 'the literal stuff of consciousness'. Among the Ancients, the reason why the nod was a binding and sacred form of promise lay in the fact that the life-principle contained in the head was involved. Horns, given familiarity with the promiscuous propensities of goats, were thought to be permanent concentrations of this life substance. They were embodiments of the seed, outcroppings of the procreative element. In order to signify death, the fact that the life-soul had departed, one placed the head of the deceased between his thighs. This custom has bequeathed to us the image of the skull and crossed thigh-bones, or *femurs*, 'that which engenders' (p. 182), but engenders no more.

What this brief foray amongst a complex body of ideas is meant to illustrate is that there is a code which persists beneath our actions. The lexical and behavioural items described above are messages of which the original interpretations have been forgotten or transformed in the course of time. Yet we continue to respond to the messages, and therefore the fragments of a once more comprehensive discourse. The associations linger; they are inscribed in our language. But the physiological processes, the phenomenology of the body in which these collective representations are grounded, could be subjected to a completely different articulation when framed within an alien tradition.

My concern in this paper will be with the powers that people who do not share our European heritage have ascribed to themselves. As Professor Needham (1980a) has indicated, this field of investigation discloses many discrepancies between the registers of inner states different peoples have devised. Given the inconsistencies, for instance, in the ascription of a capacity for belief (Needham 1972), our received ideas about what constitutes human nature must be disengaged. In their place, we must be prepared to accept one or the other of the following

conclusions. Either all the discriminations we encounter in alien vocabularies of psychological terms are true and therefore human nature is infinitely diverse; or the vocabularies are both true and false registrations. In order to ascertain those states which can be counted real as opposed to fictional, one could cull from vocabularies those terms which overlap in their specifications, determine the physiological processes to which these are meant to correspond, and consider at least a core of human universals settled on these grounds. As Professor Needham further notes, the problem then becomes one of how to account for inventions, states which are fictional in that they have no physiological concomitant.

What I am interested in are the fictions, and the development of a sociological framework within which to examine them. Towards this end, it is instructive to adopt an approach similar to that of Louis Dumont in *Homo Hierarchicus*. Dumont begins by positing two mutually exclusive 'encompassing frameworks' predicated on the ideas of hierarchy and equality respectively. Hierarchy and equality are principles of formal logic. They can be used to define the relationships which obtain within any given set of terms. It is for this reason that these principles are able to order and account for the existence of facts drawn from such dissimilar domains as economics, politics, marriage practices and religious observances.

One can assume, with considerable justification (Needham 1980b, Bateson 1973), that in elaborating their social institutions men continually make recourse to such relational principles as these. They are what we think and act with, given that any two bodies distributed in space must stand in some form of relationship to each other. The relationship can be one of hierarchy or equality, symmetry or asymmetry, opposition or analogy and so on. The realization of an asymmetric relation in a social setting can be observed, for example, in the practice of paying respect to those who are one's seniors, or in the unilateral flow of goods and persons between clans.

Traditionally, our psychology has focused not on these relationships, but on the individual in isolation. It will be seen that this hypostasization of the individual has in its turn had a profound effect on our psychology. For the sake of exposition, the individual must constitute our point of departure. But we can nevertheless distinguish between two contrasting orientations: the internal and the external. Each of us is engaged in a complex network of interactions with other persons and things. At a phenomenological level, it is possible either to focus exclusively on these relationships and so face outwards, away from the self. Inversely, it is conceivable to acknowledge these relationships not as primary, but only as given after the fact and so facing inwards. My argument is that psychology can be situated at either point on the inter-face between these contrasting orientations.

In illustration of internal orientation, I shall sketch briefly the manner in which psychology has been constituted within our own tradition. In illustration of an external orientation, I will draw on Herbert Fingarette's (1972) exegesis of Confucius'

Analects, and I.A. Richards' (1932) interpretation of Mencius. Following this, I wish to consider one anthropological case-study: What the Tanimbar islanders of eastern Indonesia conceive of as the well-springs of action with an attitude of mind open enough to receive them.

II

Marcel Mauss (1966) has traced the development of the category of 'the person' within our tradition, locating it as emerging at the juncture of Roman legal practices and Christian metaphysics. It is in part to Cassiodore, that we owe the constitution of this 'rational indivisible, individual substance' as a 'conscience and a category' (ibid., p. 358). The genesis of the category of 'the self' has been even more precarious, and is still in a process of formation (ibid., p. 359). Yet it pervades our outlook and has also informed our anthropology. We have a profound tendency to think in terms of individuals. Edmund Leach (1967) has commented that

It may be ... that conventional anthropology is at fault in thinking that the central problem of kinship studies is to show 'how individuals are linked together by relationship', for such a formulation implies that it is conceivable that individuals should not be linked together by relationships (p. 129).

By inverting this perspective, Leach argues, we arrive at a more accurate representation of experience. A mother and child are one before they become two. 'Our concern perhaps should not be with how relationships are formed but with how they become weakened' (ibid., p. 129).

The problem of how to connect things together if one starts from the assumption that they exist apart has been handled in an even more peculiar manner by psychologists. For example, it has led to the positing of libidinal and aggressive 'drives' located deep within the self in order for the individual to cathect various objects in the environment and so begin to forge relationships outwards. The circularity and logical inconsistency of such explanations of behaviour has been exposed by Bateson (1973: 25-26).

The same preliminary assumption has led to a grotesque proliferation in the discrimination of inner states within our tradition. The extensiveness of our vocabulary of psychological terms

in comparison with those of other cultures can be interpreted as proof that 'our psychology attempts ... to describe a larger range of the mind's possibilities' (Richards 1932: 80). But this has had deleterious results, for the discriminations have been further subjected to a normal/abnormal classification. Such 'abnormal' mental capacities as schizophrenia however, are not always due to chemical imbalances in the constitution of the individual. More enlightened research has revealed that such 'states' are often caused by pathologies and paradoxes in communication, a relation *between* human beings.

Still, each of these examples pales before the next, in which it will be seen that the perimeters of the self are expanded to include and reify the very relationships with the universe on which it depends:

When eventually the vocabulary of experimental psychology was extended to interpersonal contexts, the language of psychology still remained a monadic one. Concepts such as leadership, dependency, extroversion and introversion, nurturance and many others became the object of detailed study. The danger, of course, is that all these terms, if only thought and repeated long enough, assume a pseudoreality of their own, and eventually 'leadership', the construct, becomes Leadership, a measurable quantity in the human mind which is itself conceived as a phenomenon in isolation. Once this reification has taken place, it is no longer recognized that the term is but a shorthand expression for a particular form of ongoing relationship (Watzlawick 1967: 27).

The perennial problem has been one of failing to focus on the context of interaction and isolating entities instead. This inevitably leads to an inferential study of the properties of mind, when what we ought to be focusing on are observable manifestations of relationship.

III

One of the most vital respects in which the early Chinese tradition of enquiry into the constituents of human nature differs from our own is reflected in the fact that the distinction is not usually drawn between nature in general and human nature in particular. *H'sing* refers to both, together. As a result, as I.A. Richards

(1932) notes, 'the mind and its objects are not set over against one another ... psychology and physics are not two separated studies' (p. 5).

This non-separation can be taken as a preliminary indication of what we have called an 'external orientation'. This tenet alone implies a theory of mind which conceives of its powers of perception and thought as natural processes within the natural order. It also allows for associations which, given the distinctions embedded in our language, we would never make. In the thought of Mencius (372-289 BC), since man's nature and the nature of things are identical, it is conceivable that 'the very seasons fall out of order as disorder grows in the mind of ... the Ruler' (p. 76).

It is difficult to characterize the writings of Mencius as an actual enquiry into the nature of man. As Richards (op.cit., pp. 61-62) points out, his arguments were not demonstrations but instructions; his statements, in effect, edicts. The following argument (VI:1-4) between Kao Tzu and Mencius is a case in point.

Kao Tzu: Love [*Jen*] is internal, not external.
Right [*Yi* - in the sense of never contravening social etiquette] is external, not internal.

Mencius: Why do you say Love is internal, Right is external?

Kao Tzu: He is aged and I age [pay the proper social respect to] him, as I am his junior. Similarly, he is white and I recognize his whiteness. It follows that it [his whiteness] is white externally.

Mencius: Though recognizing whiteness in a horse and in a man are alike, is there not a difference between recognizing agedness in a man and in a horse? Moreover, would you say that it is his agedness [the perceiving of the other's seniority] that is Right, or the recognition of [paying respect to] his agedness which is Right?

Kao Tzu: If someone is my younger brother then I love him. If that person is a Chin's younger brother then I do not love him. This is in accordance with my pleasure. Thus, I call it internal.
The fact that I age [pay respect to] the Chu people's aged, just as I age [pay respect to] our own aged, is according to age's pleasure. Hence, I call it external.

Mencius: My enjoyment of a roast cooked by a man of Chin differs in no way from my enjoyment of a similar roast cooked by my own people. This is true of many material things which are similar. If so, then is the enjoyment of a roast also something from without?

What Mencius does in this closing remark is to dissolve the distinction Kao Tzu makes between his own pleasure in loving someone, and the pleasure which the aged derive from his duty (which is implicitly an un-pleasure) of paying respect to them. Kao Tzu's response to the final question can only be in the negative (given the distinctions he had already drawn): one's enjoyment of a roast 'naturally' derives from within. This has a false ring because, given the way in which the question is posed, one could answer either way and still remain unconvinced. The answer is not 'either/or'. It is 'both', which shows that the distinction between internal and external, subjective and objective cannot be maintained.

Richards holds that one of the most salient features of this argument is that neither Mencius nor his opponent treat the rightness of paying respect to age as age as open to discussion. Consequently, they do not inquire into the reasons for giving respect, but 'treat only of *how* this respect is to be determined' (op.cit., p. 56). For this reason, a scientific account of human nature is precluded, or rather subverted to 'giving an account of it that will conduce to the maintenance of these fixed, unquestionable [social] observances' (ibid.).

The above interpretation is plausible, but at the same time misleading. A detailed examination of the text reveals that Mencius is concerned to distinguish between different qualities or manifestations of relationship. Thus, he distinguishes between the recognition of agedness in a man and in a horse. But he pays no attention to what we would perceive as the cause or origin in the person of the impulse which issues in the form of a relationship. Indeed, Mencius confounds the internal/external distinction Kao Tzu was trying to draw. His focus is on the relationships which obtain between a person and his social or physical milieu. This is the locus of psychology for Mencius, a psychology which is not separated from physics.

At first glance, Mencius does appear to be turning social conventions into human nature. In other words, he is writing an apology for the traditions of Chinese society. From an analytic perspective, we would not be prepared to accept the paying of respect to age as age as a natural resemblance among men. It is a convention. If we were to admit that such conventions are constituents of human nature, then we would also have to admit that this nature is infinitely diverse. But there is a sense in which this conclusion is beside the point to Mencius' argument. For him, human nature is located precisely at the junctures: the synapses between what, for lack of a better word, can be called the self, and others. It is the relationships which are human nature for Mencius.

Richards was on to this when he remarked: 'the division between *the outer cue and the inner impulse* ... remains unmade' (ibid., p. 54). Yet paradoxically, he regards this as a fault in the argument. He contends that Mencius prevented Kao Tzu from fully realizing his position, which could be stated as follows: our impulse to revere the aged comes from without. It is imposed upon us by society. But our impulse to love is from within. It is a natural feeling which is not determined by external characteristics, such as age, over which we have no control.

Such a distinction is natural to Western psychology perhaps, but it is unnatural for Mencius. Like Confucius, he had no need to posit such mysterious inner powers in order for 'affairs to proceed'.

Mencius viewed human nature, *h'sing*, as a complex of compulsions which are innately good, and which, if allowed, would naturally develop into the four virtues: *Jen* can be translated as 'love', though it has no romantic or sexual connotations. It has also been translated as 'benevolence', 'fellow-feeling' and 'humaneness'. *Yi* can be rendered as 'righteousness', and means 'the shame and dislike of having done the socially wrong thing'. *Li* refers to good form or propriety, conducting oneself correctly on ritual occasions and in the presence of officials. *Chih* denotes simply the capacity to discriminate.

Significantly, this vocabulary is essentially social in nature. The terms pertain primarily to our comportment and relationships. To descry any reference to inner states among them would be misleading. Yet Richards concludes that what Mencius understood by *h'sing* was

an activity or incipient activity ... which, if permitted tended always to self-development. *This tendency to self-development - to the fulfilment of the mind - was what he meant by its goodness* (ibid., p. 71).

Richards' choice of words is unfortunate, for the concepts of self and of fulfilment are not strictly apposite. If any sense at all is conveyed by the configuration of characters, it is a sense of non-self and of emptiness, of composure as opposed to completeness.

IV

In order to form a more accurate conception of the condition which the four virtues express, we might turn to Fingarette's (1972) illuminating though brief exegesis of the *Analects* of Confucius (551-479 BC). It should be noted that Fingarette is not concerned with the distance which might exist between ourselves and Confucius, but with the latter's immediacy, especially from the perspective of modern linguistic philosophy.

There are two main points to Fingarette's argument. The first is that the magical claims which Confucius makes need not be viewed as inconsistencies or residues in his otherwise secular writings. For Confucius viewed the well-learned ceremony, *li*, as the paradigm

for human action. There is a 'magical' quality to the deployment of human powers in such a setting.

In well-learned ceremony [Fingarette writes], each person does what he is supposed to do according to a pattern. My gestures are coordinated harmoniously with yours Our gestures are in turn smoothly followed by those of the other participants, all effortlessly (pp. 7-8).

Thus, when Confucius claims 'With correct comportment, no commands are necessary, yet affairs proceed' (XIII: 6), or again, with reference to the ruler's ritual position,

If there was a ruler who achieved order without taking any action, it was perhaps, Shun. There was nothing for him to do but to hold himself in a respectful posture and to face due south (XV: 5).

then what is meant by correct comportment is conformity to the civilized forms of intercourse detailed in *li*. This obliges the other without visibly forcing him to submit to one's wishes, in so far as these can be accommodated within the traditional forms of action. According to Fingarette (pp. 10-11), such acts do not differ qualitatively from the sequence of behaviour which is initiated by someone in Western society turning to another to shake his hand, or the making of a polite request to someone to go and retrieve a book. The latter act is 'magical' in the sense that action is accomplished at a distance. One finds the book in one's hands without having had to exert any energy.

One could question, as Richards would, whether Confucius was really so detached from his surroundings that he used *li*, 'the holy rite', as a metaphor to discuss the 'performative' aspects of human interaction. Fingarette (p. 45) categorically states that

the metaphor of an inner psychic life, in all its ramifications so familiar to us, simply isn't present in the *Analects*, not even as a rejected possibility.

The most difficult term to account for within this paradigm is *jen* which, as we saw above, evokes so many psychological associations when translated into English. If *jen* is truly devoid of subjective meaning, then its use in the following passage is somewhat puzzling.

The man of wisdom is never in two minds; the man of benevolence [*jen*] never worries [*yu*]; the man of courage is never afraid (IX: 29).

Worrying has a definite subjective resonance for us. If *jen* is its opposite, as the structure of the passage suggests, then *jen* must also refer to some inner state. However, if one examines other uses of the term *yu*, a quite different conception emerges as to the condition it refers to.

When asked about being filial, Confucius replied, 'Give your

father and mother no other cause for anxiety (*yu*) than illness' (II: 6). Similarly, a man without brothers is characterised as being *yu* (XII: 5), and the wise man is *yu* towards the future (XV: 12). Given that the times in which Confucius lived were full of social turmoil, these last two remarks can be understood to refer to the impossibility of being certain about the carrying out of one's future plans, and to the potentially dangerous situation of a man who has no family to turn to for protection. Indeed, what all three uses have in common is that they refer to conditions of objective uncertainty. There is an unsettled or troubled quality to the situation. But troubled and objectively uncertain need not refer to a mental state, for, just as a child's illness is an observable condition, so too is the objectively troubled response of the parents.

The condition of being *yu* therefore, is that of a person involved in and responding to an objectively unsettled situation, where a bad outcome is a distinct and evident possibility. It follows that the absence of *yu* refers to the condition of a man who is responding in a way that is well-integrated into an objectively settled and organized situation. In other words, the *jen*-man is the one who has, in Confucius' words, 'submitted to *li*' (XII: 1).

With our predilection for identifying inner states, we are prone to characterize *jen* as some mysterious inner power, given the obscurity with which it is portrayed. But a more suitable interpretation can be arrived at by viewing *jen* in conjunction with *li* as different aspects of the same phenomenon. The mode in which they differ should not be conceived in terms of a subjective/objective split, but rather as the difference which exists between the standpoint of an actor and that of an observer. It is the direction one gives an act, the aiming to submit to *li*, which is *jen*.

The distinction between these different facets of an action can be grasped in terms of the following analogy, deriving from Wittgenstein (1965: 150-51). One decides to greet someone, and does so. While there is a 'way' to make the greeting, there is no 'way', as such, to *decide* to greet someone. The making of a decision cannot be analyzed into steps consisting of mental as opposed to physical actions. The deciding and the greeting are in this sense 'indivisible'.

Perhaps the matter is easier to grasp in terms of another analogy. If one thinks of *li* as a score of music, and *jen* as a particular performance, as Fingarette notes (op. cit., p. 53),

we distinguish sensitive and intelligent musical performances from dull and unperceptive ones: detecting in the performance confidence and integrity [or perhaps, hesitation and uncertainty] We detect all this *in* the performance. We do not have to look into the psyche or personality of the performer. It is all 'there', public.

For Confucius, the locus of the personal is *in* the performance.

To summarize briefly the point at which we have arrived: we now have before us two distinct psychologies. The one constitutes

itself in terms of entities and sees its task as an inferential study of the properties of mind. There would seem to be some form of agreement between this kind of psychology and a civilization which possesses a highly elaborate construct of the self. The other psychology constitutes itself solely in terms of contexts and relationships. It is these which are demarcated in its vocabulary, and not 'inner states' as such. If the above conclusion is correct, it is somewhat ironic that Richards (op. cit., p. 56) should have characterized the virtues as the 'terminus' of Mencius' thought. In effect, the virtues constituted Mencius' point of departure. It is what we would call the mind, or human nature, which was the terminus. Richards' whole book is based on a false object of study. He attempts to look inwards from the circumference of the sphere, the corporeal self, at which Mencius' thought ~~with~~ respect to the virtues leaves off.

V

In the preceding pages we have rejected the construct of human nature and have suggested in its place the existence of a social nature. In the rest of this paper I should like to consider this problem in connection with the Tanimbar islanders of eastern Indonesia. A series of questions, abstracted from our previous investigations, can serve as guidelines for this enquiry.

The first concerns whether in Tanimbarese culture there is a highly developed construct of the self. If this construct is absent, or only vaguely formulated, then it is conceivable that their psychology will manifest a different orientation, or locus, from our own. It is then essential to determine the encompassing framework or formal principles in accordance with which the Tanimbarese interact. In so far as these determine the prevailing patterns of social intercourse, they also enter into the structure of the person, governing his or her expectations, intentions and awareness of being.

From this formal level of understanding, we then pass to the physiological. At the level of bodily processes, there exists the possibility of an unmediated appreciation of those promptings which lend themselves to symbolization as human powers. As will be seen however, correspondences at this level are only partial, and, more importantly, such tentative universals are always mediated by cultural categories. In the final analysis, the category of 'the natural' must be dissolved.

On the Tanimbar islands, the birth of a child is an event which precipitates a complex exercise in social classification. In addition to assuming a pre-existent locus in the relationship terminology, soon after birth the child is ceremonially assigned a name, *ngare* (Drabbe 1940: 145). If the child is a couple's first-born, it will be given the name of the father's father, or father's mother. The motivation for this selection lies in the fact that this child is the one most likely to reproduce the alliances of the previous generation. An eldest son or daughter can be obliged

to marry a person belonging to the category of *wai*, the mother's brother's daughter, or father's sister's son, depending on sex. The name of an eldest child, therefore, reflects a constant orientation, an enduring position constituted by a recurring set of relations.

If the child is born at a later stage, then a name is selected from among the register of those who have belonged to the house in the past. It is critical to choose the right name so as not to offend some ancestor. The latter want their names to 'return to life', and not 'go forgotten'. They will plague a child with illness if this is not done. Thus, sickness in infancy often leads to a change in names in an attempt to appease the ancestor.

Something of the significance of this relationship to one's ancestral namesake can be grasped from the fact that the namesake is constantly propitiated. A youth will call upon his namesake to sit upon the tips of his arrows to ensure a good hunt (ibid., pp. 146-47). If the namesake is unhelpful in this respect, then another is found. This involves yet another change in name. The import of this slippage from one name to another is that each denotes a different orientation in relation to the universe. From the condition of illness, one passes to health. Each name signifies a different appearance.

In addition to a personal name, the Tanimbarese also bear and call other people by their *ngar'memet*, which Drabbe translates as 'shame-name'. It is expressive of the relationship of respect and avoidance which obtains between opposite-sexed members of the same clan, and between affines, save for those who are classified as *wai*. With the latter, as with members of one's own clan who are of the same sex and also called *wai*, there is familiarity. These persons one can hail from afar, speak rudely to, or embrace (ibid.).

A Tanimbarese then, is known by different names in different contexts. To locate an individual, a 'rational, indivisible substance' behind the names, is somewhat problematic. Different occasions entail different comportments and the attitude in each case is a feature of the relationship, not of the person.

In this regard, one could claim that the distinction between the outer cue and the inner impulse remains unmade. Yet such an observation has no analytic value. Drabbe records for example that one may not embrace or kiss a blood-sister other than on the occasion of a reunion after a lengthy absence (ibid., p. 145). The question arises whether this impulse is any more natural than the inclination to avoid contact with her on all other occasions. Were a man to embrace his blood-sister in some other situation, the behaviour would be recognized as out of context. It is the occasions, not the impulses which are incommensurate.

One further hindrance towards locating the individual as a 'rational, indivisible substance' within Tanimbarese culture is that he or she may not be entirely assembled in one place at one time. If a child whines continually, it is because its soul has remained in the last place it was at with the parents, such as the garden-house (ibid., p. 396). An expedition must be made to retrieve the soul. Threads are then tied about the child's wrists

and ankles to bind the soul.

VI

Drabbe states that the most powerful, influential and wealthy members of a community are those notables who are Masters of the eldest house, *das kajain* in each clan. In order to maintain its status, the eldest house must reaffirm its marital alliances with other noble houses in each successive generation. In order to exercise a maximum degree of influence, a nobleman must also have many series of people who 'tap palmwine', for him and not so many who 'supply [him] with loin-cloths and sarongs', (ibid., p. 187). These expressions refer to a house's *oeranak*, or wife-takers and *ndoewe*, or wife-givers respectively.

The complementarity of 'tapping palmwine', which refers to the prestation of goods classified as masculine, and the reciprocal 'supplication of loin-cloths and sarongs' which encompasses all goods classified as feminine, is the fulcrum of Tanimbarese society. No prestations can flow in the opposite direction, as for instance, were a wife-giver to give a pair of gold earrings, which are classified as masculine, to his wife-taker.

The pervasiveness of this principle of asymmetry in the orientation of social life on Tanimbar is unequalled by any other. It determines economic relations, political relations, and attendance at life-crisis rites. But in the domain of marriage regulations it gives way: one may marry either a *bat ndoewe*, a mother's brother's daughter, or a *bat waljête*, 'strange woman'. This element of contingency, or possibility of choice could be seen to contradict the structure of Tanimbarese society were it not for the fact that an *oeranak* who is recalcitrant either in supplying his *ndoewe* with the goods he wishes, or who refuses to marry the latter's daughter, may find himself poisoned, or trapped in a burning house (ibid., p. 191). Violence is the last recourse when the civilized forms of intercourse break down.

VII

Drabbe's (1932) dictionary of the language spoken on Jamdena, the largest island in the Tanimbar group, enables us to gain a sense of those powers which the Jamdanese recognize as human attributes. But there are grave difficulties involved in trying to list these. For instance, *bengeol*, 'to have a belly ache' looks straightforward, whilst *mat*, 'to be ashamed' is less so. For while the term may denote an inner state, the Chinese *yi*, which can also be translated as 'shame', has no such reference. Even more troublesome is the term *nabwai*, 'to transform or change into a crocodile', for how is

this to be included in a list of human powers? The problem is one of criteria. However another route is to focus on those powers in Jamdanese which we discriminate in English as well, and which we know have a physiological reference - such as anger.

In Jamdanese, 'to be angry' is *dalam méfanas*, literally 'someone hot'. At first sight the Jamdanese *méfanas* appears to correspond with our English 'hot under the collar'. But it is important to look further. From the same root, *-fanas*, the term *nafanas*, 'to be sick', is derived, as well as *nfanas*, 'to heat liquid'. *Méfanas* itself has a further application. One says of someone who touches the 'hot' sirih-pouch of an elder that his fingers will become 'knotted', meaning that he will receive heat blisters. The sirih-pouch is 'hot' because it contains the elder's soul-images, which no-one may touch except the owner. Were a child to touch them, it would become *nafanas*, 'sick'.

A pregnant woman ought not to journey by sea as the capes are dangerous for her. These are 'hot' because corpses and heads which have been taken in battle are often left on such promontories to rot (Drabbe 1940: 375). Other things which are *méfanas* are: the bride-price for a woman when it is assembled in a house waiting to be received, the bones of the dead, food which has been offered to the dead, warriors when they have just returned from a raid, and a man who has just seen two snakes copulating in the wood (ibid.). Riedel (1886: 285) informs us that seeing two snakes copulating in a dream is a premonition of a great wealth which will accrue to one in the near future. From this list it is possible to abstract a number of principles on which this classification of 'hot' persons and things is based. The common feature which articulates this category is that of a disturbance in the ordinary course of events, such as unexpected wealth accruing. This disturbance is often associated with a departure, or something which has departed from life. Thus for example wife-givers, about to receive the bride-price, are said to fear it. The wife-takers assuage this fear by pointing out that, 'these things are hot, but the sarong of the woman will cool them'. What this expression would seem to refer to is that once the moment of exchange is passed, the woman and the bride-price having changed hands, things return to their ordinary state in which 'hot' and 'cool' are balanced.

To sum up, then, 'anger' may be present in a given Tanimbarese context, and it would be only natural for a Western observer to recognize it. But this concept is extraneous to the logic with which the Tanimbarese system of classification operates. It may be that gesticulations used are simply a conventional form of behaviour which only we recognize as anger. It is ironic that even with far superior 'logical machinery' which Richards (1932: 102) has attributed to our modes of thought, in such a context as this, it is the Western observer who fails to make 'the distinction between the outer cue and the inner impulse'. I have attempted to show that a people who do not share our intellectual heritage, and predilection for 'interiorising' things, may not have committed the same errors in their thinking as we have (cf Bateson 1973: 460).

There is another word in Jamdanese, *ere* which means 'to be

hot-, or quick-tempered'. This may be our anger. There is not the linguistic information to prove otherwise. Thus, the question becomes: is to be quick or jerking in one's movements a natural or conventional form of behaviour? At a higher level of abstraction, the question arises: where does metaphor, or convention leave off and the real, or natural commence?

I trust this paper has led us some of the way towards an understanding of the fact that such a distinction is unnecessary - indeed entirely arbitrary.

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SOCIAL ANTHROPOLOGY AND POST-MODERNIST PHILOSOPHICAL ANTHROPOLOGY

The purpose of this paper is to draw the attention of social anthropologists to developments in philosophical anthropology which may be relevant to their understanding of their own discipline. I advance three main arguments. First, that philosophical anthropology is in transition and needs to take a post-modernist turn. Second, that philosophical anthropology, in so far as it takes such a turn, is relevant to social anthropology. Third, that social anthropology should cease to be confused with philosophical anthropology, especially since post-modernist philosophical anthropology contains the promise of a richer interchange provided such confusion is avoided.

I

The term 'philosophical anthropology' is not in common use in English-speaking countries, although historians of philosophy refer to the 'philosophical anthropologies' of philosophers such as Aristotle, Augustine, Aquinas, Descartes, Hobbes, Spinoza, Hume, Kant, Adam Smith and Hegel. In Europe however the term 'philosophical anthropology' is commonly used to refer to both the philosophy of man in a comprehensive or speculative sense, and to philosophical inquiries into more limited aspects of human behaviour. Indeed in Europe philosophical anthropology is an established university subject with chairs in several countries and

an intimidating literature. The European movement in philosophical anthropology in the 'thirties and 'forties centred on the attempt to reassert the uniqueness of man against reductionism and objectivism. It sought to inherit insights from philosophical biology, *Lebensphilosophie*, existentialism and phenomenology in a way which would overcome the fragmentation of the special sciences with their tendency to totalise partial disciplinary 'images of man', and lead to a new unified image of man, capable of both sustaining human aspirations and acting as a critical standard by which dehumanising tendencies in contemporary society and culture could be judged. The works of Buytendijk, Uexküll, Plessner, Portmann, Scheler, Rothacker, Groethuysen, Gehlen, Cassirer, Landmann, Strasser, Donceel and Kamlah can be seen as attempts to realise this ambitious goal.

Today European philosophical anthropology is in transition. The older movement, with its attempt to establish the uniqueness of man showing an anthropological difference, is in some disarray, partly because the first attempts to show such a difference were rather premature. It may be true that man is fundamentally different from other animals by virtue of his possession of certain powers, for example the power to use language or to act rationally; but to show such a difference requires long-term empirical study of both human and ethological data, and conclusions based on very incomplete studies cannot support the 'grand ends in view' which the older generation of European philosophical anthropologists tended to entertain. Moreover, in so far as such research is empirical, it is inherently doubtful whether it could ever support the kind of affirmativist politics of the subject to which these philosophical anthropologists were committed.

It may be that man ought not to accept denigrating modes of self-ascription which tend to inhibit his powers of self-realisation, but, if so, this is a *political* decision which needs to be made on political grounds, which themselves can be rationally articulated. It is a mistake to look to empirical materials to make such decisions for us. In so far as such a naturalisation of the politics of human self-ascription characterised the work of many of the older philosophical anthropologists, contemporary European philosophical anthropology tends to distance itself from their achievements, to insist on a less phenomenological, less Romantic approach to natural scientific data and to break up the utopia of a unified philosophy of man into sub-departments, one of more of which may be given a leading role in organizing and interpreting the immense bulk of relevant data.

Hence, contemporary European philosophical anthropology has tended to dissolve into various sub-disciplines, called philosophical anthropology, but really requiring re-description, viz.: (1) theories of human nature; (2) the study of images of man; (3) accounts of the human condition; (4) phenomenology of action and experience; (5) philosophical psychology; (6) psychoanalysis; (7) artificial intelligence; (8) the study of formal languages; (9) philosophy of action; (10) philosophy of culture; (11) philosophy of history; (12) philosophical biology; (13) philosophical

ethology; (14) thematisations of the results of the natural sciences; and (15) social philosophy. Such a development may not be wholly undesirable, although current attempts to revive the utopia of a unified philosophy of man based on the philosophy of action suggest that it may not be permanent.

In contrast to the existing, established trends in European philosophical anthropology I argue that philosophical anthropology needs to take a post-modernist turn. Clearly the notion of a 'post-modernist turn' requires some explanation. The term 'post-modernism' can bear at least two distinct senses. Post-modernism in the first sense can be characterised as modernism without the subject, the death-gasp of modernism as avant-garde cultural politics. Post-modernism in this sense can now be found in almost all the arts, most strikingly in literature, painting, ballet and architecture. It is a theoreticist aesthetics, oriented to trends in criticism: the exhaustion of a style, not something genuinely new. As such, it has some value, especially as a demonstration of where its assumptions lead, but it is not seriously post-modern. It comes *after modernism*, but as something more modern and more up-to-date.

Post-modernism in the second sense can be characterised as an emerging trend which attempts to take account of the fact that the parameters which defined the modern period from 1500 onwards are coming to an end, and that other, radically different parameters are beginning to emerge. Post-modernism in this sense is not more modernism. It is not an 'ism' to be set beside post-impressionism or constructivism. It is not an aesthetic, but a set of open and still-developing responses to an emerging post-modern situation in world economy and society. In the case of philosophical anthropology, a post-modernist turn involves an attempt to re-think structures of priorities and question-orderings inherited from the modern period to deal with a situation to which the inherited collections and cohesions may cease to apply. Specifically, it involves a transformation of the inherited anthropological questions ('What is man? What is his nature? What properties and powers does he possess?') into questions *opened forwards*, towards both new developments and new levels of articulation and choice where both self-ascription and self-constitution are concerned.

Such a turn is substantive; it is also political, in that it implies that individuals may acquire opportunities to make conscious choices of conceptual and linguistic frameworks in the context of both new questions, and questions which in the past were not regarded as proper matters for the exercise of individual judgement. Above all, such a post-modernist turn is *procedural*. It has no answers to impose, no doctrines to invoke. Instead, it implies that a radical differentiation of (1) questions, (2) discourses and (3) purposes is now becoming possible, which both delimits the scope and application of traditional answers and allows for unprecedented innovation at the level of multi-variable individual self-constitution.

II

A philosophical anthropology which has taken the post-modernist turn is relevant to social anthropology to the extent that it can provide differentiations which may illuminate social anthropologists' concerns. Many methodological controversies in social anthropology seem to insist on a confusion of strata or on monistic conceptions of the order of discourses, as if discourses formed a single uniform system, itself isomorphic with the way things are. Here the post-modernist philosophical anthropologist can assist the social anthropologist to make more complex distinctions between strata and between discourses and to expel the remnants of traditional philosophical anthropology from social anthropology on the grounds that philosophical anthropology is a different inquiry from social anthropology. He can also suggest differentiations which might allow the projects of earlier social anthropologists (Kardiner, Boas, Lévy-Bruhl, Malinowski, Kroeber) to be rehabilitated, provided those projects are disentangled from the ambitions and methodologies which encompassed their historical articulation.

Again, the post-modernist philosophical anthropologist can assist the social anthropologist by offering interpretative schema which can be used to interrogate rival psychological theories (Luria, Chomsky, Piaget) and to evaluate psychoanalytical systems. He can also provide delineations which may be relevant to contemporary anthropological debates about such concepts as 'natural logic', 'natural symbols' and 'natural categories'. To take the example of 'natural categories', post-modernist philosophical anthropology delimits the theoretical discourse or discourses in which 'natural categories' arise. It then insists that the precise meaning of both 'natural' and 'categories' be specified for such discourses, as opposed to what these terms may mean in ordinary language and analytical philosophy.

The question is particularly urgent since both words are used in contemporary anthropological discourse in unclear and inexact senses, and there would seem to be a special need to exclude from social anthropology senses of 'natural' and 'categories' which derive from Aristotle's metaphysics and may not reflect any results derivable from data. Those who speak of 'natural categories' often seem to mean either that such categories reflect abiding features of the 'natural' (i.e. physical) world, or that such categories are common because they reflect recurrent proclivities of the human mind. On the first view, the problem of how far the socially-interpreted perception of the physical world can be said to be 'natural' is occluded. On the second, it is unclear how it is proposed to show that such proclivities belong to an alleged natural (i.e. physical) substratum

(e.g. the brain) rather than to its socially-mediated operation after the emergence of social labour.

Similarly, talk of 'categories' is unhelpful unless clear distinctions are drawn between (1) categories in the sense of *classifications*, for example, according to kind or type; (2) categories in the sense of *concepts*; (3) categories in the sense of *aspects* (e.g. quantity, quality); (4) categories in the sense of *exclusive partitions*; and (5) categories in the sense of *divisions* which are held either by the actors or the anthropologist to be *categorical* in the sense of admitting no more basic re-description or referral. Moreover, even these distinctions may overlap and are insufficiently precise. In the context of post-modernist philosophical anthropology it is crucial to distinguish not only the classifications admitted by the actors from the re-descriptions of the social anthropologist, but also the metaphysical implications of the world hypothesis implicit in the social anthropologists's discourse from such features as might conceivably hold in other or all discourses, assuming a plurality of world hypotheses.

Again, post-modernist philosophical anthropology is relevant to social anthropology in so far as it is bound to investigate the politics and meta-philosophy of a variety of categorical schemes, to thematise the implications of different orderings and interpretations of world strata, and to develop historically-specific inventories and theories of the impostulates which currently govern such human activities as self-description, social explanation and self-ascription. This does not imply that social anthropologists should import the tentative conclusions of post-modernist anthropology, but only that a comparative awareness of the distinctions that others find useful, and where and how they choose to draw them, may be valuable.

III

Post-modernist philosophical anthropology also implies that social anthropology should cease to be confused with philosophical anthropology. The tendency to confuse social anthropology with philosophical anthropology runs through the whole history of social anthropology and has provided social anthropologists with many of their most ambitious goals. In a post-modernist context it is important to emphasise that such a thesis does not imply that social anthropology should not be 'philosophical' - in the sense of being philosophically literate, self-critical or self-aware. Nor does it imply that social anthropology cannot benefit from

the work of philosophers. On the contrary, it is clear that philosophical considerations may be important in distinguishing genuine from spurious questions in social anthropology, that a tolerance of inexact and confused distinctions constitutes one of the major obstacles to the furtherance of social anthropology as a discipline, and that a philosophically-informed clarification of the objects of social anthropology (and so of the meaning of such central terms as 'structure', 'symbol', 'category' and 'function') provides an alternative to eclecticism or the absolutisation of methodologism.

Nor does such a thesis imply the utopia of dispensing with utopia. It does not denigrate or underestimate the immense contribution made by social anthropologists who have pursued questions concerning the philosophy of man in their social anthropological inquiries. Obviously, the subject would be poorer without Bastian's concept of *Elementargedanken*, Lévy-Bruhl's postulate of a universal mystical mentality or Lévi-Strauss's quest for the innate structure of the human mind. Moreover, both Herder's project of a comparative anthropology and the attempt to determine the question of the psychological, logical and moral unity of mankind have produced detailed investigations of the highest order.

In a post-modernist context however it is necessary to define the limits of disciplinary inquiries more precisely and so the range of any general conclusions which they might reach. Post-modernist philosophical anthropology implies a distinction between the choice of a metaphysical scheme maintained in a philosophical discourse for philosophical purposes, and the choice of a mode of conceptualisation within a special science for purposes internal, though not necessarily exclusive, to it. Because post-modernist anthropology is procedural, and aims to leave particular decisions about choices of conceptual and linguistic frameworks at the level of self-ascription to the *proleptically* free decisions of individuals, such anthropology rejects any attempt to decide questions *at this level* by the special sciences. Such a move has the advantage that it helps the special scientist, in this case the social anthropologist, to distinguish between the limited theoretical claims which his researches may lead him to formulate, and the far wider need to decide on frameworks of self-ascription for which such claims are necessarily inconclusive.

The argument of this section however has wider implications. Post-modernist philosophical anthropology implies that social anthropology cannot be the philosophical study of man. It also implies that social anthropology may need to break with the dream of a unification of the sciences based on man. This dream has taken many forms - the dream of basing a philosophy of man on a unification of the sciences, the dream of using a philosophy of man to interpret the various sciences, and the dream of developing a 'science' of man as a foundation for the sciences in general. Here it is not uncommon to cite Hume's famous declaration in *A Treatise of Human Nature*:

'Tis evident, that all the sciences have a relation, greater or less, to human nature; and that however wide any of them may seem to run from it, they still return back

by one passage or another There is no question of importance, whose decision is not compriz'd in the science of man; and there is none, which can be decided with any certainty, before we become acquainted with that science. In pretending therefore to explain the principles of human nature, we in effect propose a compleat system of the sciences, built on a foundation almost entirely new, and the only one upon which they can stand with any security.'¹

It is less usual to notice the radical implications of Hume's project, including the implication that many questions cannot be decided 'with any certainty' until such a science is at hand.

Hume of course was by no means alone in his belief that a science of human nature could provide a foundation for the sciences in general. Indeed the quest for 'constant and universal' laws of human nature has influenced many social anthropologists. In a post-modernist context it is important to be clear about what a theoretically-informed empirical inquiry such as social anthropology cannot do.

No empirical inquiry can establish the metaphysical claim that there is such a thing as *human nature*, although such terminology could be pragmatically employed by one or more special sciences. Similarly, no empirical inquiry can establish the metaphysical claim that the human being has a *mind*, although empirical studies can influence the selection of distinctions used in describing human thought processes and behaviour. Similarly, it is difficult to see how any empirical inquiry could establish 'constant' or 'universal' laws of human nature, especially given the state of the pre-historical record and the difficulty of committing the future in advance to reproduce current characteristics. Such a conclusion provides no support for unduly narrow specialisms, but it does suggest that social anthropology, like many of the sciences dealing with man, would do well to clarify its objects and also the kinds and range of knowledges which can be achieved by the methods which it employs. In so far as the history of social anthropology can be read in terms of the asymmetries between its projects and the methods adopted to realise them, post-modernist anthropology can contribute a sense of the contexts in which various matters can be regarded as certain, and in which various theories can be treated as true.

The thrust of this paper may seem to reserve the wider questions for a new-fledged and little-known discipline, post-modernist philosophical anthropology. In so far as post-modernist philosophical anthropology is procedural, this is not the case. I simply argue that social anthropology cannot hope to settle questions which properly belong to philosophical anthropology. This may

1. David Hume, *A Treatise of Human Nature* [1739], (ed. L.A. Selby-Bigge 1888), Oxford: Clarendon Press 1975, pp. xv-xvi.

imply that some of the high purposes which social anthropologists have set themselves cannot be achieved by the methods they propose. But in a difficult world, this may not be a wholly unwelcome implication.

WAYNE HUDSON

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Oxford University Anthropology Society

Announcement

The Oxford University Anthropology Society was founded on January 28th, 1909 with Professor Tylor as President and G.C. Robson as Secretary and Treasurer. The Society has recorded minutes of its meetings since it was first formed. Beginning in the next issue of JASO a series of selected extracts from these minutes will be presented by the current Secretary, Mike Hitchcock.

REVIEW ARTICLE

RECENT BOOKS ON SOUTH AMERICAN ANTHROPOLOGY

I

IRVING GOLDMAN, *The Cubeo: Indians of the Northwest Amazon*, Urbana etc.: University of Illinois Press 1979 (2nd edition). 307 pp., Bibliography, Index, Ills. £5.40 (Paper).

CHRISTINE HUGH-JONES, *From the Milk River: Spatial and Temporal Processes in Northwest Amazonia*, Cambridge etc.: Cambridge University Press 1979 [Cambridge Studies in Social Anthropology, general editor Jack Goody, no. 26]. xx, 302 pp., Index. £12.50.

STEPHEN HUGH-JONES, *The Palm and the Pleiades: Initiation and Cosmology in Northwest Amazonia*, Cambridge etc.: Cambridge University Press 1979 [Cambridge Studies in Social Anthropology, general editor Jack Goody, no. 24]. xvi, 332 pp., Bibliography, Index, Ills. £12.50.

There is hardly a single publication of Amazonian anthropology dating from before 1960 that one would consult nowadays for anything more than basic ethnographic facts. There are no Amazonian equivalents of *The Nuer* or *The Argonauts*, standing out from the early days of modern anthropology, which any contemporary social theorist would consider worth taking into account when formulating his own ideas. Amazonia lay outside the mainstream of all currents of anthropology, being little more than a source of anomalies that merely served to vex those concerned to establish universal principles of social organization. However, in the interim, the situation has been radically transformed. There is now a comparatively large body of basic texts that are at least known to specialists in other areas, even if one still cannot say that Amazonian material has been fully integrated into general theory. This efflorescence of Amazonian anthropology could not be attributed to many factors but two - one practical, the other theoretical - seem particularly important. First, since 1960 it has become progressively easier to get there and secondly, not long afterwards, the first volume of *Mythologies* was published.

Both factors, in their different ways, served to bring Amazonia to the attention of a wider audience of North American and European anthropologists than before.

The publication of the second edition of Goldman's now-classic monograph on the Cubeo, a couple of months before the appearance of the new books by Stephen and Christine Hugh-Jones (henceforth SHJ and CHJ respectively), dealing with the social and symbolic processes of the Barasana, was no doubt entirely accidental. Yet this fortuitous coincidence serves to highlight some of the developments that have taken place in Amazonian anthropology since the first edition of *The Cubeo* was published in 1963.

Although *The Cubeo* stands at the beginning of the modern period of Amazonian anthropology, it is nevertheless a retrospective work. Most simply, it refers to data collected during a relatively short fieldwork project in 1939-1940. Goldman explains that he had always hoped to carry out a further period of study but finally decided that this was unlikely, and so resolved to publish the first edition. However, it appears that he was able to revisit the Cubeo in 1970 and again in 1979 and some observations referring to these later visits form the basis of the Afterword, the only feature that distinguishes the second edition from the first. In the same section, the author refers to a new book that he is preparing on a different group of Cubeo from those he studied originally.

The Cubeo is also retrospective in a theoretical sense. In the Acknowledgements, Goldman identifies himself as a pupil of Franz Boas and Ruth Benedict and it is tempting to see their influence in the way in which he has chosen to organize *The Cubeo*. Some theoretical points are raised in the Introduction but the main thrust of his theoretical argument is reserved for the final chapter. This is entirely in line with the inductive method recommended by Boas: first present the data, then draw out the principles later.

It is not only the mode of theoretical presentation that seems anachronistic about *The Cubeo*: the same can also be said for the theoretical issues it deals with. One of the off-stage figures against whom the argument is directed (but who is never actually mentioned by name) is none other than Malinowski. Even the alternative view proposed by Goldman himself has an anachronistic ring: he suggests that Cubeo social structure can be derived from an elementary model'... based upon the solidarity of the male siblings [which] has been noted by Radcliffe-Brown as characteristic of primitive societies organized into patrilineages' (p. 284). Similarly,

If Cubeo social structure can be derived from an elementary model, so can its emotional structure, or ethos. The Cubeo ethos seems to be based upon the doctrine of harmonizing each particular mode of emotional expression with an appropriate social activity It is in ritual, as Ruth Benedict has demonstrated in *Patterns of Culture*, that the ethos of a culture is most vividly displayed (p.285).

To treat emotion in this way, as a sort of free-floating social variable, does not strike one now as at all convincing.

The approach of Christine and Stephen Hugh-Jones stands in total contrast to that of Goldman. Here the method is frankly hypothetico-deductive, as SHJ explains:

In carrying out field research ... we had two principal objectives in mind. The first was to make a general ethnographic study of a still relatively unacculturated Tukanoan-speaking Indian population. Our second objective was to examine some of Lévi-Strauss's ideas on mythology ... in the light of a detailed body of ethnographic data (SHJ, p. 14)

Both books are modified versions of the authors' PhD theses and are based on 22 months of fieldwork, most of it spent together in a community of about 30 Barasana on an affluent of the Pirá-Paraná. This river lies about 100 miles to the south of Mitú on the Colombian-Brazilian border, whilst the Cubeo live to the north and west of this frontier town. Although belonging to the same language-group and sharing many cultural attributes, the Barasana and the Cubeo are different in one major respect. Whilst the Barasana are integrated in the pan-Vaupés marriage system, involving several distinct groups, each speaking a different language, the Cubeo are linguistically endogamous.

The Hugh-Joneses present their books as complementary (as was the intention of Cambridge in publishing them simultaneously); in fact, each book could stand quite easily on its own, and there is surprisingly little overlap. Between them, the authors have managed a very effective division of labour. SHJ's book focuses on the inter-relationship of Barasana mythology and one particular ritual series concerned with male initiation. First, the rites are described with a minimum of analysis, then the meaning of the elements of which they are composed - the behaviour of the participants and the musical instruments, drugs and other material objects used - is systematically elucidated by reference to a substantial corpus of myths provided at the end of the book, and also to a diverse range of other types of evidence. This includes evidence culled from a long list of published works dealing with other Tukanoan societies. Throughout the book, the arguments of Lévi-Strauss stand in the wings; in the concluding chapter, however, he considers the relevance of the points raised by his study to the arguments, not only of Lévi-Strauss, but also, albeit briefly, to those of Victor Turner and Edmund Leach. The book is rounded off with an extensive bibliography of over a hundred sources of local ethnographic material.

In contrast to SHJ's book, CHJ's represents an attempt at a comprehensive account of Barasana society. On the other hand, it involves only a minimum of comparative reference. Even Lévi-Strauss receives mention only in the bibliography, though her intellectual debt to him is clearly very great. Instead, her argument is gradually built up by the most impressive marshalling of internal evidence relating to all spheres of Barasana life.

Every new item of information introduced, every interpretation proposed, takes her remorselessly closer to her final goal of isolating a formal model of the way in which the Barasana conceive of their social and physical universe, of their place in it and of their passage through it.

CHJ begins her analysis by confronting the paradox, noted also by Goldman, of the social organization of the Vaupés. The indigenous societies of this region are, in day-to-day practice, highly egalitarian; there is no division of labour apart from the fundamental one between men and women; chiefs enjoy very little coercive power; differences in productive capacity and hence of wealth are levelled out by a communal pattern of consumption. Yet the Indians' conception of their society is of one that is highly hierarchical in character.

The core of most Tukanoan settlements consists of a group of male siblings, members of an exogamous local descent group, living patrilocally. The relative seniority of these agnatic siblings is very important in the indigenous view and they are conceived of as being ordered in a hierarchy, consisting, ideally, of five roles: viz. chief, dancer/chanter, warrior, shaman, and servant. This model of internal relations between male siblings is also extrapolated to describe relations between residential groups. Thus the series of local descent-groups living along a river bank are conceived of as ordered into five similar categories and as making up a larger and also exogamous unit. CHJ describes this unit as a 'sib' and explains that its members believe themselves to be descended from an ancestral anaconda that first settled in the river at the beginning of time. These 'sibs' are thought of as being integrated into even larger entities, also structured according to the same quinquipartite principle. These CHJ refers to as 'Exogamous Groups', with an upper-case letter at the beginning of each word. Ideally, all the 'sibs' that make up an 'Exogamous Group' are to be found strung along the same river, with the most senior (i.e. that corresponding to the 'specialist role' of 'chief') nearest to the mouth of it.

Perhaps the most intriguing feature of the Vaupés social system is that these 'Exogamous Groups' are generally though not universally, 'language-bearing' groups. In other words, the rule of exogamy by which CHJ defines them, obliges the members of these 'Exogamous Groups' to marry someone who speaks a different language. But there is yet another, more general association, described by CHJ as a 'phratry'. The ranking of 'Exogamous Groups' within a 'phratry' is apparently rather loose and neither the ideal of geographical propinquity nor that of exogamy is strictly adhered to.

I must confess that I find the terminology that CHJ uses in this description rather unfortunate. She adopts the American usage 'sib' on the grounds that it is the term used by Goldman and Jean Jackson, who worked amongst the neighbouring Bará and who has published a number of articles on these people. I also find the use of the term 'Exogamous Group' (subdivided into Simple and Compound varieties) to describe an intermediate form of association standing between a 'sib' and a 'phratry', most confusing.

All the units involved in this discussion, from the smallest (the local descent group) to the largest (the phratry) are ideally exogamous, so I cannot understand why CHJ would want to reserve this term, in a special sense, for the intermediate form of association. The main difference between the units CHJ calls 'Exogamous Groups' and the units she calls 'phratries' appears to be that the latter are dispersed geographically and only vaguely recognized by the members. Perhaps therefore it would be appropriate to refer to these units as 'dispersed phratries' and use a term such as 'localized phratries' to describe the 'Exogamous Groups'. But this terminological awkwardness does not detract from the fundamental merit of this account, it being one of the most thorough, if not actually the first, discussion of this most interesting and puzzling social system.¹

Hand-in-hand with this account goes an attempt to explain the apparently paradoxical conflict between the hierarchical and egalitarian principles in the Vaupés social system. The specialist roles, it is argued, although hierarchical in one sense, are concentric in another, or to put it another way, are symmetrical in relation to the central role, that of 'warrior'. The warrior belongs to the 'externally oriented domain'; the roles on either side of him, the dancer/chanter role and the shaman role, belong to the 'metaphysical domain', whilst the chief and servant roles belong to the 'politico-economic domain'. CHJ claims that this balanced set of roles serves as a model, not only for relations within a residential group but also for relations between groups. She also points out a series of parallels between this model and the Indians' conception of the individual life-cycle. Finally after fifty pages of closely-argued text, she proposes that the hierarchical and egalitarian principles of the Vaupés social system should be treated as '... complementary aspects of the same structure - the five specialist roles - and thus they appear as transformations of one another' (p. 106). I must confess that I find it difficult to follow this argument, and I feel that this is because it is never exactly clear what the relationship between this ideal model of specialist roles and social reality is supposed to be, nor how the one is deemed to act upon the other.

The remainder of the book is dedicated to showing how the same formal model, combining the elements of concentricity and linearity, underlies indigenous conceptions of the human body and the spatial lay-out of the settlement, of the life cycle and its rites of passage, of the oscillation of the soul between life and death, and of the relations between the sexes and their respective

1. See also two papers by Jean Jackson, 'Language Identity of the Vaupés Indians', in R. Bauman & J. Sherzer (eds.), *Explorations in the Ethnography of Speaking*, Cambridge: Cambridge University Press 1974, pp. 50-64; and 'Vaupés Marriage: A Network System in the Northwest Amazon', in C.A. Smith (ed.), *Regional Analysis*, New York 1976, ii, pp. 65-93.

duties in economic production and consumption. In a final *tour de force*, CHJ draws all her earlier arguments together in an attempt to show the fundamental homology between the native models of all these various domains of experience and their conception of the structure of the universe. This cosmic order, which has both a vertical and a horizontal dimension, is thought to have been laid down at the beginning of time by ancestral beings, as recorded in their myths. The mythic era however continues to exist as an alternative aspect of the present and is the source of power that allows living beings to reproduce themselves socially and physically, generation after generation. Contact with this source of power is achieved through manipulation of the concrete physical and social world of the present during shamanic sessions or during ritual. Since this world is derived from the ancestral world, it also provides the way to it. Thus by moving through horizontal space and cumulative time in this world, the Indians believe they enter the ancestral world of vertical space and reversible time that is the source of being.

The image that emerges of the Barasana from CHJ's book is of a people whose lives - from the most banal physical process to the most embracing cosmological concept - are very tightly controlled by this underlying structure. The author is careful to point out that she is talking about 'ideal' models and that in reality people often act in ways that do not conform to them. Even so, this book will be more amenable to those who are accustomed to the structuralist style of argument than to those who are not.

This is not the place to discuss the merits or otherwise of the structuralist method. However one question of general theoretical concern is the psychological status of the model CHJ perceives to be underlying the reality she describes. The Indians themselves are unconscious of the model, at least to the extent that they are unable to articulate it in the formal and abstract terms that CHJ herself does. The binary oppositions that Lévi-Strauss identifies as the substratum of so much Amerindian thought are also apparently unconscious in exactly the same way. For his part, Lévi-Strauss appears to claim that these oppositions are a fundamental property of the human mind; thus all concrete manifestations of human thought from totemic taxonomies to marriage systems will reveal these fundamental oppositions. However, no such claim can readily be made for the quinquipartite schema that CHJ has identified, thus leaving the question of the relation between this model and social reality hanging in the air. Although it was a wise decision perhaps to avoid such a general issue in a book that is essentially a case-study, I feel that before one can be fully convinced by the conclusions that CHJ reaches, one would need to be satisfied with regard to the relationship between model and reality that her conclusions imply.

If CHJ's book is impressive for the marshalling of internal evidence, SHJ's is impressive for the marshalling of comparative evidence from other sources referring to the Vaupés region. Yet, once again, as with CHJ's work, one can admire SHJ's book as an exemplar of the structuralist method whilst remaining sceptical about the method itself. Skilful use of the Lévi-Straussian

style of analysis permits SHJ to identify a meaningful order underlying not only the seemingly random events of Barasana male initiation rituals but also the fantastical episodes of Barasana mythology. It is by indicating the relationship between the two that SHJ attempts to demonstrate the true meaning of each: taken on its own, he argues, neither Barasana ritual nor Barasana myth would be comprehensible.

However, anyone concerned about the canons of validation that should be applied to structuralist analyses will find plenty to think about here. It is noticeable, for example, that the quinquipartite schema that plays such a prominent part in CHJ's account of the Barasana is hardly present at all in SHJ. In fact, there is a certain lack of consistency between the two accounts with regard to the underlying model. Whereas the latter distinguishes five life stages and identifies them with the five specialist roles, SHJ distinguishes only four (cf. p. 148 in SHJ and p. 65f in CHJ on the analogy between specialist roles and life-cycle stages). Moreover, although SHJ cites his informants in support of his arguments on some occasions, he reserves the right to contradict them on others, on the grounds that 'all native explanations ... should be treated as part of the data to be explained and not as anthropological explanations in their own right, a point made long ago by Radcliffe-Brown' (p. 254). Although the book is ostensibly an examination of Lévi-Strauss's ideas in the light of the Barasana evidence, it is just as much an examination of the Barasana evidence in the light of Lévi-Strauss's ideas. It is somewhat disconcerting to discover that in a book in which Lévi-Strauss's ideas are supposedly to be put to the test, that they are invoked to confirm and sometimes even to establish certain symbolic associations.

From a methodological point of view, the Hugh-Jones' books are the antithesis of Goldman's. Whereas Goldman's book consists largely of innocent description with some theoretical discussion front and back, their books represent self-conscious attempts at cultural exegesis from a clearly 'privileged' theoretical perspective. As such, they both testify to the transformation that has taken place in Amazonian anthropology over the last decade and a half. In another sense though, CHJ's book, in aspiring to be comprehensive is an exception to the general trend. This has been to produce monographs, like SHJ's, in which one particular feature of the society is examined in great depth. We are fortunate in this case to have both perspectives on the Barasana. A comparative synthesis is rare. The most recent attempt to produce a concise but comprehensive account of the peoples of the Amazon Basin was Steward and Faron's *Native Peoples of South America*, published in 1959. Even this book however was little more than a *précis* of the *Handbook of South American Indians* produced in the late 1940s. Far from becoming more integrated into the mainstream of general anthropology, Amazonian anthropology seems to be breaking up into smaller and smaller parts. Not only is it difficult to accommodate Lévi-Strauss with Victor Turner, but it is even difficult to integrate one South Americanist's

work with another or even one area specialist's work with another. Yet perhaps the period demarcated by the books reviewed here represents the most fruitful period in the anthropology of aboriginal Amazonia so far. It may well turn out to be the most fruitful period ever - if the fecundity of a particular period is to be regarded as dependent on increasing quality in the information collected, rather than merely on the progressive elaboration of theoretical constructs on an already-established 'data-base'.

PAUL HENLEY

II

DAVID MAYBURY-LEWIS (ed.), *Dialectical Societies: The Gê and Bororo of Central Brazil*, Cambridge, Mass. & London: Harvard University Press 1979 [Harvard Studies in Cultural Anthropology, general editors D. Maybury-Lewis *et al.*, no. 1]. xiv, 340 pp., Index. £15.00.

Dialectical Societies is a collection of essays by members of the Harvard-Central Brazil Project, a research programme conducted among six Central Brazilian societies in the 1960s. The volume is of interest both as documentation of an area of recent anthropological attention (it is the myths of these societies that form the foundation of Lévi-Strauss' *Mythologiques*) and as an illuminating illustration of 'comparative' anthropology at the present time. The book is fittingly dedicated to Curt Nimuendajú, a self-trained German-Brazilian Indianist and the authority on Central Brazilian societies during the first half of this century, who established the importance of comparative studies in the region. *Dialectical Societies* continues Nimuendajú's ambitions, though, in the intervening years, the study of a number of cultures from a general perspective by one individual has been replaced by the intensive study of facets of a single society by one or more specialists. As the stated ambitions of the volume are comparative - ethnography is reduced to the minimum necessary to sustain the theoretical arguments - I will devote my attention to the work as an exercise of comparative anthropology rather than attempt to assess individual contributions.

Within the large and varied region of Central Brazil, *Dialectical Societies* focuses on a group of societies speaking the Gê language. The term Gê refers to a language stock though, unfortunately, the details of this classification have not been totally resolved. The category has, however, tended to be associated with a culture area, particularly as cultural and language boundaries approximately coincide in this area of South America. As the first major work on this region since the description of the Gê in the

Handbook of South American Indians, compiled in the 1940s, it is unfortunate that *Dialectical Societies* does not include a reappraisal of the whole question of Central Brazilian culture and a re-examination of the utility of the Gê category. Thus, if we take the common features of Gê-speaking societies - matri-uxorilocal residence, an emphasised ideological and spatial separation of 'male' and 'female' categories, community-wide associations, village morphology etc. - then a number of non-Gê-speaking societies should also qualify; the Tapirape and the Caraja are two examples that spring to mind. The demarcation of a Gê-speaking culture appears to rest on two related assumptions. First, that all present Gê-speakers are the remnants of a society active in the unrecorded past and, secondly, that non-Gê speakers exhibiting Gê characteristics are explicable as a result of diffusion where the society in question has been in prolonged contact with Gê-speakers. Whilst both of these hypotheses may possibly be valid, it would have been interesting to have included an examination of a non-Gê-speaking society in the volume particularly as the Nambikwara, of an isolated language stock, were included at the research stage of the project. My point here is not that a comparative study need necessarily include all relevant societies but rather that the exercise is reduced in value if only those societies with a suspected common origin are included. If we are concerned with a common 'kind' of social structure, as the essays suggest, then surely it is more meaningful to include representatives of different language-groups inhabiting the same region, since, even if these examples support the theory of diffusion, we might be able to obtain wider and better-founded understanding of this particular form of social structure.

The philosophy behind *Dialectical Societies* appears to be that detailed analyses of specific societies will lead to the formulation of generalisations of comparative value. One result of this approach is that the individual interests and the methodological preferences of the contributors, together with the particular emphases of the society under study, produce analyses directed at the same question - the understanding of Gê social structure - but from distinct theoretical viewpoints and concerning distinct facets of the societies under discussion. Whilst this approach may not present critical problems for the informed specialist reader, a non-specialist may well have difficulty in following the often complex arguments. Moreover, the individualistic presentation often prevents the reader from pursuing leads of his own, or indeed those offered by the contributors, as vital material is often absent. To take one example: the volume is focused on the question of social structure yet there is no uniform presentation of relationship terminologies; comprehensive details are given for three societies, minimal information is given for two and incomplete data are given in the text for one (the Bororo). One notable and, perhaps, indicative exception to this is the apparent collaboration by the two Brazilian representatives of the project whose contributions permit the reader to draw direct comparisons between the two societies in question.

The individualistic form of presentation is mirrored by the conclusions and generalisations offered by the authors- for example, we find four separate 'explanations' for the prevalence of matri-uxorilocal residence among the Gê. The question is whether this is a reflection of the societies under study or of the anthropologists involved. These problems appear to rise from the very nature of the project in that specialisation and the intensive analysis of one society tends to obscure over-all comprehension. Thus each contributor tends to weight his generalisations in terms of his 'own' society. This bias is apparent in Terry Turner's otherwise extremely stimulating analysis of Gê and Bororo social structure - the only comprehensive attempt included in the volume to consider the social structure as an abstract system. Turner's argument that it is the dominance of the father-in-law over his son(s)-in-law that holds the key to an understanding of Gê and Bororo social structure (and in particular of matri-uxorilocal residence), appears to stem primarily from his own experience among the Kayapo where the role of the father-in-law is emphasised. David Maybury-Lewis points out in the concluding chapter that this generalisation is not supported by all Gê societies and, from my experience with the Panara (a Gê-speaking society contacted after the completion of the Harvard-Central Brazil Project) I have to concur. Even if we take 'dominance' at its widest meaning, Turner's suggestion cannot be supported by the Panara example, where it would be more accurate to consider both father-in-law and son-in-law as sharing the common dilemma of uxorilocal residence in the same household. Whilst such details need not detract from the general value of Turner's thesis - in particular his emphasis of intra- and inter-family relationships - it does illustrate the problems inherent in moving between the levels of intensive and personally-experienced knowledge of one society and that of generalised comment on a number of societies documented by other anthropologists.

The comparativist ambitions of this volume could possibly have been enhanced by a greater concentration on specific themes. All the contributors stress the importance of domestic households which form a ring around the periphery of Gê villages in conjunction with matri-uxorilocal residence, and this could provide a suitable focus as Turner's essays demonstrate. Peripheral households are a domestic and female domain, concerned with the production of 'raw materials' - children and food - in the processes of production and reproduction. It is also in this domain that the relationship terminology emerges as a dominant classificatory principle. The failure of the contributors fully to explore Gê relationship terminologies may in part result from the difficulty of applying the traditional 'tools' of 'kinship' to these societies, though this need not necessarily mean that the relationship terminologies are in some way peripheral to an understanding of social structure or, for that matter, to a general comprehension of the Gê. In this respect it is unfortunate that the volume does not contain a comparative examination of Gê relationship terminologies (or Central Brazilian relationship terminologies) although one contributor has advanced

a tentative scheme elsewhere.¹

In fact, rather than moving towards synthesis on this important question, the volume leaves the reader with a rather awkward picture where differences are emphasised. Thus a cursory reading of the chapters in the Northern Gê and that on the Central Gê may well result in the impression that the latter have a 'two-line' terminology (though referred to as a 'two-section' system), whereas the former patently do not. Does this imply that the presence of a 'two-line' terminology has little bearing on the social structure which, among other things, would question the possible significance (discussed elsewhere by Rivi re) of the distribution of such relationship terminologies in Lowland South America? A closer reading shows that the Central Gê do not in fact have a 'two-line' relationship terminology and that the categories are presented in a 'two-line' block-diagram for the purpose of emphasising the importance of an 'us'/'them' or 'kin'/'affine' distinction in the societies. This presentation tends to play down the fact that the Central Gê have distinct affinal terms for wife's mother and wife's father and also distinguish matrilineal relatives from within the general category of affines; in point of fact there is a prohibition on marriage into the latter category of relatives which makes the application of the term 'affines' rather obscure.

Rather than stressing the differences between the Central and Northern Gê, it would seem more in keeping with the general aims of the volume to emphasise the similarities and, more specifically, the fact that all Central Brazilian societies have what can best be termed a 'three-line' terminology, comprising distinct categories of patrilineal, matrilineal and affinal relationships. The pertinent question is why the Central Gê should lack a complete 'two-line' terminology when they have all the hallmarks of such a society: for example, an extension of relationship terms to the level of tribe, and a fundamental 'us'/'they' dichotomy. This question is too complex to pursue here though we can suggest that it has a bearing on why the Central Gê practise matri-uxorilocal residence in spite of an ideology that is frankly patri-virilocal.

A possibly significant feature of the 'three-line' terminology is an application of the principle of generation to the category of 'affine'. In effect ego distinguishes two categories of affines; through the first ascending generation those of one of his parents (in the case of the Central Gê this would be matrilineal relatives and in the case of the Bororo or Panara, patrilineal relatives), and through his own generation, his wife and her relatives. Each Central Brazilian society accords a different weighting to the relationships between these three categories; the matrilineal and patrilineal categories may be approximately balanced or may reflect 'patrilineal' or 'matrilineal' biases; the identification with a category focused on a parent may diminish in time and be

1. See Roberto Da Matta, *Um Mundo Dividido: A Estrutura Social dos Indios Apinay *, Petropolis: Editoria Vozes 1976.

replaced by identification with the wife's relatives, or the parental relationships may endure.

However, in all these societies we find a common ideology of the individual as comprising a social and a physical self with the distinction expressed through the ideology of matrilateral and patrilateral roles. This suggests that Turner's hypothesis regarding the dialectic between the identity of the family and the necessity of families to 'reproduce', deserves more extensive examination. Finally, the social structure of Central Brazilian societies can be interpreted as articulating a common ideological principle that is given diverse expression in the form of dichotomies.

My own view is that these societies appear to conceptualise productive processes as the alternate antithesis and synthesis of dyadic categories. To give an illustration, the life-cycle can be represented as a process involving the differentiation of 'male' and 'female' (for example, men's house residence, sex-determined economic roles etc.) alternating with the synthesis of 'male' and 'female' (as in marriage, procreation, economic cooperation etc.). The 'three-line' relationship terminology would appear to express the ongoing nature of this dialectic; i.e. that the individual should not contract a union with a spouse associated with either parent. Rather, the individual should marry a representative of the category 'male' or 'female' who is 'uncontaminated', so to speak, by previously-established relationships - who is, in other words, a distinct 'affine'. This contention is apparently supported by the general impression that the confusion of affinal roles with matrilateral or patrilateral roles - depending on which society we are considering - is undesirable. Interestingly, it is only among the Central Gê that we find a prohibition on marriage into one of these categories. As a more general comment we can also note that according to this ideology the initial phase of marriage at least should be considered as an essentially 'biological' union, between representatives of the 'male' and 'female' categories directed at reproduction, rather than as an alliance between two groups.

Many of the characteristics of the Gê appeared to be fairly anomalous before this series of studies. Certainly this volume has given us some insights into how these features fit together and in this respect the work is aptly titled. Regretfully the features have still not been accounted for in a comparative framework. The study of matrilateral, patrilateral and affinal categories offers one of the most intriguing aspects of the Gê and although reference is recurrently made to this distinction no attempt is made to discuss the significance of this anomalous feature either from the Gê or, for that matter, in social anthropology generally.

RICHARD HEELAS

BOOK REVIEWS

JAMES L. WATSON (ed.), *Asian and African Systems of Slavery*, Oxford: Basil Blackwell 1980. 348 pp. £9.95.

Slavery is not an easy category to dissolve. Whatever the admittedly striking variations in the institution in time, place and language, and however scientifically suspect the moral or emotional load that the word may carry, it will not go away. There is a gritty residue which survives the widest cross-cultural application, as it defies the most acid of scholarly attempts to define it away. Some writers are nervous of using the word slave, fearing charges of ethnocentrism or of too firm a political or moral judgment upon the myriad forms of subordinate status offered by the ethnography of the world. The editors of a recent American collection of papers on slavery were so anxious to avoid commitment of this kind that they decided to use the word always in inverted commas - thus having to refer, absurdly, to the Atlantic, trans-Saharan, and eastern African 'slave' trades (S. Miers & I. Kopytoff (eds.), *Slavery in Africa*, Madison: University of Wisconsin Press 1977; p. 69). If ever a slave were unambiguously a slave, and not to be called by some such name as 'acquired stranger of marginal status', it was surely so in these contexts. Academic over-scrupulousness of this kind, supposedly in the interest of value-free comparison, neutralizes the language to a point where there is nothing to compare. Among many non-senses in Miers & Kopytoff's 'Introduction', we might mention the analogy drawn between the separation of a person from his original society, and his transportation and incorporation into another as a 'slave', with the *rites de passage* accompanying normal changes of status (p. 15); or the assertion that to see institutions of 'slavery' in Africa only in response to outside stimuli is 'to deprive the African past of internal economic dynamism, inventiveness, entrepreneurship, and, above all, of its fundamental cultural concepts of rights-in-persons' (p. 67).

The present collection, the first of its kind to stem from the British anthropological tradition, does not suffer from academic artificialities of this kind. The fact that the various authors, in treating of slavery and related institutions in a variety of local contexts in Africa and Asia, do not shy away from using the term does not mean that they are insensitive to the problems of definition. James Watson's own paper offers a most careful analysis of the difference between various categories of bought children in south China, restricting the use of the term slave to the hereditary status of the *sai man*, who are attached to important families. In his 'Introduction' Watson takes his bearings from the

pioneering work of Nieboer (*Slavery as an Industrial System*, The Hague 1900) and Professor Moses Finley (see especially his article on the subject in the *International Encyclopedia of the Social Sciences* and his *T.L.S.* article 'A Peculiar Institution?' of 2 July 1976). Watson develops a definition, or rather a 'set of guidelines for a definition' of slavery, emphasising that slaves are acquired by purchase or capture, their labour extracted by coercion, and as long as they remain slaves are not accepted into the master's kinship group. This is a great improvement upon Miers & Kopytoff, who pay little attention to the origin of slaves, focusing only upon their host society, and seeing little 'coercion' in the African variety of the institution but rather a benign continuum from the status of 'slave' to that of 'kinsman' as the stranger is gradually incorporated (after all, kinsmen, in their analysis, are bought too - through bridewealth etc.). In spirit, Watson's approach is much closer to that of Claude Meillassoux and his collaborators in the other recent collection on African slavery (*L'Esclavage en Afrique Précoloniale*, Paris 1977). It is true that Meillassoux emphasises much more strongly the original uprooting, separation from his own society and depersonalisation which the slave or his ancestors have necessarily suffered - and also the consequent close links between the internal relationships of a slave-holding society firmly to its external relationships with other societies. An appreciation of the external structural relationships of a slave-holding society is implicit in the analysis of several contributors to the present volume (for example Bloch on Madagascar, Gill Shepherd on the Comoros, Burnham on the Fulbe states of what is now Cameroon). Another aspect of Meillassoux's treatment of African slavery is his insistence that the physical reproduction of the slave is everywhere controlled, whether the labour of slaves is used productively or not. He is not free to marry and found a family as he chooses, in other words; and this point again appears in many of the individual papers in the present volume but is not developed by Watson to the extent that it might have been. Goody for example gives striking evidence that in Africa the price structure always favoured eunuchs and women above men - the opposite of the price structures of the New World, where able-bodied men were the most valuable. Whether an indigenous African or Asian system of slavery allowed marriage or concubinage with the slave population or not, the element of control and exploitation in this matter does seem to be of major importance, and even to outweigh the coercive use of slave labour as a general feature.

The present volume brings together a number of papers originally presented at a University of London seminar, and by covering cases from both Asia and Africa most usefully extends our geographical base of comparative thinking. The individual studies, moreover, are not presented as isolated instances but are often set in their own regional context. Perhaps one of the reasons for the relative neglect by anthropologists of the topic of slavery has been, in addition to the obvious lack of a historical dimension in much of their work, a tendency to focus too closely on one community at a time rather than to take a broader regional view of

a series of inter-connected communities. The clearest example in this collection of a regional treatment is Burnham's analysis of the nineteenth-century expanding circle of raiding, enslavement and political domination by the southern Fulbe kingdom of Ngaoundere upon the surrounding peoples. Upon assimilation into the expanding circle, the peripheral communities themselves began to raid and enslave neighbours even farther out, and internal structures of kinship, slavery etc. within any particular community were directly related to their position within the regional system. This case is a clear example of the 'open' mode of slavery put forward in Watson's 'Introduction' as a type distinct from the 'closed' mode.

The open mode is a system in which there are many routes to emancipation, either through manumission or through various mechanisms for freeing children of the next generation - for example those of slave concubines and free fathers; there is therefore a need to recruit more people from outside to maintain the slave population, and so this kind of society may often raid into neighbouring peoples. The closed mode, by contrast, is a system in which the recruitment of the slave population is largely internal - for example from those who commit serious offences, fall into debt or sell their children out of desperate need. The status of slave is passed on from one generation to the next and there is little or no hope of manumission. In this case, furthermore, there are likely to be permanent barriers against any unions between the free and slave population, and so no question of a rising generation with some claim to freedom. The clearest cases presented here of this closed mode of slavery are perhaps those of Nepal and Hindu India ('Power and Status in South Asian Slavery', by L. Caplan) and of south China (Watson's own paper on 'The Chinese Market in Slaves, Servants and Heirs'). In these cases there was no road to becoming a freedman, with all the opportunities that that might imply in the more fluid political societies of the Islamic frontier in Saharan Africa, for example, or even in the ancient world.

Watson is properly cautious about terming these two modes the African and Asian modes respectively, partly because it is clear that the systems of Madagascar described by Maurice Bloch resemble the 'closed' model, and those of Thailand described by Andrew Turton resemble the 'open' model, with the slave population of the Thai states being drawn partly from a predatory relationship with the surrounding stateless societies. But Watson does nevertheless put forward the idea of a general contrast between Asian and African systems. He defends this to some extent on the grounds that the differences correspond to different concepts of property in the two continents. Land was at a premium in Asia; whereas (following Goody) in Africa, control of land was less important than control of people and their absorption into local communities. Watson here accepts the Miers and Kopytoff view of the African lineage as having rights, corporately, over its own members, and being able to negotiate these rights, as in bride-wealth or by extension in the buying of outsiders who will eventually become kin. Generalisations of this kind, however, not

only underplay the reality (and often quasi-permanence) of slave-status in Africa but also the crucial distinction made very widely by African peoples between bridewealth and market transactions in which slave-dealing is included. Moreover, the 'assimilative' approach neglects the important historical fact that the African continent, for as long as we have records and beyond, has been a source region, a reservoir of people, drawn inescapably into the world's long-distance slave systems. The Nile Basin for example has been drained of its population by the slave trade for some three millennia. The Saharan trade has drained the African interior for many centuries, and the ravages of the Atlantic and east African trades are too well known to need elaboration. There are few parts of Asia in which patterns of 'indigenous' slavery have to be set against this sort of background. In most of the African cases in the present book (as well as those in the other two collections referred to) it is quite clear by direct statement or by implication that any one local system of 'slavery' is part of a bigger jigsaw, and that at least one factor in the local development of slavery has been the presence of long-distance trade. The African systems have often appeared as long parasitical chains; and to no small degree the 'open-ended' systems of Africa must be seen, especially on the Islamic frontier, as a response to long-distance exploitation.

It is helpful, I consider, not only to see Africa on a continental scale as having been a source region in patterns of slave-trading, but also to look more closely at particular societies or smaller regions which have had to live through extended periods at the wrong end of the slave-trade within Africa - as net contributors, rather than gainers, of slave population. This perspective would make possible a much more subtle comparative analysis; for comparisons to date have been of societies as holders of slaves, as 'host' societies, or centres of spreading domination. But to focus on the source of slaves, those societies from which they are uprooted and to which they occasionally even return, would give a more balanced picture. Sociologically, as Lévy-Bruhl observed in a perceptive essay which is seldom referred to ('Esquisse d'une Théorie Sociologique de l'Esclavage à Rome' [1931], in Moses I. Finley (ed.), *Slavery in Classical Antiquity: Views and Controversies*, Cambridge 1960), slavery is an *international* phenomenon. Slaves always come from somewhere else - if their origin is traced back far enough. Although, as the present volume illustrates, in Asia, slaves were frequently drawn from close to home, it is significant that an element of detachment is still present: for example in the south China case, the buyer and seller of an infant remain ignorant of one another, and the sale is conducted by an intermediary. The communities from which the slaves are drawn, even when close to the 'host' family, are surely stamped by this fact; Watson even writes of weeping women who had lost children in the Chinese trade. In regions where the worst forms of long-distance slave raiding and trading have depopulated whole zones (as in the Middle Belt of West Africa) it is inconceivable that the forms of culture and society found today do not bear the imprint, and have not been significantly transformed by this history. This is cer-

tainly the case among the peoples of the Sudan-Ethiopian borderlands, for example. It may be that on a continental scale, the main lines of difference between Asia and Africa in their indigenous modes of slavery could be profitably explored in this way.

A further topic on which there could be more analysis of slave-based or slave-holding societies is that of the position of 'freedmen', as distinct from the ordinary free population. The distinction was of extreme importance in the ancient world, and I believe still is so in many parts of the African Islamic frontier. Finley has pointed to the interesting paradox that it is from those societies which practised forms of the most severe exploitation of slaves that have sprung some of the noblest conceptions of freedom - such as those of Greece and modern America. When laws are drawn up for the emancipation of slaves, freedoms are actually defined for the first time - freedoms which probably are not possessed by the ordinary citizenry who are still burdened by obligations of family, lineage, property and so forth; the *libertas* of the noble Roman citizen was a different matter from the *personal*, individual freedom of the emancipated slave. In these terms we might approach the extraordinary phenomenon of political power, even the foundation of new dynasties, associated with slaves or recently freed slaves in the Saharan kingdoms. This paradox appears clearly in the slave associations of the royal Funj of old Sennar, and their Hamaj successors. In old Sennar, slavery was 'the greatest nobility' according to James Bruce. The present volume does not offer much on paradoxes of this kind, but it is an important and timely publication which brings us closer to a properly historical perspective on the peoples we study.

WENDY JAMES

DAVID J. PARKIN, *The Cultural Definition of Political Responses: Lineal Destiny among the Luo*, London: Academic Press, 1978

There is an important and provocative book behind this bewildering title. Based on field work conducted mainly in a Nairobi housing estate in 1968-9, the study shows how Luo migrants from western Kenya maintained a distinctive cultural identity in an environment of diminishing political and economic opportunities.

The introduction and conclusion address an ambitious variety of theoretical questions. Arguing that the 'superstructure' of culture can retain its autonomy and its character in the face of such changing conditions as urbanization and increasing wage dependency, Parkin weaves together his thoughts on *ethos* and *eidos*, illocution and proposition, structure and *communitas*, etic logical necessity and emic logical consistency, and levels of homology or opposition in semiology, to pick only a few of the issues. Inevitably, some teasing threads are left untied, but the final product

is one of commendable scope, and no one will call it pedestrian.

Although Parkin believes that culture is more a matter of continuity and order than one of flux and disorder, he suggests with his ethnography that urban Luo culture is now a mixture of two opposing sets of ideals and expectations. One favours the small monogamous family unit, high levels of education, and individual achievement for men and women; the other favours the polygynous family (traditionally the conceptual model for segmentary patrilineages of all sizes), solidarity with wider circles of kin, and the authority of elder men. Findings of such value conflicts are now common enough in the literature of urbanization and social change. But Parkin's study is particularly notable in showing how the same individuals switch from one ideology to the other, using emotionally charged 'key terms' to set the tone of their relations one way or the other, according to the intimacy or formality of the social encounters in which they are engaged.

The schism between old and new values pervades urban Luo life: Luo-speaking youth gangs are contrasted with English-speaking students' associations; a Luo bar with minstrel singing is contrasted with a polyethnic bar with a juke-box; hospitality for kin is contrasted with hospitality for unrelated friends; and the history-conscious, country-based leadership of Oginga Odinga is contrasted with the westernized, city-based leadership of Tom Mboya. The ethnographic body of the study is rich and broadly cohesive, and yet it does not appear to be unduly schematized.

In its main theoretical underpinnings, Parkin's book represents a significant swing of the pendulum back towards a certain functionalist mode of interpretation that has been unfashionable for some years. Luo culture, he argues, has 'a relatively high degree of autonomy as a self-regulating system of key terms, ideological assumptions, and practices' (p. 288). He singles out high rates of polygyny, high bridewealth payments, and the hierarchical, 'poly-segmentary' lineage structure - this form of classification being emulated, as he shows, in the structure of urban ethnic associations - as crucial elements of the old order, all closely integrated. I suspect that some readers, even if agreeing with his use of the term 'self-perpetuating' in connection with these institutions, will nevertheless balk at his use of the term 'self-regulating' with its added suggestions of organic integrity and balance. Parkin is careful, however, not to project his conclusions onto other societies in Kenya or elsewhere. 'Cultures may differ', he writes, 'in the extent to which their constituent institutions are related in a logically consistent way' (p. 20). How much fruitless controversy might anthropologists avoid among themselves if more of them thought this way?

Parkin successfully demonstrates that the features he selects as the critical elements of traditional Luo culture are all inter-related, but his implications about the causal arrows in the system are perhaps more open to question. The persistence of polygyny, he argues, presupposes the segmentary lineage structure, which in turn depends on a conceptual distinction between men's and women's roles (see pp. 8-9). (One may note here that there is a multitude

of East African societies that have polygynous marriage but do not have segmentary lineages. In some cases polygyny has persisted while segmentary lineages have disappeared.) I, for one, am left with the feeling that Parkin is sometimes treating ideology and attitudes as foundations for behaviour and conventions, when the case might be better stated the other way round, or as a varyingly circular chain of cause and effect. The use of 'key terms' is treated more as an action of social control than as a reflection or expression of conflicts that are occurring at a more basic level. Parkin writes,

At present men successfully contain this ambivalence within the status of women [as mere producers of men's offspring, or as urban wage-workers in their own right] through controls which draw much of their efficacy from the use of customary verbal concepts in domestic and political speeches (p. 30).

Surely these terms and concepts will have some bearing on how men and women continue to behave toward each other, but one cannot help wondering whether, in some of these propositions, Parkin may have the tail wagging the dog.

The study treats cultural self-perpetuation as being, for the Luo, a desirable end in itself. Parkin suggests that among the migrant population, this continuity, as seen in distinctive patterns of marriage, filiation rights, and lineage structure, may serve to conceal the disjunction between the agricultural and the industrial modes of livelihood. More importantly, he suggests that this continuity may shape the migrants' reactions to the political problems they face in a city where they are not the dominant group. Parkin finds that the Luo see cultural survival in terms of increasing numbers, something they can best achieve through the old polygynous and strongly unilineal family pattern. This, as I understand it, is what the title means.

Since the book was published, the administration of Kenya's President Moi has made great strides toward reconciling the large Luo minority with the Kikuyu and other powerful groups, restoring several Luo leaders into top-level government positions and seeking to bring about an atmosphere of fairness and cooperation. It will be interesting now to see whether the trend of decreasing opportunities that Parkin describes for the Luo will be reversed, and whether some of the distinctively ethnic customs and institutions by which Luo urban migrants have maintained their solidarity and their integrity will begin to seem less necessary to them as a result.

Unfortunately the book lacks the sparkling clarity of exposition that characterizes much of Parkin's other work, notably *Palms, Wine and Witnesses* (London: Chandler, 1972). True, the theory of *The Cultural Definition* is more complex - arguably more sophisticated - and so may require more words and more repetition. But there are many instances, particularly in the introduction and conclusion, where strings of lengthy sentences with abstract terms tend to obscure the points. This is only a minor criticism, however, and the reader who perseveres will get more than his money's

worth. As this book is expensive (£14.80), this says a good deal.

PARKER SHIPTON

JACQUES MAQUET, *Introduction to Aesthetic Anthropology*, Malibu: Undena Publications 1979. 108 pp.

Introduction to Aesthetic Anthropology is an attempt to extend the idea of the aesthetic to the world outside the boundaries of the western art discourse. The book is divided into two segments: 'The Aesthetic Phenomena' (chapters 1-11) and 'The Anthropological Approach' (chapters 12-20). The first delimits the western idea of art as referring to a set of objects (sculpture, painting and drawing) which is produced to be 'used in one way only, to be looked at' (p. 10). These objects, in western societies, tend to be isolated in the context of the artistic establishment; a domain made up of art markets, art collections and art museums. Maquet's intention is to return aesthetics to the wider world in the dual sense of affirming its existence in the quotidian realms of life and establishing its presence in non-Western societies. The aesthetic is shown to exist outside of the artistic establishment; although the book is weak on particular illustrations, Maquet does mention the formal grace of the Japanese tea ceremony, the elaborate knot-work of Bororo-Fulani saddle straps, and the intricately carved masks and figurines of African ancestral cults as examples of aesthetic activity outside the western context.

Maquet does not, however, transcend the western idea of the art work as an object isolated from the mainstream of life so that it can be contemplated as a thing in itself. The second section, which is devoted to proposing a method for the study of aesthetic phenomena in culture, fails to pull its subject loose from its western frame. Most of the chapter enunciates the cultural materialist approach to cultural phenomena. This approach, which is criticized below, serves in the context of an 'aesthetic anthropology' to justify treating an aesthetic artifact as an object which is produced to fulfil man's universal need for contemplation. The section advocates the delineation of an 'aesthetic segment' of culture and attempts to prove that objects within this segment exist solely to satisfy that need. The 'framing' of the western art work is thus extended to the aesthetic domain in large. The fact that Maquet's two extended applications of his approach deal with western phenomena, socialist realism and protest art, seems to affirm the ethno-particularity of his concept.

The rest of this review is a criticism in particular of Maquet's idea of the aesthetic segment and in general of the concept of art as an autonomous domain. It will contend that the term 'aesthetic' is particular to our own culture and cannot, therefore, be used as a means of delimiting a particular aspect of another culture's

production. To open a study with the question, 'what in a culture is aesthetic and how does it relate to other aspects of the culture?' is to presuppose that the aesthetic refers to a universal domain of valued experience. Such a start assumes that the aesthetic relates to other realms of experience in some sort of pluralist manner; it either reflects life, presents an alternate vision, or rejects the secular in its striving towards universal form. The possibility, which Arnold Hauser in *The Social History of Art* comes nearest to asserting, that the realm we would term the aesthetic is continuous with and indivisible from the 'discourse' of a society is hidden by the initial definition. An anthropologist cannot carry his own culture's conceptual baggage with him if his visit to another culture is not simply to prove that everywhere we find the same in different guise.

Maquet's opening gesture, performed in the interests of the 'empirical' discipline of anthropology, is to sever aesthetic artifacts from the belief systems which motivate their production and ground their affective power: 'the mental state of aesthetic awareness, inaccessible to anthropological observation, is translated into an objective quality - the aesthetic quality - which is located, as it were, in the visual form of the object' (p. 19). What is significant to the anthropologist, Maquet contends, is the 'non-instrumental features' of the aesthetic object. These are aspects of an artifact such as ornamentation or precision of form which do not pertain to its practical or symbolic functions. Such palpable evidence of aesthetic impulses frees the anthropologist from the necessity of studying their significance to the producers or possessors:

Defining an aesthetic quality of an object by its non-instrumental form aims at converting a subject-object relationship (the subject's contemplative awareness stimulated by the object's form) into a quality of the object itself (its non-instrumental form) The observer need not refer to the subject's state of mind; he may reach his conclusion *exclusively* [my emphasis] by examining the artifact (pp. 29 and 21).

Maquet posits the existence within culture of an 'aesthetic locus'. A reader hoping here to find an association of production and belief will be disappointed to discover no more than a tautologous definition which not only fails to attend to cultural reasons for 'marking' certain domains more distinctively than others, but which provides no resistance to the outsider's temptation to impose his own categories on the elements of a people's material culture. Maquet's aesthetic locus is 'the class or classes of objects that are localized in areas of heightened aesthetic consciousness' (p. 30). He is not concerned with why such areas exist; the empiricist anthropologist's only interest is in the objects themselves.

An object cannot, however, be studied 'in its own right'. When it is sundered from one context it must, to be discussable, be placed in another:

An aesthetic locus is the area in which aesthetic sensibility and concern are intensified and where, consequently, the aesthetic form of the object is granted priority even if its instrumental functions have to suffer Thus locus objects tend to be of less utility than others, or of no utility; they are superfluous (p. 73).

Maquet is not asserting that there are cultural areas which are organized around a concept of 'art for art's sake,' but instead that the criterion of non-instrumental features reveals that the aesthetic value of certain objects is far more important than their roles in ceremonials, religious rituals, ancestral rites, and the like. This idea of a radical incompatibility between the meaning a people ascribes to its productions and the actual significance of those objects has a lone and tenacious heritage which passes in manifold forms from the armchair anthropologists of the nineteenth century through Malinowski and Durkheim to certain anthropological schools of the present day. Maquet sounds rather like Malinowski when he claims

Aesthetic locus is an analytical category It is not necessarily a category recognized by the participants in the culture studied. Such is the case with most anthropological analytic concepts; our distinctions between magic and religion, between judiciary and legislative function, etc., make sense in the study of most cultures, regardless of whether or not the culture bearers possess equivalent distinctions in their conceptual systems (p. 32).

There is some validity to such a statement; anthropology needs, at one level of analysis, to deal with the conceptual translatability of cultures. Failure to take native classification into account leads either to the use of categories which are so general as to prove worthless in any specific application, or to the blatant substitution of categories derived from the observer's own culture for indigenous ones. Maquet's approach to the aesthetic artifacts of other cultures makes use of both of these forms of anthropological malapropism.

Malinowski, in 'The Problem of Meaning in Primitive Languages', (in Supplement I to C.K. Ogden and I.A. Richards, *The Meaning of Meaning*, London 1923) wrote 'in its primitive uses language functions as a link in concerted human activity, as a piece of human behaviour. It is a mode of action and not an instrument of reflection' (p. 312).

His idea of the 'context of situation' alleged that social behaviour and institutions (including linguistic and ritual codes) could only be understood as practical means of satisfying the biological needs of a community of individuals. Cultures were expressions of the different ways the human animal pursued food, warmth and procreation in different environments. Maquet, Marvin Harris, Leslie White, and other proponents of 'cultural materialism' accept this perspective as their own. Maquet speaks of 'the fundamental

psychic unity of mankind' (p. 47) and declares 'the human component is logically prior to the cultural and idiosyncratic; it is possible to imagine an 'abstract man' endowed only with human nature and stripped of any cultural and individual determinations' (p. 51). Cultural forms are, then, simply the means of expressing and fulfilling certain innate and universal needs of this 'abstract man'. If one can discover the character of these needs and trace their manifestations in a culture, one has performed an act of anthropology.

Maquet's *Introduction to Aesthetic Anthropology* is the result of his discovery of 'contemplation, one of the few existential relations between man and the world' (p. 45):

Contemplation is not a reflective attitude of the subject thinking of himself, turning his thoughts inward; on the contrary, the subject is oriented toward the object, fully alive to it. Contemplation is not cognition, the discursive process through which the intellect actively and critically builds a mental image of the object; on the contrary, the contemplative subject does not analyze and reconstruct, he simply attends to the object (p. 14).

In support of his definition, Maquet refers to Charles LeCoeur's work among the Saharan Tubu which revealed that 'the idea of repose is at the center of their aesthetics' (p. 26). 'Repose' as a philosophical term is rather nebulous. Martin Heidegger has directed the greater part of his philosophical speculation to the idea of being 'at home' in the world. His 'at-home-ness', in its sense of being at one with one's surroundings, is analagous to Maquet's concept of 'repose'. Being 'at home' for Heidegger involves an endless series of self-positings and choice-makings in pursuit of an existential 'fit'. If Maquet's contemplated object can provide such a *rapprochement* of observer and form, that object is of great anthropological interest. Yet it remains anthropologically insignificant until the anthropologist has uncovered and understood the interplay of self-conception, form, and environment which results in such an engrossment. Maquet, however, avoids the whole question of the link of culture and the forms capable of 'orienting' the attention of its bearers. He places the grounds of an aesthetic article's appeal outside of history - far beyond the realms of willing and doing. His proclamation of 'the irreducible character of the contemplative relationship to reality and the rooting of the aesthetic experience in contemplation' (p. 95) protects it from analysis by placing it, by definition, prior to anything we could use to question it. If the contemplative relationship is truly universal and *a priori*, it is of little use to a discipline interested in comprehending the significance of the varied ways persons relate to their environments and their social systems; it can tell us no more about cultural variability than can the statement 'all societies are made up of one-headed persons'.

Maquet's categories do not, however, stand outside of history in a realm of universals. Let us look, for example, at his

explanation of behaviour within an aesthetic locus. Maquet claims artisans are drawn to the locus by the simple desire to produce aesthetic objects: 'Those who, in any society, have a keen appreciation of aesthetic forms, and those who are eager to create them, meet here' (p. 31). The empiricist observer, by disencumbering cultural relationships of the ephemera of religious beliefs and hereditary role relegations, has managed to elevate the form of the market relations of Western art production and dissemination to the status of a universal category. In Maquet's formulation, the aesthetic domain is simply a meeting point for individuals with a natural talent for creativity and those whose appreciation of aesthetic quality is backed by sufficient wealth or power to allow them to 'purchase' the created artifacts. There is no significance to their association outside of the symbiotic affiliation of supply and demand.

There is, however, quite a bit of pernicious meaning behind the naive ethnocentricity of Maquet's discourse. Maquet would set up aesthetic appreciation as an irreducible characteristic of human sensibility: 'just as it has been said that man is a thinking animal, it may be said also that man is an aesthetic animal' (p. 25). The innate capacity for contemplation is the ground for the creation of art objects, the articulation of aesthetic values, and the establishment of a network of supply and appropriation. Yet Maquet acknowledges that aesthetic objects don't just happen; they occur 'because craftsmen have an aesthetic concern, they do not content themselves with the forms required for the efficacious use of the implements they make' (p. 24). He does not mention that the labour involved in making something more aesthetic than necessary is labour diverted from other social ends. A thing of beauty is not just a joy forever; it is also a luxury item which announces to those who view it that the owner had either the excess time to produce it (and to train for that production), or the power to appropriate it from its creator. Although an aesthetic article within the culture matrix which produced it speaks of much more than the labour its production entailed, the fact that aesthetic loci coincide with centres of ideological, political or economic power reveals the control those institutions or individuals have over their society's distribution of wealth and labour. Medieval reliquaries were wondrously crafted containers made of precious metals and jewels by a group of craftsmen whose labour was controlled by the cathedrals. Their sumptuousness testified to the power and influence of the shrines which possessed them. Yet in times of need, these aesthetic objects served as handy sources of concentrated, easily liquified capital; they were melted down and sold when the churches needed cash. It would be reductionist to claim these articles were created solely for either their laudatory function, their aesthetic appeal, or their ready convertibility into money. Yet an anthropologist who programmatically advocates the purely aestheticist approach is serving, whether intentionally or not, groups which benefit from the masking of ideological manipulation or economic exploitation.

Western civilization has a two hundred year history of ignoring the beliefs and institutions of subjugated peoples in order

to wring whatever labour it can from them. Maquet's relating of the aesthetic locus to areas in which excess craft labour goes into artifact production suggests that, for him, aesthetic value and surplus labour value are coterminous. His detachment of aesthetic quality from the beliefs and feelings of the people who produce and venerate it bears a suspicious likeness to both the melting down of reliquaries for their cash value and the rendering of whole peoples for their labour value.

The western art community is an empowered institution, and its machinations are international big business. The covetousness of its collectors and the Janus-faced attitude of artists who produce statements of alienation which can be sold for 'big bucks' perpetuates the concept of the aesthetic artifact as an object of contemplation complete in itself. An anthropological study of that historically-determined concept would reveal that such autonomy is partially conditioned by the need for the aesthetic artifact to be simultaneously valuable and freely circulable. Art objects in the western world are luxury commodities, and 'primitive art' is now highly valued on the art market. One would expect a bit more critical insight from a man who writes

The first things to be metamorphosized into art objects are the artifacts of foreign societies that have lost their independence, and of the weak strata inside a society such as the peasants.

It may be that museums begin with looting (p. 38). Despite the fact that little in that characterization has changed other than the prices paid for the art objects, we receive from Maquet nothing but an academic justification for the systematic separation of third world peoples from their religious and ceremonial artifacts. Intellectual and economic colonialism go hand in hand.

Maquet concludes with the assertion that

If these demonstrations do not carry conviction, it matters very little. Aesthetic anthropology is an empirical discipline. Consequently the positions ... mentioned may be considered to be the basic assumptions of a 'contemplative theory of art'. Such a theory, like any other, may operate as a source of hypotheses and an explanatory synthesis (p. 95).

However, unlike the cultural materialists, we recognize that the definition of an object determines the uses to which it will be put. Any anthropology which accepts Maquet's idea of an aesthetic anthropology as a starting point for its labours will function both as a tool for exploiting the peoples it claims to study and as a means of destroying the integrity of anthropology as a disinterested academic discipline.

GLENN W. BOWMAN

SHORTER NOTICES

COLIN RENFREW & KENNETH L. COOKE (eds.), *Transformations... Mathematical Approaches to Culture Change*, New York and London: Academic Press 1979. 515 pp., biblio, £22.00.

In this weighty (515 pp.) tome, jointly conceived by a mathematician (Cooke) and an archaeologist (Renfrew), 21 contributors have been brought together with the express purpose of bringing 'contemporary mathematical techniques to bear on the phenomena of culture change, and to formulate the problems of the archaeologist, the anthropologist, and the historian in such a way that they are indeed susceptible to treatment of a mathematical kind'. In this line an impressive array of mathematical models are trotted out (e.g., optimization models, dynamical systems, simulation, computer-aided transformations of symbols, kladistics, and catastrophe theory to name but a few). Obviously encouraged by the successful wedding of biology and mathematics, the editors here intend to generate a new subfield within our discipline - that of mathematical anthropology. One can only wonder however, whether this particular wedding will be equally fortuitous or whether it will ultimately prove to be of the shotgun variety. While not disputing the validity of the mathematical models produced in the text, one can only wonder at their utility and/or applicability for most social anthropologists. As the editors themselves make abundantly clear, this book is neither for the nonspecialist nor the uninitiated. Of the last 4 chapters, for example, we are told that 'the mathematics is effectively inaccessible to the nonspecialist' and that 'the chapters that follow can be fully understood only with some prior acquaintance with catastrophe theory' (p. 421). Thus, unlike the authors, I did not find my understanding of the operation of hunting-gathering bands (pp. 405-418) significantly increased by the application of their model. Indeed, I take issue with their contention that 'It is easy to see ... that pandemonium models can compute the maximization function with finite-order local functions' (p. 416). I can only ask: 'easy for whom?' Certainly not for the nonspecialist. For those with the necessary mathematic sophistication however this book will provide many interesting hours and will undoubtedly stimulate much debate.

The illustrations by Escher and Osbert Lancaster are an unexpected bonus.

STEVEN SEIDENBERG

PATTY JO WATSON, *Archaeological Ethnography In Western Iran*, Tucson: University of Arizona Press (Viking Fund Publications in Anthropology, No. 57, 1979, Arthur J. Jelinek (ed.), xv, 322 pp., biblio, \$9.95.

As stated in its title, this book represents an ambitious attempt to bridge the gap between archaeology and ethnography. From data gathered 1959-60 the author has produced a copiously illustrated (94 drawings, 47 plates, 25 tables) account of life in the West Iranian village of Hasanabad. As she is an archaeologist her account is heavily biased toward the material culture of the village. The student of social anthropology should not however, be daunted by this as the bulk of her text nevertheless maintains an amazingly close resemblance to standard ethnographic accounts (as her chapter headings readily attest ... Chap. 1: Hasanabad: The Land and the People; Chap. 2: The Village and Its Economic Organization; Chap. 3: Agricultural Methods; Chap. 5: Domestic Technology; Chap. 6: Kinship and Community; etc.).

Watson claims to have had two purposes in mind in producing this book: '1) to make available as much data as possible on details of technology and subsistence within the context of village life in the region studied ...; [and] 2) to make a contribution to our knowledge of behavioral correlates for material culture' While this reviewer cannot comment on Watson's success as an archaeologist, this book nevertheless represents an admirable introduction to the material culture of village Iran and would be quite useful to the anthropologist of the region.

STEVEN SEIDENBERG

C.C. LAMBERG-KARLOVSKY *et. al*, *Hunters, Farmers, and Civilizations: Old World Archaeology*, San Francisco: W.H. Freeman 1979. 306 pp. \$10.00 (cloth), \$4.50 (paper)

This book is a collection of 30 separate articles which originally appeared in *Scientific American* between 1953 and 1978. They are arranged in four sections (1. Paleolithic Hunters and Gatherers; 2. Neolithic Villagers and Farmers; 3. Bronze Age Cities and Civilizations; 4. European Communities: Neolithic to Medieval), each prefaced with a short Introduction by C.C. Lamberg-Karlovsky, Professor of Anthropology and Director of the Peabody Museum at Harvard.

The book is vast in scope with articles on such diverse topics as Stonehenge, obsidian trade, Harappan civilization and pharaonic tombs. Similarly, the articles represent a broad geographical sweep; included here are articles ranging from Winchester through Mycenaen Greece, Isimila and Jericho all the way to Sumer, Persia and,

finally, the Indus Valley. Each article in the collection is by a well-known specialist in the field, and they are all reproduced from the *Scientific American* with their texts and illustrations intact. As with all *Scientific American* publications the artwork, maps and photographs are excellent.

Although each article was written by a specialist this remains an eminently readable book. As each article was produced specifically for the *Scientific American* they tend to be geared to an informed, but nonspecialist, audience. Further adding to the *pot-pourri* nature of the collection is the short length of the articles. At an average of only 9 pages each they remain *hors d'oeuvres* that entice, successfully avoiding the too common tendency of bloated over-specialization.

While none of the articles are especially useful for the social anthropologist, I found some to be of more interest than others. John Chadwick's 'Life in Mycenaen Greece' (pp. 225-233) for example, while telling me rather more than I cared to know about the Linear B syllabary, was nevertheless anthropologically quite interesting, as was Martin Biddle's 'The Archaeology of Winchester' (pp. 281-292).

Minor criticisms relate to the selection of some of the items included in the collection. Walter B. Emery's 'The Tombs of the First Pharaohs' (pp. 218-224), for example, seemed rather *passé* to me ... more of a last gasp of Tutankhamenism than a valuable contribution to Old World archaeology. In contrast the omission of articles on more recent discoveries I found disconcerting. The inclusion of Norman Smith's, 'Roman Hydraulic Technology' (*Scientific American*, vol. 238 no. 5, pp. 154-161) for example, would have been a welcome addition to the volume. Nevertheless, this book remains highly readable and represents an interesting introduction to Old World archaeology.

STEVEN SEIDENBERG

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SELF : PUBLIC, PRIVATE
SOME AFRICAN REPRESENTATIONS¹

At the very beginning of Lévy-Bruhl's book *L'Ame Primitive* (1927) (translated as *The 'Soul' of the Primitive*, and those inverted commas round *Soul* mark it as an area of the translator's uncertainty), he took it for granted that his 'primitives' would be confused if asked about those aspects of themselves which now concern us:

It is scarcely likely that primitives have ever given a form, however indefinite, to the more or less implicit ideas they have of their own personality. At any rate it would be quite useless to question them about it, for ambiguity and misunderstanding would be the only result.

He suggests that his readers, by contrast, would have more clearly formulated answers ready. Yet in the year in which *L'Ame Primitive* was published, Aldous Huxley, reflecting on Proust's phrase 'the intermittence of the heart', observed that:

The number of completely unified personalities is small. Most of us go through life incompletely unified, - part person, the rest a mere collection of discontinuous psychological elements.

¹ Text of a Lecture given at Wolfson College, Oxford, on 11 June 1980, as part of a series, 'The Category of the Person', organised by Michael Carrithers and Steven Collins on the subject of the Lecture given by Marcel Mauss, 'Une Catégorie de l'Esprit Humain: La Notion de Personne, celle de "MOI"' (*Journal of the Royal Anthropological Institute*, 1938). The present paper may appear in modified form in a forthcoming collection based on the series.

And Ouspensky of course had gone much further. In a lecture given in 1922 he is reported on as follows:

... Man misunderstood himself: he thought he had a permanent self, a master "I", which integrated and controlled his thoughts and actions. But this was an illusion. Instead of the single "I" there were innumerable "I"s, many of which said contradictory things. Then Ouspensky got up and drew a circle on the blackboard, and divided it by criss-cross lines until it looked like a fly's eye seen under a microscope. In each little space he put an "I", and said "this is a picture of Man".

Academically more central though, and nearer to our own time, place and intellectual habits, the Waynflete Professor of Metaphysical Philosophy in Oxford, Gilbert Ryle, reflected in *The Concept of Mind* on the 'enigmas ... which all turn on what I shall call the systematic elusiveness of the concept of "I"'. It was ultimately this elusive 'I' which St. Augustine in the *Confessions* sought in God, and which Mauss in 'L'Idée du Moi' sought in a history of Man's self-image.

Early in his essay, Mauss distinguishes his own investigation - 'entirely one of law and morality', he says - from those of contemporaneous linguists and psychologists. 'In no sense do I maintain', he wrote for the linguists,

that there has ever been a tribe or language in which the word *je-moi* (I-me or self: you will see in France that we still decline it in two words) did not exist, and did not express something clearly represented

In that seemingly casual aside - 'I-Me or self: you will see that in France we still decline it in two words' - he allows for subtle difficulties of translation, and hints, perhaps, that some languages have been better equipped than others for making the particular moral, philosophical, legal and theological distinctions which then interested him. In relation to academic psychology he added:

I shall ignore everything about the "self", the conscious personality as such. I shall say simply: it is clear, above all to us, that there has never been a human being without a sense not only of his own body but of his simultaneously mental and physical individuality

Indeed there is much more to the idea of the self than Western ideas of legal and moral personality; and I do not think that ideas of the self can be so readily separated from the sense of simultaneously mental and physical individuality as Mauss's purpose then required. The 'average', 'archaic' or 'total' man, as Mauss called those outside the academically educated classes of modern society (and from whom he thought those educated classes had much to learn), does not think about himself as though he were examining an intellectual construct; and even that sense of mental and physical individuality appears to be dissolved or surrendered

in trances, mystical experiences, spirit possession and contemplative prayer; and even those who have had none of these experiences may have had, in dreams, some intuition of the transformation of the self - of 'the conscious personality' - those experiences are said to involve. Certainly, to consider African ideas of the 'I', we have to begin by allowing 'the self' to be more labile than are the ideas of it in Mauss's essay.

Let us take our bearings, then, not from Mauss but from African forms of self-expression not elicited by questions put by foreign observers in a foreign philosophical and psychological idiom. Here is a summary of a West African, most probably Yoruba, folktale:

The king invited the animals to a great feast, and offered a prize to the best dancer. The animals danced energetically before him, each showing off its own most striking qualities - the elephant its grave dignity, the leopard its beautiful coat and sinuous agility, the gazelle its spectacular leaps and so forth. When, at the end of the dance, they gathered around the king to hear his judgment, to their surprise and displeasure he awarded the prize to the tortoise. Answering their complaints, the king asked them who had provided the feast, and who was giving the prize, to which they could only reply "It is you, O king!". "And so it is that I award the prize to the tortoise", said the king, "for it is only I who can see the dance of the tortoise: his dance is entirely inside him".

In much West African folklore, the tortoise represents intelligence, resourcefulness, trickery and luck. Thus for those who tell this tale, the success of the slow, ungainly tortoise is an extreme example of the deceptiveness of outward appearances, though the moral is not that hidden intellectual agility is preferred, as such, to physical display: both are parts of the dance. The tortoise too, now public and exposed, now withdrawn and hidden, is a fitting and subtle image for the self.

Otherwise the story is immediately comprehensible without anthropological or literary comment to any thoughtful child; and since folktales in Africa as elsewhere contribute to the education of children, it may be assumed that from childhood the Yoruba are not only supposed to have an idea of a hidden, private self - here an inner *activity*, you will have noted - but to understand that it may ultimately be more important than the outer activity, the *persona*, or mask, in Mauss's terms, presented to others. There are many other African stories about tricksters - the spider, the hare and others besides the tortoise - who often admirably succeed, but sometimes ludicrously fail, by being, as we might say, 'all out for themselves'.

I emphasize this because much of what has been written about African ideas of self, rightly putting to the fore the importance of a person's group and status - the public self - for defining what and who he or she is, can deflect interest from this African

concern, also, on occasion, with individuals as individuals. Professor John Beattie has drawn my attention to what Burckhardt wrote about pre-Renaissance man in Europe in this connection: 'Man was conscious of himself only as a member of a race, people, family or corporation - only through some general category'; and he quotes for comparison the modern French Africanist Professor Roger Bastide: 'It is clear that the African defines himself by his position When one asks him what he is, he places himself in a lineage, he traces his place in a genealogical tree.' Lévy-Bruhl, whose 'primitiveness' subsumes all mentality except that of his most rationalist readers, earlier made a generalization which, with more or less qualification, has underlain many interpretations of African thought:

If primitive mentality pictures the individual as such, it does so in a way that is wholly relative. The individual is apprehended only by virtue of his being an element of the group of which he is a part, which alone is the true unit.

This collectivist philosophy, so to call it, appeared as characteristically 'African' very recently in a pamphlet put out to explain African values in the Rhodesian-Zimbabwe elections. It is summed up at a religious level of interpretation (to which in a somewhat different way I shall later return) in Fr. Placide Tempel's almost mystical recreation of a traditional African metaphysic:

For the Bantu, man never appears in fact as an isolated individual, as an independent entity. Every man, every individual, forms a link in a chain of vital forces, a living link, active and passive, joined from above to the ascending line of his ancestry and sustaining below the line of his descendants.

So, it might be said, at a more mundane level, do all who take the idea of incorporation seriously - members of royal houses, for example, or ancient Colleges. 'Bantu philosophy' here corresponds to that of the Bourbons, the Hapsburgs, the Tudors, and innumerable families established as the 'so-and-so's' of their local communities, whose secure conviction of their hereditary status, far from inhibiting individuality, has sometimes led them to indulge and exploit it.

Much humour and drama in African (as in other) oral literature and history, derive from a keen perception of individual eccentricities, the deliberate or accidental flouting of convention, slips of the tongue which reveal private reservations, clever calculations of personal advantage, and selfish obsessions (often represented in Africa as gluttonous greed), all of which defy or subvert accepted standards of judgment and behaviour. Many African songs are also, contrary to what was once supposed about their anonymous, communal, 'folk' origins, usually assigned to their individual composers, who hold the copyright, as it were, and they contain images and allusions which are incomprehensible (though they may be exciting in the context of performance) without a knowledge of intimate local and personal experience of the composer himself. In this

respect, like much of the best poetry, they make the private self public, while retaining a sense of privileged admittance to its privacy.

Dr. Francis Deng, himself a Dinka, writing of the Dinka of the Sudan, describes how songs voice experiences, attitudes and emotions which people keep to themselves in the course of daily conversation. His account of the relationship between private self-esteem and public esteem (and it is clear to anyone who has taken part in a dance that up to a point the dancers are dancing for themselves) recalls the Yoruba dance of the tortoise:

The power of group song lies largely in the chorus, even though the role of the individual solo is a pivotal one, showing that the significance of the individual is not overshadowed by this group demonstration. The fact that there are points in dancing when every individual chants his own *mioc* [individual praises, or 'he does his thing'] shows the significance of songs and dances to the ego of each person. Even the group reference to "I" [when the choral singers refer to themselves together as 'I' and not as 'we'] indicates that group solidarity is fundamentally a construction of individual egos.

Evans-Pritchard pointed out in 1928, *contra* Radcliffe-Brown's doctrinally sociologizing interpretation of dancing as an expression of, and training in, social harmony and conformity, that Zande dances were often turbulent affairs, involving '... slanderous songs, sexual indiscretions, competition (for self-display is essentially aggressive when thwarted)', and referred to the airing of private grievances at large public gatherings, with several hundreds of dancers.

I have said perhaps more than enough to suggest that one can lay too much one-sided stress on the collectivist orientation of African ideas of the person. Obviously, the less differentiated a people are by occupation, interests, ideals, and origins (and the readers of Mauss and Lévy-Bruhl were by comparison with most Africans of the time very differentiated indeed), the more of their private, as well as public, values and resemblances they may be expected to share; but the recognition of the importance of an inner, mysterious *individual* activity, comparable to what is meant by speaking in English of 'what goes on inside' a person is attested by many proverbs.

The 'what goes on inside' a person may not be fully grasped by that person himself, as is recognized in a Fipa (Tanzanian) proverb: 'What is in the heart, the heart alone knows', glossed by Dr. Roy Willis as '... we don't know everything about ourselves through our intellect', which accords both with psychoanalysis and with some African notions of witchcraft, for a witch may not always be thought conscious of being so. Fr. Tempels, with all his mystique of the collective, quotes a Congolese proverb, 'None may put his arm into his neighbour's inside', meaning 'The neighbour's conscience remains inviolable even for his closest friend'. The most quoted of all Zande proverbs according to Evans-Pritchard is

'can one look into a person as one looks into an open-weave basket?', the open weaving being contrasted with close weaving, which conceals what is inside - as when a Zande also said that 'Our bodies are like a man who builds a hut to reside in. Our real person is the strength (or breath) which is in our bodies and is the soul (*mbisimo*).' Such statements, and many more - the Fante proverb 'one never knows what is in another person's heart', the Dinka proverb 'what is inside a person is like what is in the forest or the river' (i.e. hidden, often dangerous, and unpredictable) - all allude to the importance, no matter how much store may be set by social role and status, of individual, private, intellectual and emotional activities: the private self. The real difficulties of translation arise when we ask (and we may ask mistakenly, for the question presupposes particular kinds of answer) for a description of the private self that acts and is acted upon, and where that action is located.

There is now for Africa (as there was not in Mauss's time) an extensive literature on the vocabularies used by African peoples to describe the emotional and intellectual attributes of human beings, attributes often represented in that literature as separate 'components' of the total person or personality. It appears from much of that literature that some African peoples (and naturally some of the most articulate in this respect come from Francophone Africa with its inheritance of French education) formulate their indigenous metaphysical systems more clearly than others; have reflected more than others on the nature of the self, for example; and have their own men of learning to enter into debate about it. Also, whether in the nature of the information or in its interpretation, there are, in any language, difficulties in deciding whether some expressions are to be taken more literally or more metaphorically, since in all societies some people are bound to be more literal-minded than others. The close Fanti friend of mine who gave me the proverb 'One never knows what is in another person's heart' added (for even their friends never know what anthropologists may make of their information) that 'of course this doesn't refer to the physical heart'.

I now return particularly to the Dinka of the Southern Sudan, for among them I had that experience of daily conversation which enables one to discriminate, as we take for granted in the language into which we were born, between what people mean and what they say. Then one learns also what kinds of questions, formulated in an alien mode of thought, might receive answers - but answers which, though grammatically, syntactically and even semantically plausible, do not represent, and may positively misrepresent, indigenous and spontaneous interests and ideas.

The Dinka were indifferent to many of the metaphysical speculations and distinctions which comparative studies in Africa and elsewhere might lead one to seek among them. The commonest answer to a foreigner's questions of a speculative kind is 'I don't know'. In the first, and even now outstandingly competent, grammar and vocabulary of the Dinka language, published almost exactly a hundred years ago, the great missionary-traveller Fr. Giovanni Beltrame gave samples of dialogue in the 1870s

between the missionary and the Dinka. Despite imposing upon the Dinka some conspicuously Christian eschatological doctrines about heaven and hell which were clearly being fed back to him, the missionary truthfully represents in the dialogue the frequency of the answer 'We don't know'. But it is an assertion of agnosticism in the strict sense, a doubt about the questions, not a confession of ignorance. It does not mean 'but we should like to learn' but rather 'The answer means nothing to us'.

According to the literature again, some African peoples have ideas of a soul-body dichotomy analogous to that which is generally assumed in Christian Europe, but differing from it, importantly, in allowing for the presence of several distinct 'souls' in each person. It might be possible to make some sort of translation of the belief into Dinka, but it would make no traditional sense; for the word by which 'soul' would have to be translated is the word for breath and breathing, and for the presence of life which breathing signifies. To suggest therefore that a person might have several 'breaths' with different attributes, would be as odd to the Dinka as would be the notion of a plurality of different souls in one of their number to the other fellows of All Souls College, Oxford. Missionaries, using *weei*, breath and life, as the best approximation to translate 'soul', have presumably successfully reshaped the Dinka word for their converts - reshaped it into a unitary term for a moralised and spiritualised self-consciousness of each separate individual in relation to a personalized God.

I hope that further research especially among converts will tell us something about how this translation of conscience intimately takes place. In Dinka traditional thought, the breath/life comes from and in some way returns to God, but otherwise little resembles the 'soul', understood as a ghostly counterpart of the living person, the 'ghost in the machine' as Ryle called it, which atheists as well as theists could imagine to be morally good or bad, and doctrinally consigned to heaven or hell. There is a Dinka word, *atyep*, which might adequately translate 'ghost', but not 'soul' for it means primarily a shadow, image or reflection, and may properly be regarded as the image of the dead as reflected in the memories and experiences of the living. The *atyep* is not 'something inside' a living person.

I shall now imagine (and with some misgivings, for the experience upon which I base it are some 30 years behind me) that a Dinka without recourse to the vocabulary of European philosophy or theology were to contribute to this discussion in traditional Dinka idiom; and I shall try to represent what might be said by using the nearest literal equivalents in English to Dinka expressions, leaving the Dinka words for footnotes in a later publication. The Dinka word for 'person' has strong masculine overtones, but in some contexts may mean 'mankind' as when we use the capital 'm' for 'Man'. A living person has a body which is animated by breath/life, but body and breath are not in apposition as 'body and soul' are in English. Dinka would not normally say what would be translated word for word as 'a dead body'. For 'corpse' they might perhaps say 'the body of a person who has died', but

the natural expression would be simply 'a person who has died'. What is then left is not strictly the 'body', which also means 'self' as we shall see, but flesh and bones and the rest. The breath/ life is stronger in the more vigorous, whether people or animals; is weaker in the old, in children and in the sick; and departs when a person dies. In prayer and sacrifice God is asked to give and support the breath/life of people and cattle, but this breath can scarcely be regarded as a 'component' of human personality, since it differentiates humans only according to their degree of vitality, has no moral qualities, and merges the human self-image with what is in the nature of all sentient beings, perhaps especially cattle. Further, breath is obviously both inside and outside our bodies, whereas the qualities of personality are spoken of as in the body. The 'what is inside' a person is in general spoken of as 'in the belly', though the reference may specify that particular part of the body more or less according to context. (The 'what is inside', it is interesting to note, seems to be cognate with the Dinka word for 'truth').

It is through metaphors based primarily upon the head and the heart (and more from the heart than from the head) that most moral, affective and intellectual states are expressed. Like many other peoples, the Dinka tend to relate thinking to the head (though not to the brain, and cleverness is shown in the eye) and feelings to the heart. But many mental activities much more complex than sensations and affections are referred to the heart - doubt and suspicion, for example. There are numerous expressions using the words for 'heart' and 'head', with adjectival qualifications. Here are a few examples taken from the entries under *pwou*, 'heart', in Father Nebel's little Dinka dictionary. I give my literal translations of the Dinka entries. Fr. Nebel translates the word *pwou* generally as 'heart, chest, mind, intention', and then includes: 'my heart is there' (or 'in it') for 'I agree, I like'; 'my heart is not there' (or 'in it') for 'unwillingly'; 'heart lost' for 'to forget, to lose control of oneself' (though for 'to forget', 'my head has lost' would be more usual); 'to forbid the heart to someone' as 'to be heartless to' (perhaps 'to harden one's heart' would be as close in the English idiom); 'to have the heart darken' as 'to be startled, frightened or sorry'; a phrase which may mean 'not to have enough heart' or 'heart not to suffice' as 'to be suspicious'; 'to have a small heart' as 'to be discreet, humble'; 'to have a big heart' as 'to be proud' - for '*magnanimous*' I think one would have to begin by saying simply 'good-hearted', and add words for generosity, nobility, forgivingness and so on. The most commonly-used expressions are 'sweet (or tasty) heart' for 'happy' or probably more accurately 'contented', and 'bad heart' for 'aggrieved'.

Even from this short list it will be seen that the metaphoric associations of the Dinka word *pwou*, heart, for defining human characteristics, thoughts and feelings, often does not coincide with the English idiom, and in another context it might be interesting to consider the implications of such cultural and linguistic differences. ('Lionheart', for example, could literally in Dinka suggest a were-lion who changed form in order to

devour people.) But here my interest is of another kind. Although, like ourselves sometimes, the Dinka often put the hand to the heart, or the head, when speaking of conditions associated with those parts of the body, the linguistic usages are consciously metaphorical. To take and use the physical heart of a victim in order to possess oneself of its qualities (as is sometimes reported to happen in ritual murders in other parts of Africa and elsewhere) would appear as evilly superstitious and wicked to the average Dinka as to the average European, though that is not to say that there are no superstitious and wicked people either in Dinka land or in Europe. Those whom Europeans call 'psychopaths' are those whom the Dinka call by words translated as 'witch' or 'sorcerer'. But for most Dinka, the distinction between the metaphorical and the literal resembles that of an ethnically-related people, the Southern Luo of Kenya, for whom the indigenous distinction between the physical and the moral content of metaphors is quite explicitly and deliberately emphasized by a Luo author, A.B.C. Ochalla-Ayayo:

The heart, *Chuny* according to the Luo, is the site of the intellect and ethical emotions and wisdom of a person. They consider emotions of attitudes, evil thoughts, pure feeling, wisdom, hospitality and generosity as invoked from the heart, *Chuny*. The Luo make a distinction between physical heart, which they call *Adundo*, and the spiritual heart, *Chuny*. It does not appear that *Chuny* which also means liver is [in that meaning] referred to in this context, since the positions they point at when asked for physical *Chuny* [in the sense of liver] and spiritual *Chuny* of a human being do not correspond. The spiritual heart is situated somewhere beneath the end of the central cartilage, a spot believed to be occupied by the physical heart. Yet they do not call it *Adundo* when ethical emotions are implied, but *Chuny*, spiritual heart.

The Dinka do not have two words for heart, but otherwise this distinction - and also connection - is implicit in their usage.

Such are some of the ways in which Dinka speak about themselves and others, and these forms of self-expression clearly represent that 'sense' of the simultaneously mental and physical which it was not part of Mauss's intention to dwell upon, but without considering which we should have little to say about their ideas of human personality. It seems to me that the Dinka language, unlike modern, educated, and for the most part metropolitan English, compels its speakers to integrate the moral and physical attributes of persons together within the physical matrix of the human body. In modern English, moral and mental conditions are spoken of in more or less abstract terms (anger, suspicion, forgetfulness and so on), cut off, for most, from their etymological roots. We say 'I trust him', for example, and could ask in a Platonic way 'What is trust?'. In Dinka, one would have to say 'I know his heart', and should the question then arise of what it means to know someone's heart, it would be necessary to return to what is meant by 'heart' in other contexts. It may

be that the disjunction, for most modern English speakers, between abstract terms and concrete imagery has something to do with the complex foreign origins of the English language. Non-literate Africans can explain the etymology of words as non-literary English-speakers cannot, but that is beyond my province. The difference is however consistent with the absence, among (in this case) the Dinka, of the mind-body dichotomy which many writers of this century have wished to resolve. D.H. Lawrence is perhaps the most fervid of many who attacked what Eliot called the 'dissociation of sensibility', the separation of thinking from feeling, in modern civilisation, and (like Mauss) attributed some ideal undivided self to American Indians (he read a good deal of anthropology), peasants, workers and others whose lives and language had not been corrupted by bookish education - among people who, like Matthew Arnold's Scholar Gypsy, had escaped:

... this strange disease of modern life,
With its sick hurry, its divided aims,
Its heads o'er tax'd, its palsied hearts.

And still an integration of thought and feeling in metaphor and imagery is what we seek to have recreated for us in the best literature. We go to the theatre to hear Cordelia say:

Unhappy that I am, I cannot heave
My heart into my mouth

not 'Even on this distressing occasion I cannot bring myself to display my emotions'.

The importance of the bodily matrix in Dinka notions of self is shown finally in the very word most often used where we should use 'self', for that indeed is the word *gwop*, 'body'. 'I myself' is literally 'I body', 'yourself' is 'you body'. Body here is obviously not like 'self', a pronoun, but a noun intensive of the personal pronoun. Body, *gwop*, is incorporated in many metaphors, for example 'light body' for 'healthy', 'sweet body' for 'lucky', 'body afraid' for 'shy, embarrassed or timid', 'body heavy' for 'sick'. There is also a real reflexive pronoun for 'self', quite different from the word for body, and which signifies also 'apart from others' or 'separated from others'. Thus 'look after yourself' ('take care') uses the reflexive pronoun *rot* as does 'to kill oneself', and 'to love oneself' - that is to be a selfish, self-interested and self-indulgent person. If one were to translate Shakespeare's line 'Sin of self-love possesseth all my soul' into Dinka, it would have to be something like 'I have been very wrong (mistaken, missed the mark, as in aiming) because I have loved myself very much' using the reflexive pronoun *rot* again. But that is enough of a discussion which does no justice to the poetry of the Dinka language, by reducing it to something that sounds like a dull form of pidgin; I introduced it to give an impression of the way in which at almost every point the Dinka language allows for a wide range of intellectual and moral discriminations without leading into a seemingly autonomous world of abstractions. Words, as it were, must return to base.

And here I return to Professor Ryle, for the Dinka mode of thought and expression has correspondences in the work of a distinguished English academic. Consider Professor Ryle's account of his elusive concept of 'I':

Like the shadow of one's own head, it will not wait to be jumped on. And yet it is never very far ahead, indeed sometimes seems not to be ahead of the pursuer at all. It evades capture by lodging itself inside the very muscles of the pursuer. It is too near even to be within arm's reach.

Thus in the use of bodily imagery, the Dinka (and probably other African peoples), and one of the most reformist of modern British philosophers, come together - the Dinka never having been entangled in the 'entities and quiddities' of European metaphysics, the Waynflete Professor of Metaphysical Philosophy in Oxford having determined to get rid of them, as did Samuel Butler in satirizing his omniscient philosopher more than 300 years ago:

Beside he was a shrewd philosopher
And had read every Text and gloss over

....

He could reduce all things to Acts,
And knew their Natures by Abstracts,
Where Entity and Quiddity,
The Ghosts of defunct Bodies, flie;
Where Truth in Person does appear,
Like words congealed in Northern Air.

If our imaginary Dinka mentioned earlier were to be given a course in Descartes, he might well conclude, like another of Descartes's critics, A.J. Krailsheimer:

The *Cogito* is achieved at the price not only of severing all the traditional bonds by which man has been joined to other men and the world around them, but also of splitting in two the personal union of mind and body and expelling the instincts of the latter.

Mauss seems to have sensed (to use that word) this strain placed upon European intellectuals of his time; but one fact seems strange to me. Mauss was in very close collaboration with Henri Hubert, especially in writing of religion; and it is reported that on one occasion, when that doyen of Catholic studies of comparative religion in the Vatican, Pater Schmidt, referred somewhat slightly to Hubert and Mauss as 'two Jewish authors', Mauss replied: 'I accept for myself, but not for Hubert, who is descended from Pascal.' With his awareness of, perhaps even pride in, an indirect connection with Pascal, it does seem odd that one of the three most famous statements in French about 'le moi' (the others being 'L'Etat c'est moi' and 'Après moi le déluge') is not seriously considered, as far as my reading has gone, by Mauss: that is Pascal's aphorism 'Le moi est haïssable'.

For what follows from that aphorism in the *Pensées* would certainly be taken for granted by those whom Mauss contrasted with enlightened philosophical (or philosophized) man:

In short, the self (*moi*) has two qualities: it is unjust in itself, in that it makes itself the centre of all; it is offensive to others, in that it wishes to enslave them; for each self (*moi*) is the enemy, and would wish to be the tyrant, of all others.

Mauss may too easily and optimistically have described that Pascalian self as an aberration from the idealised self of his essay, directed by only the most altruistic and rational categorical imperatives:

... I shall show you how recent is the philosophical term "self", how recent is the "category of the self", the "cult of self" (its aberration) and how recent is respect for the self - and in particular for that of others (its normality).

He does not really take much account of the part played by religious conviction in moulding ideas of the self, though surely how men see themselves must be influenced by how, or if, they see the gods. Dr Deng represents for the Dinka what could be found in one form or another in, I should think, all African ethnography. Referring to the Dinka myth of creation, to which I shall shortly turn, he writes that it

... addresses itself to the question "Where is God?" which the Dinka sometimes wonder about, and not to the question "Does God exist?". Among the Dinka the latter is never posed. Should it be posed, as it is now with the introduction of inquisitive Western culture, the immediate answer would be "Who created you?".

And if egotism and egoism were condemned among the Dinka, as they certainly traditionally were, it is not because of some democratic and secular ideal of the quality and brotherhood of man (though equality and brotherhood were probably actually achieved among them more than among many who politically profess them), but because of the profoundly religious orientation of their thought, their respect for the gods.

God and Man begin to be mutually defined in a myth recounting that in the beginning, God created a man and a woman, whom he kept close to him. He forbade them to pound more than one grain of millet a day which sufficed, but because they were 'greedy', the woman pounded more, and in doing so raised up her long pounding pestle (as women do now when pounding). The pestle struck God, who then withdrew into the above, and must now be brought near to help human beings by prayer and sacrifice. So human beings, quite usually referred to as 'the ants of God', are as tiny and helpless in relation to God as ants are to men. God's transcendence ultimately reduces all merely human persons to the same level, and since in Dinka thought God and gods are quite different in kind from Man (scarcely any less anthropomorphic or more

abstract representations of divinity could be found in Africa), the virtual deification of human beings and human qualities is quite alien to Dinka thought. There are no man-made representations of the divine.

But this distant God (like Professor Ryle's elusive 'I') though out of reach can be *interfused* (I may say this is not sociopsychological jargon, but comes from Wordsworth), with the human person and the human body. In states of possession, which any Dinka may experience and all must certainly have seen in others, divinities 'seize' or 'capture' the human person, body and all. The self is then temporarily replaced by a god, both subjectively and objectively, for it appears that the person possessed has no subjective experience of possession. He (or she, for it often happens to women) is replaced by a spiritual being, a being of another order. People appear temporarily to lose 'self-control'.

It will have been apparent from what I have said earlier that the Dinka are a very rational, even in some ways rationalist, people, especially when confronted with the non-rational constructs of foreigners. It is not difficult for the most part to share, or at least give a notional assent, to their way of talking about themselves. But here, in the acceptance of the interpenetration from time to time of the human and the divine (and of the divine as defined, of course, by their tradition), there appears an experience into which foreigners cannot really enter, for while still living in the same political and social world, they do not belong to it by descent, and descent itself has a profoundly religious value.

This is brought out clearly in the relationship between the members of Dinka clans and what I have elsewhere called the 'divinities' of those clans, for which the commoner anthropological term is 'totem'. The Dinka comprise a large number of such clans, of which the members are all the generations of the descendants of an ancestor in the male line. Each clan has its divinity or divinities, inherited through all the fathers. The divinities are figured as plants, animals, natural forms, etc., which clan members take care not to injure; but for the Dinka, they are not themselves these material emblems, but spiritual beings. The clans are religious corporations, and the Dinka themselves speak of clansmen as being related to, and through, their divinities, and of being 'joined' or 'united' in those divinities. From the Dinka point of view, though all clansmen are equivalent in certain situations - in blood feud, for example - this clanship does not diminish the individuality of its members by making them mere units or cells of the larger organization, as some of what I earlier quoted about the predominance of the collectivity in African thought might suggest. Rather it adds something to each individual, as (on a rather shaky analogy) a strong sense of belonging to an Oxford college does not diminish the individuality of its members.

The archetypical clan-divinity, that of the most respected clans of priests, is Flesh itself, represented by the flesh of sacrificed oxen, held to be intermittently immanent in the bodies of its clansfolk, but also, like other divinities, transcendent.

It is both within those who inherit it, and outside and above them; the most spiritual aspect of the self is embedded in flesh and blood. The clan-divinities are most commonly called upon in invocations and prayers as 'that [quality, possession, very nature] of the father', and brings to mind, figured in the divinity, the life they have inherited, embody, and pass on - some vital power which indeed is part of each clansman, but does not come from him or her alone, and which informs each successive generation. When, at sacrifices, the divinity Flesh 'awakens' (in the Dinka term) in the flesh of some of those who venerate it, they become possessed. Sometimes they produce a kind of glossolalia. Occasionally they may break into short staggering runs. For the most part they appear withdrawn into themselves, their eyes unfocussed and unseeing, their muscles twitching and quivering. According to the Dinka, when thus possessed they are literally 'not themselves'.

On such occasions, there appears a dimension of the Dinka self into which an outsider cannot really enter, excluded as he is from the intensely-felt relationship of clanship which in part, at least, possession by the divinity seems to represent. The individual 'I', both public and private, is temporarily submitted to and replaced by the clan 'we', and perhaps only Dinka can tell us further what this entails.

GODFREY LIENHARDT

TIME AND TABOO, CIVILISATION AND SCIENCE:

THE WORK OF NORBERT ELIAS¹

Since the belated publication in English of the first volume of *The Civilising Process*,² the work of Norbert Elias has excited considerable interest among Anglophone social scientists. In this, his *magnum opus*, Elias traces the development of personality structure in European societies since the Middle Ages, identifying through changes in manners a general trend towards greater control over the expression of affect - as observed in the growth of taboos governing the handling of bodily functions, table manners, aggression and overt emotion. Elias is thus using the word 'civilisation' in a sense which has been unfashionable in anthropology since early this century. Since then, anthropologists have on the whole adjusted to the non-historical character of the societies they chiefly studied by contenting themselves with the depiction of *differences* in patterns of taboo from one human group to another. Being short of information about how modes of behaviour in these societies actually developed, many anthropologists would feel some inhibition about saying that restraints in particular societies are not just

¹ I should like to thank Norbert Elias himself for his helpful comments on the first draft of this article; responsibility for its final form remains, needless to say, my own.

² Norbert Elias, *The Civilising Process* (vol.I: *The History of Manners*), Oxford: Basil Blackwell 1978; originally published as *Über den Prozess der Zivilisation*, Basel: Haus zum Falken 1939 (2 vols.).

different, but greater or less than in others.³ Elias, in contrast, studying European historical societies, traces what he claims to be not simply *changes* in patterns of taboo, but changes in a specific direction, a structured process of growth or development. This brief article is intended to sketch Elias's argument and set it in the context of his other writings and the research which his work has inspired.

I

Norbert Elias was born in Breslau in 1897. After studying at Breslau and Heidelberg, he was in 1930 appointed Karl Mannheim's assistant at Frankfurt, where, as the principal members of the University's Department of Sociology, they were not members but tenants of the Institut für Sozialforschung. In 1933, Elias fled first to Paris and later to London, where he was for some years a Research Fellow at the LSE. In 1954 he went to Leicester, where he became Reader in Sociology. After retirement in the early 1960s, he was briefly Professor in Ghana. He has now returned to Germany and, at the age of 83, is still teaching and writing very actively at Bielefeld.

Über den Prozess der Zivilisation was the product of his first years in London; as Bryan Wilson has remarked in *New Society* (1977), that was not a propitious moment for the reception of a two-volume work in German on, of all things, civilisation. In fact it did not attract widespread attention until its reissue in 1969. Since then, it has been widely discussed in

³ It will be obvious that in this article I am not using the word 'taboo' in the normal technical sense current among anthropologists. Paul Kapteyn has pointed out that the word is used in two different senses, and distinguishes between 'primitive' and 'civilised' taboos. The former, associated with *Fremdzwang*, is that to which twentieth-century anthropologists have paid most attention. They have on the whole neglected taboos in the latter sense, which are associated with *Selbstzwang* and which, despite being closer to the sense in which the word 'taboo' has come to be used in everyday speech, stand in need of further investigation. See Paul Kapteyn, 'Taboo: One Word, Two Concepts', a paper presented at the conference on 'The Civilising Process and Figurational Sociology', Balliol College, Oxford, 5-6 January 1980; and, at greater length, *Taboe, Macht en Moraal in Nederland*, Amsterdam: Arbeiders Press 1980.

Germany, the Netherlands and France.⁴ But it is important to see the work as the product of its time: like several other notable books of the period, it was a response to the problem of Nazism.⁵ How could the German people, so proud of their 'civilisation', treat so many of their fellow human beings in so barbarous a way? More generally, how did people, how do people, become more - or less - 'civilised'? Elias, far from seeing the 'civilising process' as an inevitable, irreversible, iron law of history, sees it as highly contingent and precarious, a matter of delicate balances too easily disturbed. That is not surprising, for the greatest single intellectual influence, among many underlying *The Civilising Process*, was Freud, whose *Civilisation and its Discontents* (London 1930) had depicted the internalisation of the demands of social life as a difficult process fraught with tensions.

The essence of Elias's argument is that the long-term processes of social and political development which gradually, and with many reverses, promoted the internal pacification of European societies, were associated with changes which, again gradually and with many reverses, took place in patterns of individual behaviour and personality structure. The theory of state-formation processes given in Volume II⁶ is essential to understanding the argument about manners in Volume I, and it is just as original. State-formation is one aspect of the more general process of the weaving of more and more extensive webs of social interdependence. Elias traces the emergence of larger and larger territorial units out of the patchwork of tiny feudal fiefs which formed the map of

⁴ For a study of its reception, treated as a problem in the sociology of knowledge, see J. Goudsblom, 'Responses to Norbert Elias's Work in England, Germany, the Netherlands and France', in P.R. Gleichmann, J. Goudsblom and H. Korte (eds.), *Human Figurations: Essays for Norbert Elias*, Amsterdam: Stichting Amsterdam Sociologisch Tijdschrift 1977.

⁵ For example, much of the work of members of the Frankfurt School is a response on various levels to the same issue: see T.W. Adorno *et al.*, *The Authoritarian Personality*, New York: Harper 1950; T.W. Adorno and M. Horkheimer, *Dialectic of the Enlightenment*, London: New Left Books 1978; H. Marcuse, *Eros and Civilisation*, Boston: Beacon Press 1955. In anthropology, some of the works of the 'culture and personality' school such as Ruth Benedict's *The Chrysanthemum and the Sword* (Boston: Houghton Mifflin 1946) represent a related if less enduring response.

⁶ An English translation is to be published by Basil Blackwell in 1981.

Western Europe at the end of the first millennium AD. These small political units were, or soon became, unequal in power-chances, as a result of being selected as victors or vanquished in the qualifying rounds of the contest over the distribution of resources. As they competed amongst each other, some grew territorially larger by defeating and absorbing their neighbours. The larger they became, the more easily could they support larger and more effective military and administrative forces, which made it still more probable that bigger units would grow still bigger. This continuing process endowed those who had gained mastery of larger resources with two related monopolies: a monopoly of the use of physical force, which could be used both externally and internally - that is, both for war and for the internal pacification of their own territory; and a monopoly of taxation, since a fiscal monopoly was necessary to support the first. Elias's theory of the 'monopoly mechanism' is closely analogous to Marx's conception of the development of economic monopolies out of competitive markets. From an early stage, the beginnings can be discerned of the 'depersonalisation of the exercise of power', with the growth of administrative and fiscal apparatuses. This went hand in hand with the growing complexity of society - itself at least in part made possible by the internal pacification of larger territories. Its consequence was that conflicts between groups within a given territory would normally be conducted without violence, or, if violence were used, conflicting parties had to contend with the use of the monopoly forces on one side or the other. In this complicated series of inter-dependently-developing power-balances - between neighbouring territorial units, between lords and vassals, and between the landed nobility and the rising bourgeoisie - kings were often able to increase the royal power by balancing the conflicting groups against one another. There emerged the absolutist states of post-Renaissance Europe,⁷ exemplified most perfectly in France, and in Versailles under Louis XIV and XV. In France too, from the sixteenth century can be most clearly seen the process of *Verhöflichung* ('curialisation' or 'courtisation'), the transformation of the old warrior nobility, which for so long had had its own territorial power bases, into mere courtiers.

Elias has in fact written a separate book entirely about the sociology of the French court-society.⁸ He shows how, as their

⁷ For an interesting discussion from a Marxist viewpoint of absolutism and the power-balances between classes which it involved, see Perry Anderson, *Lineages of the Absolutist State*, London: New Left Books 1974.

⁸ N. Elias, *Die Höfische Gesellschaft*, Neuwied & Berlin: Luchterhand 1969. Although not published until a quarter of a century later, this was to have been Elias's Frankfurt *Habilitationsschrift*.

own revenues and political power declined, and as those of the bourgeois *noblesse de robe* increased, parts of the old *noblesse d'épée* acquired positions at court, and thus became entrapped in an elaborate 'system of expenses' which ruined many if the king did not help them. The system of fine distinctions and involvement in status battles was too closely connected with their social identity for them to be able to economise like good bourgeois. Their increasing dependence on royal favour and patronage also enabled the king to use as a means of control the related system of ritual, precedence and etiquette for which Versailles is famous.

The theory of state-formation processes does not, of course, end with the absolutist states; but let us now turn to the question of how these macroscopic processes might be connected with changes in personality structure. Internal pacification would seem most obviously and plausibly connected with increased self-control over the expression of aggression. Elias (in Volume II of *The Civilising Process*) makes the point in a characteristically vivid illustration. Travelling by road was dangerous in medieval times, and it remains so today - but the nature of the danger has changed. The medieval traveller had to have the ability - temperamental as well as physical - to defend himself violently from violent attack. Today, the chief danger is from road accidents, and the avoidance of these depends to a great extent on high capacity for self-control in the expression of - and skill in warding off - aggression, whether in overt or disguised form. Elias is thinking not just of modern motorists, but also of Versailles, where so much depended on the courtiers' extreme self-control and alertness to the niceties of courtly intrigue - mostly non-violent status- and power-battles - depicted so memorably in the diaries of the Duc de St Simon.

But, if we concede Elias's argument with respect to aggression, does it also follow that he is right about the taming of affect in the much broader sense? Here, his principal sources are the numerous 'manners books', whose authors (from Tannhäuser in the thirteenth century through Erasmus and Castiglione in the Renaissance to their numerous successors down to the nineteenth century) set out the changing standards of acceptable social behaviour. They were addressing at first very small literate upper classes, and later somewhat larger audiences. They tell their readers how to handle food and conduct themselves at table; how, when, and when not to fart, burp or spit; how to blow their noses; how to behave when passing someone in the act of urinating or defecating; how to behave when sharing a bed at an inn; and so on. In earlier centuries such matters - discussion of which now causes embarrassment, or at least the sensation of a taboo having been broken - were discussed openly and frankly, without shame, and apparently needed to be discussed. Then gradually, from the Renaissance, a long-term trend becomes apparent towards standards of greater restraint and more differentiated codes of behaviour. For example, the fork and the handkerchief slowly came into use at courts, and in time spread gradually to lower ranks of society.

One may ask whether the fork and the handkerchief represent greater restraint or merely a change of fashion. Both were originally expressions of the greater 'refinement' of upper classes, and of their social distance from lower social groups; as part of an overall pattern of increasing demands of 'good manners', they can be seen as requiring greater restraint. And both are part of what Elias calls a 'sequential order' through time: the use of the fork or other utensils for eating temporally follows rather than precedes the use of the hands, just as the use of the handkerchief follows the use of the fingers. A general reversion to eating with the hands, and certainly to wiping one's nose on one's sleeve, is more or less inconceivable in the absence of some catastrophic and comprehensive regression in the structure of contemporary society. The regulation of defecation is perhaps a more clear-cut illustration of the thesis. As late as the sixteenth century, courts were making rules against the seemingly widespread practice of urinating and defecating in case of urgency in the corners and corridors. As time went on, the manners books could take such basic matters for granted, devoting less space to the niceties of how to blow one's nose with the fingers of one hand only, or to use only one (the other!) to take food from the common bowl. Later books would be concerned more with the refined etiquette governing the use of a plethora of cutlery.

In tracing the effect of developing patterns of social interdependence on personality structure, Elias is not of course arguing that behaviour in small-scale societies or in the medieval period lacks all social patterning.

The expression of feeling by people in the Middle Ages was altogether freer and more spontaneous than in the subsequent period. But it certainly did not lack social patterning and control in any absolute sense. There is, in this sense, no zero-point. But the type, the strength and the elaboration of the taboos, controls and interdependencies can change in a hundred ways. And as these change, so does the tension and equilibrium of the emotions and, with it, the degree and kind of gratification which the individual seeks and finds. (*The Civilising Process*, i, 215)

So though there is no zero-point in self-control and 'civilisation', Elias does contend that the Superego becomes stronger - gradually, precariously and with regressions - as more elaborate social interdependence exerts increasing pressures on the individual. One corollary is that the distance between adult and childhood behaviour increases. Whereas Philippe Ariès in *Centuries of Childhood* places the emphasis on 'the discovery of childhood' from the seventeenth century, the changes Ariès observes would be explained by Elias rather as the effects of changes in adult standards of self-control.

To the modern layman, if not the modern anthropologist, it may seem obvious that many of the changes Elias describes must have come about for reasons of hygiene. But Elias is able to

show that in each case thresholds of shame and embarrassment rose first; only later were reasons of hygiene advanced as *post facto* justifications of the new standards. For instance, when spitting was accepted and frequent, it was said to be unhealthy to retain sputum; only after spitting became socially unacceptable was it declared unhygienic. Siding in effect with Freud against Max Weber, Elias declares that "Rational understanding" is not the motor of the "civilising" of eating or of other behaviour.⁹ The justification most frequently given initially for new standards of restraint was that the former unrestricted behaviour shows a lack of respect for associates, particularly social superiors. Reasons of hygiene became prominent only in the later period when upper-class standards of shame and restraint were spreading to all ranks of society.

Elias's account of the connection between changes in manners and in social stratification and social power is complex. He argues that from the Renaissance onwards, 'feelings and affects are first transformed in the upper class, and the structure of society as a whole permits this changed affect standard to spread slowly throughout society.'¹⁰ This is in marked contrast to the medieval period, when the social figuration was less conducive to the permeation of models of behaviour through society as a whole. A code of behaviour, like knightly chivalry, might apply to one estate throughout Christendom, without much affecting the quite different behaviour of other strata. In early modern Europe, transitionally, forms of behaviour were often considered distasteful or disrespectful in social inferiors which the superiors were not ashamed of in themselves. Thus it was disrespectful for a man to appear unclothed before a man of superior rank, yet for the superior to do so before the inferior could be a sign of affability. Yet by the twentieth century, symmetry was established, and largely similar patterns of shame and restraint expected equally of all classes. Elias's explanation - to simplify it greatly - is in terms of the advancing division of labour creating much closer and less unequal interdependence between social strata, a process of 'functional democratisation' leading to more equal power-balances. For example, as de Tocqueville noted long before Elias, in a highly unequal society, members of powerful strata have no real conception of physical

⁹ *The Civilising Process*, i, 116. See also J. Goudsblom, 'Zivilisation, Ansteckungsangst und Hygiene', in P.R. Gleichmann, J. Goudsblom and H. Korte (eds.), *Materialien zu Norbert Elias's Zivilisationstheorie*, Frankfurt: Suhrkamp 1979, pp. 215-253; and J. Goudsblom, 'Rationalisation and Civilisation', paper presented at the conference on 'The Civilising Process and Figurational Sociology', 1980.

¹⁰ *The Civilising Process*, i, 115.

suffering in lower strata - nor, in times of rebellion, do lower strata exercise much restraint in cruelty towards their masters.¹¹

II

A thesis on so large a scale as *The Civilising Process* plainly calls for testing through comparative studies aimed at discovering whether analogous processes have occurred in other cultures. Elias himself has hinted that they can be observed in the classical world. And it would be extremely interesting to know whether they were found in China or Japan; one would expect to find similarities, but also differences - the use of chopsticks rather than forks would scarcely constitute a refutation of the theory! So far, however, such studies have not been undertaken. One obstacle is that many historians appear to be affronted by the sheer boldness of Elias's hypothesis. As for anthropologists, perhaps they are too far gone in relativism to give the thesis the initial credence which is necessary to justify an inevitably difficult comparative study.

Nevertheless, sociologists, anthropologists and historians especially in Holland and Germany have begun to explore in a more limited way particular implications. One of the most obvious issues is whether the relaxation of many taboos and the relatively easier manners of the present century represent a reversal of the civilising process. Elias noted this development when writing between the wars, and he considers that while the long-term trend of the civilising process has been clear, in the shorter term there have always been waves and spurts in both directions, and that these are normal and still possible. Elias rejects one interpretation of contemporary trends which might find popular favour, that 'law and order' is breaking down in Western societies, and that 'civilised standards' are declining in consequence. His own tentative interpretation is that relax-

¹¹ See John Stone and Stephen Mennell (eds.), *Alexis de Tocqueville on Democracy, Revolution and Society*, Chicago: University of Chicago Press 1980, pp. 36, 102-106. For evidence on the decline of judicial torture in Europe (interpreted from a very different theoretical perspective) see Michel Foucault, *Discipline and Punish*, London: Allen Lane 1977. Elias also comments on cruelty to animals - see *Civilising Process*, i, 203-204.

ation of controls in *specific* instances depends upon the internalisation of self-restraints *generally* having proceeded still further and become less problematic than they were then. There is, as he puts it, a 'highly controlled decontrolling of emotions'. Two examples of specific relaxations which might illustrate this principle are the trends towards scantier dress on the beach (and elsewhere) and towards easier divorce laws. Notable essays by Cas Wouters and by Abram de Swaan have explored this issue of the 'informalisation process'.¹²

Much of the work now being done in Germany and the Netherlands on aspects of the civilising process remains for the moment either unpublished or untranslated into English.¹³ Two substantial studies which are readily accessible, however, are Anton Blok's *The Mafia of a Sicilian Village 1860-1960* (Oxford 1974) and Eric Dunning and Kenneth Sheard's *Barbarians, Gentlemen and Players* (Oxford 1979). Blok's book, subtitled 'A Study of Violent Peasant Entrepreneurs', is a good example of an historically-orientated anthropology. He interprets *mafia* as a social *process* in which people willingly resort to the private use of violence as a means of control; the incidence, patterning and fluctuations of violence are related to the failure of the Italian state (except to a large extent under Mussolini) to establish an effective monopoly over the internal use of violence. Dunning and Sheard, on the other hand, present a sociological study of the development of rugby football. They show how the public schools and the structure of Victorian society in general provided a favourable milieu for the 'civilising' of the unbelievably violent traditional folk games and their codification into the modern games of football.

¹² Cas Wouters, 'Informalisation and the Civilising Process', in Gleichmann, Goudsblom and Korte (eds.), *Human Figurations*, *op. cit.*, pp. 437-453; Abram de Swaan, 'The Politics of Agoraphobia', paper presented at the conference on 'The Civilising Process and Figurational Sociology', 1980.

¹³ It is hoped that a collection of papers will eventually be published under the editorship of Eric Dunning and myself.

III

Though *The Civilising Process* remains his pivotal work, Elias has in more recent years written extensively on many other subjects. A minor work, but one of some interest to anthropologists, is *The Established and the Outsiders*, a study of a community near Leicester.¹⁴ It depicts three neighbourhoods, the residents of two of them being working-class groups of very similar social composition but differing in length of settlement, and remaining in conflict over several generations along this established/outsider axis. Gossip networks are one of the interesting means by which the three groups are distinguished. Elias advances the established/outsider distinction as a pair of categories more general in scope than such conventional concepts as class and status-group, and he employs it again in his recent work on scientific establishments.¹⁵

The sciences - or, to be more exact, theories of knowledge scientific and non-scientific - have been the subject of many essays by Elias in recent years. One of the earliest, 'Problems of Involvement and Detachment' (*British Journal of Sociology* VII, 1956, pp. 226-252), shows clearly the link between *The Civilising Process* and his interest in the sciences. He presents detachment (or what used to be called 'objectivity') not as some heroic psychological quality of individuals, nor as any finally attainable terminal state, but as the collective achievement of many generations: the gradual development of relatively autonomous communities of scientists makes possible greater social control over the expression by scientists of individual affect and fantasy with respect to physical, biological and - eventually - social data. Though again there is no final state, scientific knowledge gradually becomes more 'object-adequate'. That may sound rather Comtean, and indeed Elias in *What is Sociology?* (London 1978) is not afraid to acknowledge the value of some ideas found in the work of the founder of positivism; but his reading of Comte, as of Marx, is highly critical and selective.

¹⁴ N. Elias and J.L. Scotson, *The Established and the Outsiders*, London: Frank Cass 1965. See also Elias's important essay, 'Towards a Theory of Communities', Foreword to C. Bell and H. Newby (eds.), *The Sociology of Community*, London: Frank Cass 1974.

¹⁵ Forthcoming in R.D. Whitley (ed.), *Sociology of the Sciences: A Yearbook*, 1981.

However, it is certainly true that Elias would see Western science as more 'civilised' and 'advanced', more 'object-adequate', than the cosmologies of tribal societies; in terms of anthropologists, one might say he is with Jack Goody (*The Domestication of the Savage Mind*) and against Evans-Pritchard (*Witchcraft, Oracles and Magic among the Azande*) and Lévi-Strauss (*The Savage Mind*).

IV

When sociologists write about the sciences, they are usually implicitly concerned to say something about the nature of their own discipline. And indeed Elias has developed a highly coherent view of the social sciences which in many respects makes him an 'outsider' in relation to some present-day tendencies. It is impossible in a still briefer conclusion to a brief article to do more than hint at this view. Let it suffice to say that his central categories are interdependence rather than interaction, and power-balances and processes rather than static structures - though it is possible to speak of the structure of processes.

Unlike the over-used concept of 'interaction', which leads the social scientist into difficulties as soon as he attempts to go beyond the small face-to-face group, creating an artificial gap between micro and macro 'levels of analysis', the idea of interdependence can be used in investigating human figurations from the smallest to the largest. Interdependence leads directly to the idea of power-balances, which may be more or less unequal, more or less unstable, and which are found in figurations of every scale. Thus Elias's was one of the first sociological attempts to discuss within the same framework inter-state, intra-state, inter-group and interpersonal processes.

'Interdependence' has further significance in relation to the nature of social scientific theories. Elias argues that the greater the degree of interdependence, integration or inter-connection found in a science's subject-matter, the more inappropriate is methodological atomism - by which in this context he means the attempt to explain the properties of complex wholes in terms of the properties of their constituent parts. The method is less appropriate in the biological than the physical sciences, and less in the social sciences than the biological. Timeless, reversible laws on the model of physics, the science which has inappropriately been taken as the model for all the sciences by generations of philosophers, are not a fruitful goal for social scientists. They should seek instead 'process-theories'.

Human societies are unintended, blind social processes which, though produced by the interweaving of intended actions of countless individuals, are unintended and unplanned by any particular individuals. They nonetheless possess structure; the division of labour, the monopoly mechanism and the civilising process are three of many strands of such processes. All attempts at conscious planning - as for 'development' - must be made against the background of such unplanned processes. This processual view, Elias tries to show, overcomes the sterile traditional problem of 'the Individual' and 'Society'; that is a chicken-and-egg issue, the result of using concepts which falsely isolate and freeze two aspects of one process. These concepts have their origin in the egocentrism of Western philosophy since the Renaissance. The old issue of freedom (or 'free-will') versus determinism is seen in similar terms: 'it is usually forgotten that there are always many mutually interdependent individuals, whose interdependence to a greater or lesser extent limits each one's scope for action.'¹⁶ The static polarities of voluntarism and determinism can be replaced by the investigation of the constraints exerted on individual people as a result of their location in a particular network of interdependence, and of the compellingness of particular social processes. As for whether there are long-term 'laws of historical development', Elias characteristically again turns the question into one with empirical reference. He argues that social figurations differ in their 'potential for change'; study of a past figurational sequence may always show that figuration B *had* to be preceded by figuration A. But it is not always very easy to show that figuration A *had* to be followed by figuration B. In some cases, it may be possible to show convincingly that an earlier figuration had so little potential for change that a later figuration was the only conceivable outcome, but in many cases alternative outcomes were possible. Once again, in a typical way, Elias here turns the dichotomous poles of an old quasi-philosophical controversy - his target here, of course, is Popper - into a more subtle, processual form.¹⁷

Perhaps the notion of *Zustandsreduktion* ('process-reduction') is as central as any to Elias's thinking.¹⁸ By it, he means

¹⁶ *What is Sociology?*, *op. cit.*, p. 167.

¹⁷ See *ibid.*, ch. 6, 'The Problem of the "Inevitability" of Social Development'.

¹⁸ There are some parallels between Elias's use of the idea of *Zustandsreduktion* and Adorno's use of 'reification' (*Verdinglichung*) - see Gillian Rose, *The Melancholy Science*, London: Macmillan 1978. However, there are also striking differences between their concerns: most obviously, though Elias's work shows the influence of Marx, it is not Marxist, and its style is far less metaphysical in tone.

the tendency in everyday and social scientific thought to reduce processes to states. Whorf suggested that this pattern (as seen in an expression like 'the wind blows') was inherent in the linguistic structure of Standard Average European. Elias would also see it as inherent in the egocentric and ultimately solipsistic philosophical tradition which runs from Descartes through Kant to Husserl and Popper. Elias rejects the search for the universal and timeless structures underlying the flux and diversity of social processes - a search which Lévi-Strauss for one elevated to the cardinal principle of his anthropology.

STEPHEN MENNELL

BYWAYS IN OXFORD ANTHROPOLOGY

Selections from the Minutes of the Oxford University Anthropological Society

1909: The Magic Lantern and the 'Messrs.' of Oriel and Lincoln

The Oxford University Anthropological Society was founded in 1909. The first page of the earliest minute book opens in the following manner:

Historical Note [sic]. The idea of founding an Anthropological Society which has been simmering in Oxford for many years, took definite shape during the Christmas Vacation of 1908-1909 in the minds of Mr.R.R. Marett, M.A. and Mr.G.C. Robson of New College. Not only were their views independently conceived, but they were so far different that whereas Mr. Marett's project was for a general Anthropological Society (upon the lines of the University Diploma for that Science), Mr. Robson more directly contemplated Field Work of an archaeological kind in the neighbourhood of Oxford along with the considerations of such problems as would therewith arise. However, having become aware of one another's designs through the common proselytising of Mr.A. Montgomerie Bill, they decided to make common cause; and the programme of the new Society was circulated in the first week of Hilary Term 1909; and the Society formally constituted at a meeting held in Exeter College Old Bursary on Thursday, January 29th, 1909.

At this meeting Marett (later President of the Society) took a shaping role and proposed Prof.E.B. Tylor as Acting President, Mr.H. Balfour as Acting Vice-President and Mr.G.C. Robson as Secretary. Mr.J.A. Hampton was proposed as Treasurer by Robson, but he later refused the post, and the Hon.P.A. Methuen was proposed in his place. The annual subscription was agreed upon at 2/6 per annum.

In deference to Robson's particular interests, a field section was formed and time was allotted at meetings for it to present its finds. One detects a hint of anomaly in its position: every time the field section asked for funds in 1909, a decision was postponed to the next meeting. It had a shadowy presence and it is noted on February 26th that 'the Acting President and Secretary made statements with regard to the field section'.

Possibly the field section was playing a hidden role because on May 11th the Secretary was instructed to send round notice 'of the need for members interested in the Bronze Age to announce themselves'.

Equally elusive were the college secretaries. On June 1st it was decided to have secretaries in colleges for recruitment and publicity. Only after their election was it discovered that three of them had already left Oxford. The secretaries from Oriel and Lincoln were so discreet as to have no recorded names except 'Mr.'

An 'extraordinary' meeting of the Society took place on May 10th. Mr. W. McClintock gave a paper on 'The Blackfoot Indians of the Rocky Mountains (with Lantern Demonstration)'. No account is given of the content of the talk or discussion; it is clear however that this was the first time a lantern had been used. Obviously this set a trend because a further three talks that year out of a total of nine were illustrated by lantern slides. There was no innovation in reporting the content. In the following years, the lantern seems to have been in constant use; under the supervision of an official operator. In 1916 the tired lantern finally broke down.

Lectures were often illustrated by specimens and, at a talk by Marett on October 22nd, '... a number of Bull Roarers were exhibited and practical illustrations of their use were given'.



22 October 1909: Mr. Marett Demonstrating his Bull Roarer

The enthusiasm of the Society can be seen from the length of the meetings which began at 8.30pm and often lasted until after 11pm. The year 1909, in which the style of the nascent Society became set, ended with a paper by Mr. Halliday entitled 'The Psychological Basis of' [sic].

The wide range of interests supported by the Society's members is indicated by the full list of talks for the inaugural year 1909:

| | |
|-------------|--|
| February 23 | Prof.E.B. Tylor on 'Archaeological Work on the Zambesi' |
| February 26 | Mr. A.R. Brown on 'A Prehistoric Religion (Andamanese)' |
| March 5 | Mr. A.M. Bell on 'The Neolithic Invasions of England' |
| April 30 | Mr. W.J. Lollas on 'The Neanderthal Race and its Most Recent Representative' |
| May 10 | Mr. W. McClintock on 'The Blackfoot Indians of the Rocky Mountains' |
| May 22 | Dr. C.G. Seligman on 'The Veddahs' |
| June 4 | Dr. W.H. Rivers on 'Melanesian Communion' |
| October 22 | Mr. R.R. Marett on 'Mr. Lang's Theory of Savage Supreme Beings' |
| November 3 | Mr. T.C. Hodson on 'Some Naga Customs' |
| November 16 | Mr. Routledge on 'Custom and Myth of the A'-Kikuya' |
| and lastly | |
| November 23 | Mr. Halliday on 'The Psychological Basis of' |

MIKE HITCHCOCK
Secretary, 1979-80,
Oxf. Univ. Anthropol. Soc.

SPECIAL SECTION ON THE LAMAIST WORLD

RECENT BOOKS ON TIBET AND THE BUDDHIST HIMALAYAS

I

GIUSEPPE TUCCI, *The Religions of Tibet* [1970], (translated from the German and Italian by Geoffrey Samuel), London and Henley: Routledge and Kegan Paul 1980. xii, 340 pp., Bibliography, Index, Maps, Ills. £8.95.

ANTHONY JACKSON, *Na-khi Religion: An Analytical Appraisal of Na-khi Ritual Texts*, The Hague etc.: Mouton 1979 [Religion and Society, vol.8]. xxii, 365 pp., Bibliography, Index, Maps, Ills. £40.60.

JAMES F. FISHER (ed.), *Himalayan Anthropology: The Indo-Tibetan Interface*, The Hague etc.: Mouton 1978 [World Anthropology Series, general editor Sol Tax]. xvi, 567 pp., Index. £30.00.

MARK TATZ and JODY KENT, *The Tibetan Game of Liberation: Rebirth*, London: Rider 1978. 231 pp., Ills., Poster/tanka game-board included £4.95 (paper).

Three Styles in the Study of Tibet

Tibet is not just a New Guinea or an Amazonia with a collection of exotic peoples, but possesses a literate tradition which has largely been tied to the power and influence of the state. Until the consolidation of Chinese rule in 1959, religious institutions occupied a central position in the administration and expansion of the polity. Moreover, for more than a millennium Tibet has been influenced by the two great civilisations of Asia, the Indian and the Chinese; it knows the former as 'The Land of Religion' and the latter as 'The Land of Law'. Their influence, which has been cultural and directly political, has been a complex assimilation and accommodation which has also taken account of indigenous and varied local social forms and ideas. Hence the development can only be termed Tibetan.

This presents the anthropologist with a major problem of method in the study of Tibetan peoples. Furthermore, the tradi-

tional society in which he is interested no longer exists within the Tibetan polity itself, and it is not clear to what degree the marked heterogeneity which characterises the fragments outside was a feature of Tibet itself. As in other parts of Asia, the major analytical problem is the relation of local forms to the literate tradition and the power of the state. In the case of Tibet this is compounded with all the problems of historical reconstruction.

In practice there have been two major academic traditions in which the study of Tibet has been attempted. The first is within Tibetology, a subject based firmly on comparative linguistic analyses of textual sources. To the anthropologist the translation of esoteric Buddhist texts away from their social context is of little direct help, though the Tibetologist's reconstructions of the cultural history of Tibet have certainly been of major significance. The second and more recent tradition is the anthropological one itself: here field-studies have been attempted either in the pockets of traditional Tibetan culture that exist in the Himalaya, or amongst refugee populations from political Tibet itself.

There have been no field-studies of the stature of Evans-Pritchard's *The Nuer* or Leach's *Political Systems of Highland Burma* in the anthropological study of Tibet. The ethnographic method of Malinowski developed at a period when Tibet was largely closed to foreigners, first by the Tibetans themselves and then, after 1959, by the Chinese. Even if such a study were made today it would be of a fundamentally different society, one that has moved from 'medieval' to 'modern' in the lifespan of a single person. Neither have there been any works of the importance of Dumont's *Homo Hierarchicus*, which is perhaps a more relevant comparison. This absence is partly explained by the lack of adequate first-hand material; Tibetology is still very much in its infancy and anthropological studies have tended, possibly unwisely, to go very much their own way without being able to see this particular and indigenous form of unity in diversity.

Along with these academic endeavours there has been a vast amount of popular and semi-scholastic writing on Tibetan culture and society; this has been the work of missionaries, explorers, botanists, geologists, hunters, soldiers, administrators and other agents of Imperial India. These vary from the banal to the brilliant, and the latter have to be taken into account in any serious consideration of our knowledge of traditional Tibet.

These are the three major styles in which Tibet has been studied in the West. Today the situation is changing: academic work has begun to integrate these three disparate styles of research, and in so doing has had, even if only implicitly, to attempt to come to terms with the problems that these different perspectives raise. In this review I will be concentrating on works that are a positive development in this direction, bringing together the textual with other forms of writing.

Giuseppe Tucci, the author of *The Religions of Tibet*, is a major figure in the growth of Tibetological studies in this century. He is both a pioneer and an authority, responsible for the journal *Indo-Tibetica* (Rome 1932-1941, 7 vols.), *Tibetan Painted Scrolls* (Rome 1949, 3 vols.), *Tibetan Folk Songs from Gyantse and Western Tibet* (Ascona 1966, 2nd. edn.), as well as numerous minor works. The present work was first published in 1970 by Kohlhammer as *Die Religionen Tibets* in *Die Religionen Tibets und der Mongolei*, by Giuseppe Tucci and Walther Heissig, the work having been translated from the Italian manuscript by Gustav Glaesser. In 1973 it was published in French by Payot as *Les religions du Tibet* in *Les religions du Tibet et de la Mongolie*, having been translated from the German by R. Sailley.

Quite correctly the work is here published by Routledge on its own. The translation into English by Geoffrey Samuel, whilst purportedly from the Italian and German, bears in part a French ancestry, and it is rather odd that no mention whatsoever is here made of the Payot volume. Samuel has improved on the index of the work, extending it to cover more Tibetan terms which are now in the conventional transliteration of T.V. Wylie ('A standard system of Tibetan transcription', *Harvard Journal of Asiatic Studies* XXII, 1959, pp. 261-67), rather than the particular system of L. Petech (in *Handbuch der Orientalistik*, Leiden 1966, part 1, Vol. V, Ch. 5, p. 347). The maps have also been changed and now emphasise political areas rather than towns and religious sites.

For all its circuitous path into English the work is as clear as the other two translations. It stands, along with two other classics (R.A. Stein, *Tibetan Civilisation*, translated from the French by J. Driver, London 1972; and D.L. Snellgrove, *Buddhist Himalaya*, Oxford 1957), as a major contribution on Tibetan religion and society. These two are the only other volumes which in their scope and scholarship bear comparison, and the three together are necessary reading for any student of Tibet.

In breadth Tucci goes beyond Snellgrove, dealing not only with western Tibet and the Himalaya, but the area in its entirety; in depth he goes beyond Stein, his first-hand familiarity with the area and people allowing him to rise above the texts, which illustrate rather than define his exposition. As well as a number of journeys to southern and western Tibet (in 1931, 1933, 1935, 1937 and 1939), he travelled in central Tibet in 1948, visiting Lhasa, Shigatse and Gyantse. His familiarity with spoken as well as literary Tibetan allowed him direct access to the ideas of the people more in the manner of an anthropologist than a literary historian as such.

The work is divided into three main sections. The first two chapters are a historical account, at once cultural and political, of the arrival of Buddhism in Tibet. The next two chapters deal with the religious thought of various Buddhist schools as represented in Tibet, and the following two chapters deal with religious institutions in their local context. In addition there is a chapter on the Bön religion. The term Bön is used confusedly in Tibetan studies, being variously applied to pre-Buddhist thought, non-Buddhist lay-belief, and to the present-day organised Buddhist

church of that name. For the main part Tucci uses the term in the first and third senses, but here he does not progress beyond a consideration of the corpus of texts he possesses, which he admits are inadequate (p. 248). To a large degree this Chapter has been supplanted by Snellgrove's work *The Nine Ways of Bon* (London 1967).

His account of the doctrines of the most important schools of Buddhist thought in Tibet is of particular interest, as he is not content solely with tracing the names of writers and their affiliation against textual sources, but attempts to give the substance of their doctrine in potted form. This is the philosophical basis of what he himself describes as a gnostic, and hence esoteric, mode of thought (p. 50), and hence a particularly difficult task. For those already initiated into the concepts of Indian Buddhist thought this stands as an extremely useful summary; for those not acquainted with this terminology the ideas are too condensed and opaque to be intelligible from this Chapter alone, which can be no more than a pointer for study.

For the anthropologist the two chapters on religious institutions are the most interesting, especially the second, entitled 'The Folk Religion'. His technique is to contrast the religion of the virtuoso to the beliefs of laymen, a device which Tucci surely appreciates is a convenience for exposition rather than an absolute division. By his use of the term Lamaism rather than Monasticism he avoids the problem, otherwise difficult to resolve in the Indian Buddhist tradition, that Tibetan religious orders are not necessarily monastic. In doing so he is resuscitating a term made famous, or rather infamous, by L.A. Waddel (*The Buddhism of Tibet, or Lamaism*, London 1895), about which Tucci has written elsewhere that it ought no more to be used for Tibetan Buddhism (*To Lhasa and Beyond*, Rome 1956). It remains to be seen whether this label has rid itself of its earlier connotation of 'degenerate monasticism', and will find wider approval.

The label 'folk' also has its drawbacks; but the substance of this Chapter is unmistakably excellent. It is an account of the different categories and levels of interpretation of local spirits, in which Tucci pays attention both to the variations due to the diverse economic and social forms in Tibet, and to their historical relationship to the Buddhist tradition. In addition he dwells in some detail on the forms of ritual used by religious specialists when performing various types of exorcism or spiritual purification for the laity. However, it has to be said that although this account is of major importance to a student of the area or of popular forms of Buddhism, the analytical level is not sophisticated enough to be of use to the anthropological comparativist: terms such as 'magic', 'hallucinatory level' or 'special psychological state', although suggestive, beg too many questions for a theoretician. This criticism is minor and Tucci is to be congratulated for having come into range at all in this work; its excellence lies both in its ethnographic description and in its broad and cogent account of Tibetan thought at a number of levels and in a historical context.

Anthony Jackson's *Na-khi Religion* is a detailed and scholarly work. The Na-khi are a Tibeto-Burman speaking group located to

the north-east of the Himalaya, where Burma, China, India and Tibet coincide. They have Tibetans to their north, the Lo-lo to their east, and are closely related to the adjacent Mo-so. As in the case of political Tibet, the form of society described disappeared with the expansion of Communist China. However, much of the excellence of Jackson's work consists of a historical analysis that connects the differentiation of the Na-khi from the Mo-so, and the creation of the pictographic script and texts that characterise the ritual described in this book, to equally circumstantial political events. He is aware of the need to explain the rise of this religious form in both a social and a historical context. At the same time he considers the symbolism of the texts in great detail and analyses the cosmology as represented by myth, belief and action in the context of ritual, to derive a basic structural framework, which he then extends to include social forms. The work is both empirical and conceptual.

The West first knew of the Na-khi through their pictographic writings in the nineteenth century, which were then held to be an example of the original script of man (A. Desgodins, *Mission du Thibet*, Paris 1872). There are over 10,000 of their texts in Western libraries, the majority of which were collected by or in reaction to the purchases of J.F. Rock, an American botanist, who stayed in the region in the first part of this century. Jackson's volume is largely based on his analysis of the texts, the semi-scholastic writings of Rock, and the work of Lin-ts'an Li on the Mo-so (*Academia Sinica*, Taipei, 1953-1960).

The first part of the book is a critique of previous writings on the Na-khi by way of a painstaking historical and social reconstruction. He criticises Rock for his amateurish ethnology: Rock held that Na-khi religion is a pure survival of pre-Buddhist Tibetan Bön religion, and that a golden age of Na-khi writing existed in the sixteenth century; indeed, he criticises him for his mystification of Na-khi studies generally. The second point is the easiest to verify: it appears that over 90% of the Na-khi texts in the West are copies of one another, that the ceremonies that Rock has translated are unusually long, and that all ceremonies are made up of smaller sub-ceremonies which are themselves only various combinations of a limited set of smaller elements. Jackson connects the lack of standardisation of texts with the absence of an organised church: each religious specialist could create his own variation. He connects the origin of the script with the Mo-so phonetic script, seeing a shift towards pictography as a natural development for rituals in which a text was little more than an *aide-memoir*, especially given the problems of the representation of a tonal language. He sees the original form of the ritual as being that of the Tibetan Bön religious order as it existed in the seventeenth and eighteenth centuries. This Bön order was then proscribed by the Chinese authorities and survived only at the local level amongst a peasantry to whom the Tibetan script was unintelligible. These lay religious specialists recorded and developed the religious tradition in their own manner. Hence the origin of Na-khi ritual texts.

Jackson considers the study of Na-khi society to have a textual bias. By a careful consideration of the kinship terminology, mythology, history, patronyms, and inheritance, he shows why it is likely that they were originally matrilineal, and why it is only latterly that they acquired those characteristics that have led other writers mistakenly to identify them as being 'basically' patrilineal. Furthermore, this allows him to explain the dramatic increase in female suicide in the eighteenth century in terms of the forced adoption of patrilineal institutions after the Chinese pacification of the area in 1723. These suicides, Jackson continues, in their turn increased the need for local religious specialists, and when in the nineteenth century the Na-khi profited from the opium trade, the increased wealth allowed a growth of religious activity. Hence the proliferation of religious texts, especially after 1875.

Put as straightforwardly as this, Jackson's argument may appear to be too simple to be true. In his text, however, it is his close comparative technique, in which he examines the effect of various factors and events in different locations and within different groups, which gives his historical analysis substance. These social themes are also taken up in the second and longer part of the work, which is devoted to the analysis of symbolism. The analysis here is complex and is given first at the level of the classification of the cosmology and then in the context of belief and ritual which is subsequently analysed into various types of dyads. He considers the form and sequence of the ritual and gives a detailed account of one ceremony before returning to social parallels. Na-khi religious practice is clearly parallel to Tibetan ritual; only here the popular conceptions of spirits and deities have become dominant rather than subsidiary. The major concern is with ritual action to remove the effects of misfortune and misdeed, and to promote health and well-being; it completely lacks the Buddhist concern with soteriology. The name of the people itself suggests this concern with ritual: *Na-khi*, like the related term *sngags-pa* in Tibetan, has the sense of 'black' and 'ritual specialist'. In outer form, the altar and the dress of the specialists, like the form of the ritual itself, show a clear Tibetan influence. According to Jackson, the indigenous term for ritual specialist, *dto-mba*, is a derivation of a Tibetan term, *tönba*, which means priest. Since he is unable to give a transliteration of this latter word in Tibetan, it is not obvious to which Tibetan word he is referring. It could be spelt in any one of a dozen ways (and in any case would not be a common term for priest or religious specialist). He also evidently knows little about Tibetan religious texts in that he considers mantras to be Tibetan, rather than Tibetan transliterations of the Sanskrit: they are as foreign to the Tibetan as they are to the Na-khi texts. One could, in addition, criticise his account of the anthropological study of ritual, and his style of exposition which is unnecessarily repetitious at times. These are however minor points, and in no way detract from Jackson's scholarship and competence: his is surely the definitive work on the Na-khi, and a model of reconstructive methodology.

Himalayan Anthropology, edited by James F. Fisher, is also a major contribution, if only because of its size: it contains no less than thirty-five articles. Of these, eleven are on directly Tibetan themes, including a lucid summary by Jackson of his work on Tibetan Bön Rites in China. The collection derives from the massive Chicago conference of 1974. The standard of the papers included in the volume is extremely varied and there is no common theme or approach other than that of geographic area. Robert Ekvall ('Correlation of Contradictions: A Tibetan Semantic Device') gives an interesting account of the way in which the Tibetan language can form abstract categories from polar concrete pairs, such as 'weight' from 'light-heavy'. Chögyam Trungpa ('Some Aspects of Pön') gives some fascinating ethnographic information on Bön; however he is quite uncritical as to which sense of the term he is writing about, and many of the deities that he describes will be found in Tucci's *Religions of Tibet* in the Chapter on the Folk Religion. John Ardussi and Lawrence Epstein ('The Saintly Madman in Tibet') provide an excellent account of the Tibetan classification of certain states of mental abnormality, which they also characterise in the terminology of clinical psychology. Their central concern is the *bla-ma smyon-pa*, the 'saintly madman', who is a major character in the oral and literary tradition of Tibet. His position is described both in relation to the other forms of Tibetan madmen, and in terms of Buddhist philosophy: their treatment is both entertaining and scholarly. William Stablein's paper ('A Descriptive Analysis of the Content of Nepalese Buddhist *Pujās* as a Medical-Cultural System with References to Tibetan Parallels') is also, in spite of its ponderous title, of interest in the understanding of esoteric Tibetan ritual. There are, of course, many other articles in this volume which bear more on the particular situation of the Himalaya than on Tibet itself.

The earliest uninformed and popular writings on Tibet can be dated back to the thirteenth century and the visits of Western clerics to the Mongol Court. They hoped to find an ally in the form of a vassal of the ubiquitous Prester John to enlist in the fight against evil, here represented by the tribes of 'Gog and Magog'. Friar William of Rubruck, who saw Tibetan clerics at the Mongol court in 1254, describes figurines of 'praying bishops' and a 'winged St. Michael' along with shaven-headed monks in saffron robes. To him these were aberrant Christian relics of the influence of Prester John, not the representatives of a coherent religion. Another cleric, equally intent on allying his State with the Mongols against the forces of evil, visited the same court in 1247. This was Sa-skyā paṇḍita (1182-1251), also known as Sa-pon, a Tibetan monk of the Sa-skyā sect; he must have had an accurate appreciation of the political realities as the subsequent support of the Mongols was instrumental in ensuring the power of the Sa-skyā order in Tibet until the middle of the fourteenth century.

Sa-pon is also credited with the invention of the 'Game of Rebirth' which is the subject of this entertaining volume by Mark Tatz and Jody Kent (*The Tibetan Game of Liberation: Rebirth*). The work consists of introductory essays on the popular notion of

rebirth according to one's moral worth - that is the law of 'just returns' of *karma* in the context of Tibetan Buddhism, and on the origin, history and form of the game. The book contains a coloured print of a Tibetan painting of the board (75cm. x 50cm.) which in this version has 104 squares leading from the lowest of the hells up through this world, the heavens and then the Buddhist path to *nirvana*. The game can be considered either to be educational, or as a device for legitimating a Buddhist hierarchy; this depends on one's point of view. Certainly it is of interest that from square 38 onwards to 104 the player, already above the heavens, only then for the first time encounters Buddhist monastic vows. Until the highest levels the board makes a clear distinction between the Mahāyāna and Vajrayāna paths within Tibetan Buddhism, and the highest levels of all are reserved for the sermons of the historical Buddha, Gautama.

The dynamics of the board are interesting: below a certain point it is difficult to rise, and above a certain point it is impossible to fall. Again, this suggests that the only way one can be certain of 'permanence away from suffering' is through Buddhism. The player moves by a throw of a die and here the rules cleverly cater for greed: high throws invariably lead the player downwards. Unlike most successful Western board-games there is no opportunity to attack, delay or extort money from other players; but then this is a Buddhist game in which such activities would invariably lead the player downwards. It is also true to the higher Buddhist tradition rather than to popular belief in that there can be no transfer of 'merit' between players: you cannot help your friends and success is a solitary endeavour.

As in Dante's inferno the tortures of the hells are chosen specifically to suit the crimes of the person. For this reviewer the major social enjoyment consists in seeing a fellow-player descend into the hells, and hearing him read out the description of the sins he has committed and the corresponding tortures to which he must be subjected. The reviewer having spent most of his own time in the hells, his assessment of the true merits of the game may be a long way from that seen from the Buddhist path. However the following description at p. 69, taken from the 'Howling and Great Howling Hells' (*du-'bod che-chung*) may give the flavour of the game:

One falls laughing, it is said, into the Howling Hell The Great Howling Hell arises from lying or harsh speech with spiteful intention: perjury, bribery, betrayal of trust, false advertising, and venal government or justice. One who has lied while quarrelling with a companion has his tongue grow to great length. Demons then dig in it a deep ditch, which is filled with molten copper; worms are born in its sides, which devour the tongue.

This is, one might think, a hell especially reserved for book-reviewers or writers in general. Possibly one corner of it is kept for the writers of uninformed popular works on Tibet, such as Michel Peissel's recent work on Zanskar (*Zanskar: The Hidden Kingdom*, London 1979): being battered by Buddhists with BBC-TV

boom-microphones off a bridge into a Himalayan gorge would be too good for him. This is a fate that Tatz and Kent would be able to avoid. Like any truly popular work it can be faulted on academic grounds, although I was pleased to note that the square marked Bön distinguishes between Bön in the sense of a Buddhist religious order and other meanings of the term. In most cases the references to the secondary and in some cases primary textual sources are clear, and contained in separate notes to the chapters. The work is a splendid compromise and is far removed from the excesses which now as always characterise much of the popular writing on Tibet.

GRAHAM E. CLARKE

RECENT BOOKS ON TIBET
AND THE BUDDHIST HIMALAYAS

II

BLANCHE OLSCHAK, *Ancient Bhutan: A Study on Early Buddhism in the Himalayas*, Zürich: Swiss Foundation for Alpine Research 1979. 173 pp. Ills., Maps, Annexes, together with Map in end-pocket. SFr 196.00; limited edition of 1000 copies.

SHERRY B. ORTNER, *Sherpas Through Their Rituals*, Cambridge etc.: Cambridge University Press 1978 [Cambridge Studies in Cultural Systems, editor Clifford Geertz, no.2]. xii, 185 pp., Ills., Bibliography, Index. £7.50 (£2.95 Paper).

MICHAEL ARIS, *Bhutan: The Early History of a Himalayan Kingdom*, Warminster: Aris & Phillips 1979. xxxiv, 330 pp., Ills., Table, Maps, Bibliographies, Indexes. £12.50. Microfiche Supplement, xviii, 173 pp., available separately from publishers.

Although China is showing signs of relaxing its policy towards central Tibet, following its recent admission of repressiveness and of the fact that the economic condition of the annexed country is now even worse than it was when 'liberated' thirty years ago, there seems to be little hope for the restoration of the traditional Tibetan culture and government. Apart from settlements of refugees in India and other parts of the world including

Switzerland, pockets of Tibetan civilisation continue to thrive outside the border which defines the limit of Chinese power. The last of the books reviewed here deals with one such community in Nepal, the Sherpas. However, the only country to preserve a 'lamaist' state is Bhutan, which, understandably worried by the fate of its northern neighbour, has kept its borders closed to foreigners.

We should therefore be grateful to the few writers such as Blanche Olschak and Michael Aris, who have been afforded the exceptional opportunity of visiting that country, though few have produced books as indicative as theirs of the authors' interest in and understanding of their subject. However, in reading Olschak's book we find certain areas in which it is not unequivocally successful, and I consider some of these before returning to its merits.

One of the first questions that must be raised concerns the extent to which it is possible to write a history of early Bhutan without recourse to Bhutanese and Tibetan sources from different periods. The fact is that Olschak bases her reconstructions on legends and popular accounts - her references are almost all secondary Western sources - and she apparently has a limited knowledge of literary Tibetan. One of the few Tibetan-language texts which is used is a late biography of the 'Sindhu Rāja', translated with the help of Geshé Thupten Wangyal, and quoted in its twenty-five page entirety, but unfortunately without critical comment or evaluation as to its historical merit. That it assumes such a central position in the book is justified by its being the 'First Translation of a Hidden Book Treasure [*gTer-ma*] of Bumthang [in Central Bhutan]', but it is not mentioned that a section of this work, translated by Michael Aris, is to be found in Mehra's *Bhutan* (Delhi 1974), which was published six years ago and which is, incidentally, cited in Olschak's own bibliography.

Another factor which perhaps diminishes the value of this book as a plausible historical survey is the author's cautious avoidance of criticism of any aspects of the Bhutanese polity. It is understandable that a writer should wish to be tactful and not to give offence, but there are a few instances in which euphemism leads to misrepresentation when it is implied that the ideal situation was actual. For example, Olschak understates the burden on the ordinary peasant of forced labour services, and suggests that the requisition by government officials of animals for transport purposes was not abused.

It was ... usual that travelling officers, in fulfilment of their duties - *and only then, misuse was forbidden by law* - had to be provided with mounts and provisions by the inhabitants of every district they crossed. (Emphasis added).

We might compare this with the account of an earlier European visitor to Bhutan, Ashley Eden, who remarked acidly that

The only limit on the revenue demand is the natural limit of the power of the officials to extort more. Nothing that a Booteah possesses is his own; he is at

all times liable to lose it if it attracts the cupidity of any one more powerful than himself. The lower classes, whether of villagers or public servants, are little better than the slaves of the higher officials.

(*Political Missions to Bootan*, Calcutta 1865, p. 118)

This certainly represents an extreme view, but one which must be taken into consideration in any fair assessment of the Bhutanese revenue system.

To give another example, on page 31 there is the almost off-hand mention of a sequence of events which alerts the reader to a certain bias in the author's attitude. In a single paragraph we are told that for the first twelve years after the ascension to the throne of King Jigme Dorje Wangchuk, the latter 'stayed in the background' until the acting prime minister, Jigme Palden Dorji, 'was killed by an attempt of murder' (*sic*), and his brother, who had succeeded him resigned and emigrated a few months later, 'stayed abroad with his family'. The following year, an unsuccessful attempt was made on the king's life, and finally, we are told that 'in 1967 H.M. the King himself, enthusiastically acclaimed by the whole population, took over the position of a prime and foreign minister' - as if this assumption of power were a *non sequitur*. Although it is only possible to speculate on the implications - if any - that the author is making here, it is clear that her overt intention throughout is to present as harmonious a picture as possible of the country's internal affairs. However, it is for the reader to decide whether such evasive passages as those cited above are intended for acceptance at face-value, or whether they are meant to be transparent to the perhaps more cynical Western reader than to certain understandably anxious Bhutanese protagonists.

There is also evidence that the author is ill-acquainted with certain aspects of the broader perspective involving more specifically Tibetan subjects. For example, she rather misrepresents the Bön religion, dismissing it as 'prehistoric' and as part of the 'Shamanist Tradition', which, again, represents the 'official' view of the Tibetan theocracy and presumably of the Bhutanese hierarchs. Sufficient work has been done on the earliest written records from Tibet to demonstrate that only one facet of the early religion, that which was fully in evidence recently as what Stein (*Tibetan Civilisation*, London 1972) has called the 'Nameless religion', could rightly be called shamanistic. Ariane Macdonald¹ has presented a substantial body of evidence to indicate the existence of an organised religion, *gTsug* or *gTsug-lag* (the term is usually translated as 'science'), the memory of which was all but obliterated by the later Buddhists' contemptuous and deliberate

¹ 'Une lecture des Pelliot tibétain 1286, 1287, 1047 et 1290: essai sur la formation et l'emploi des mythes politiques dans la religion royale de Sron-bcan sgam-po', in *Etudes Tibétaines Dédiées à la Mémoire de Marcell Lalou*, Paris 1971.

association of it in their writings with the indigenous shamanic tradition. Her misunderstanding of the subject results in certain inconsistencies and errors. She takes for granted the validity of *gter-ma* (rediscovered texts) without entertaining the prosaic possibility that their discoverers (*gter-ston*) were also their authors and concealers. I raise this issue not to imply that Olschak is being credulous, but because the *gter-ma* tradition in both Tibet and Bhutan was a prominent feature of the reformed Bön-po who were keen to create for themselves a more 'respectable' historical tradition. No doubt the author would have suspended her belief in the texts' authenticity in this case, but the Bön-po are not mentioned at all in this context. Her unfamiliarity with the work on the pre-Buddhist religion is further illustrated by one significant mistake. On page 50 there is the frequent appearance of the term '*gTsug-lHa-khang*' which is taken to be synonymous with *lHa-khang* (temple, or literally 'god-house'). A *lHa-khang* is a Buddhist (*chos-pa*) temple, but the term '*gTsug-lHa-khang*' is almost certainly a mistake for *gTsug-lag khang* (the pronunciation of *lha* and *lag* being very similar), a temple belonging to the old royal religion which she would classify as Bön.

Although the book is ostensibly a history of early Bhutan, it lacks the disciplined approach necessary to give it unity and direction. The author condenses into little more than a page a summary of the major figures and events from the seventh to the twentieth centuries as if to provide in advance a framework upon which the rest of the account can be seen to assume coherence. But this approach is clearly inadequate, as may be seen, for example, from the section on 'The Hidden Treasures of Bumthang', which is a summary of about a dozen disparate legends without analysis or any comment on how they might interrelate to give a fuller picture of Bhutan.

In spite of these weaknesses, *Ancient Bhutan* is indeed a valuable addition to the literature on that country, and although, when it appeared, it was certainly the best English-language work on the subject, it is perhaps unfair to assess it as a historical document. To present historical data with scholarly analysis is not its aim. The photographic facsimiles of two texts, one of which is the Sindhu Rāja's biography, must have contributed greatly to the cost of the book, and are not really necessary from an academic point of view. The same may be said for the reproduction in Tibetan script of lists concerning the 'Brug-pa monasteries and temples ('because it might be of interest to scholars and students'). The majority of these lists are written in the cursive script, which is particularly trying to read even for those with some competence in literary Tibetan. The texts and lists, which together occupy over forty pages, could certainly have been presented more informatively - and economically - in transliteration.

The paucity of analytical material in Olschak's *Ancient Bhutan* is complemented by a full and usually perceptive description of the temples, religious works of art, legends and other diverse features of Bhutanese history. Finally, the wealth of photographs, maps, and Augusto Gansser's fine illustrations (including some unique photographs of pre-Buddhist wooden carvings), in addition to the above-mentioned facsimiles, help to characterise the book as one which is more valuable for aesthetic rather than academic reasons.

It is these characteristics which are likely to appeal to anyone with a casual or even romantic interest in Bhutan, while those with greater patience and willingness to inform themselves of the country's early history will prefer to negotiate the Byzantine complexities of literary research as documented in Michael Aris's book. But first let us consider one which deals with a different region of Tibetan culture and approaches its subject from the standpoint of symbolic anthropology. Ortner's *Sherpas through Their Rituals* is one of the very few theoretically-oriented works produced by ethnographers of Tibetan communities, the remainder of the literature being either largely descriptive or concerned with other Himalayan peoples. The book comprises a description and analysis of a number of rituals and social institutions of the Sherpas, an ethnically and to a great extent culturally Tibetan people who emigrated from Eastern Tibet into Nepal in the sixteenth century. However, since this book has been selected as a recently-published representative of the theoretical side of Tibetan ethnographic research, I shall restrict my review of it to an evaluation of its methodology and a consideration of just how accurate a tidy model can be when the elements of which it is constituted are sometimes speculative and incomplete. To perform this adequately in such a brief review unfortunately necessitates omitting a fair discussion of the book's many strong points, but these have been accorded enough treatment elsewhere (e.g. Messerschmidt in *Reviews in Anthropology* VI, no.2, 1979).

First I shall consider what must be the principal flaw in her argument, and then give examples of specific features - one might even call them devices - which by themselves would seem insignificant but which are collectively quite insidious insofar as they conspire to support her thesis. I submit that there is a *petitio principii* at the centre of a significant part of the argument, and shall demonstrate this principally through an examination of her conclusions.

The author contends that rituals provide the Sherpas with a means of coming to terms with the harsh realities of their social system and are to some extent 'primary arenas for symbolically confronting that [i.e. orthodox Buddhist] ideology, and rendering it more compatible with lay life'. These rituals are not formulated specifically for this purpose but are already present in the religion ostensibly as 'the primary conduits of high ideology'.

In spite of the many divergences in Tibetan Mahayana Buddhism from the earlier Theravada tradition, the former, as practised by the Sherpas 'nonetheless retains the central Buddhist tendency to isolate and atomize the individual, and devalue social bonding and social reciprocity'. Why this feature of Buddhism should be present in the Sherpa religion when it is, as Ortner says, not obligatory (citing the case of Thailand in evidence - a spurious argument which I return to below), is explained partly on historical grounds as having been more appropriate to the Tibetan society in which it originated; but the main answer 'lies in those aspects of secular life that both express and produce these tendencies toward atomization and individualism'. In other words, to use Geertz's terminology, the religion becomes a 'model' of society.

To summarise the position: rituals enable people to cope with adverse social conditions; the rituals are provided by the religion, the alienating quality of which derives from the atomism of the social structure. I shall not contest her evidence for the latter proposition, but if she is to prove that the religion is indeed modelled on the society, she cannot of course take for granted that Sherpa Buddhism is fundamentally separative if this is one of the implications of her conclusion. Independent proof is required here, and this, alas, is sadly inadequate.

Ortner contrasts Thai Theravāda Buddhism with Sherpa Buddhism, and attempts to show that the former has developed 'in contrast to the Sherpas, precisely such a social, communal Buddhism', which is indeed 'ironic', as she says, Mahāyāna Buddhism being 'supposedly a more socially concerned, "compassionate" form of the religion'. She then attempts to demonstrate the relative proximity of Thai villagers and Sherpas to their respective monastic communities. In assessing her data for the Sherpas I shall show by juxtaposition of parallel statements from Fürer-Haimendorf's *The Sherpas of Nepal* (London 1964), that her observations, while not necessarily contradictory to his, show a startling degree of unwarranted emphasis. While it would be quite possible to refute her case by using Tibetan material, I shall use other data concerning the Sherpas as far as possible. Readers of her book will notice that she is inconsistent in her use of orthodox Tibetan and heterodox Sherpa explanations of events to corroborate different stages of her analysis.

Ortner: They [Sherpa monks] draw their recruits from many villages and have no established ties with any single one. (p.158)

F-H: ... Analysis ... shows the close link between the monastery and villages in the vicinity. With the exception of the abbot's Tibetan step-brothers not one of the monks hailed from any village outside Khumbu, and the vast majority came from the three villages of Namche, Khumjung and Kunde. (p.136)

Ortner: Lay people and monks do not systematically interact. Monks are invited to the village only for funerals, or for special meritorious activities.... (p.158)

F-H: Monks... do not permanently live in an ivory tower. Many are the occasions when they are called to the one or other village... [and he lists a series of possible reasons]. On all these occasions they are in close contact with the lay villagers, and they freely visit their families and kinsmen, staying for some days in their houses. (p.133)

Ortner: Lay people usually go to the monasteries only for major festivals. They may also go ... to give donations and thus earn merit, but this again is relatively rare.... (p.158)

F-H: Lay folk from all villages of Khumbu as well as many parts of Pharak and Solu occasionally visit the *gomba* [i.e. *dgon-pa*, monastery]

In this manner there is a continuous two-way traffic between the monastery and the lay world.... (p.137)

Ortner: Few monks take vows, but they are expected to stay a life-time [as monks]. (p.158)

F-H: The large number of *thawa* [*grwa-pa*, monks] who left the monastery leads us to the conclusion that the state of monk is not considered as a man's final choice, but that in many cases it is but a phase in career, comparable almost to some years spent in an institution of higher education. (p.137)

Other examples could be cited, but the above, it is hoped, is sufficient to make the point. I stress that I am not merely favouring Fürer-Haimendorf's perceptiveness above Ortner's, since a close consideration of the above and other comparable statements will show a greater consensus than is at first apparent. What is to be deplored is the undue emphasis placed by Ortner on those features of Sherpa life which, if as prominent as she would have us believe, would validate her argument. The excerpts from Fürer-Haimendorf show these same features in what is probably their true proportions since the latter is not concerned to force the data to comply with any particular model.

Ortner seems to construct her case on a foundation of circumstantial evidence and cements her examples together with a range of semantic devices which, individually, escape the reader's attention, but which in aggregate are too thin to cohere. I give two apparently minor examples. On page 110 she makes an opposition between 'envying the rich' and 'pitying the poor'. This is surely an inappropriate pairing, envy being a grudging sentiment and pity sympathetic. A more apt contrast would be either deferring to or respecting the rich, which is sympathetic, or despising the poor, the opposite of envying them. Secondly, in attempting to demonstrate the multiple interpretations of a particular exorcism ritual, she first gives a 'psychological', more orthodox, interpretation, in which a ritual object, a metal Buddha, is seen to represent the immortal spirit and is described in passing as being made of 'indestructable' metal. A few pages later, she interprets this ritual in social terms, and the rich are represented by the 'expensive Buddha idol'. I am not proposing that symbols can have only one interpretation, but these and other statements often read like a parody on Turner's theory of symbolic polysemy, and even Messerschmidt, in his otherwise adulatory review of this book admits that some readers 'may classify it as a clever endeavour of words'.

Ortner's alternation between monastic and folk interpretations has been mentioned earlier, but she is unable to construct a plausible pattern for the interaction of orthodox and heterodox rituals (this duality itself being an oversimplification) as successfully as does Tambiah in a South-East Asian setting

(*Buddhism and the Spirit Cults in North-east Thailand*, Cambridge 1970) - largely because she apparently does not read Tibetan. For example, using such a system of transliteration 'as to evoke from the English-speaking reader the sounds as I heard them', she gives as the term for charitable acts 'gyewa-zhinba' and for funerals the term 'gyowa'. This has caused her to understate the traditional stress on funerals as merit-making occasions, for, as Snellgrove has pointed out² in his similar criticism of Fürer-Haimendorf (op.cit.), both 'gyewa' and 'gyowa' are the same term, *dge-ba*, meaning virtue or merit.

One of Ortner's main contentions is that the rituals themselves are mechanisms for 'desocialising and existentialising actors' experience of the problem'. This conclusion is arrived at largely by ignoring the position of the lama (the reincarnate lama, *sprul-sku*, as opposed to the lay village lama) in Tibetan religion. While Ortner overstates the extent of interaction between Thai monks and laymen, since rituals such as healing, exorcism and divination - all popular concerns - are performed by lay specialists, the monks having no part in them, these are areas which in Tibet are all within the province of the lamas. Certainly there are lay ritual specialists in Tibet, but they have no monopoly on the performance of their rituals. Whatever they can do the lama can do better. Geoffrey Samuel has shown in his Ph.D. dissertation, *The Crystal Rosary*, (Cambridge University 1975), that while in strict orthodox terms tantric rituals can be performed by the celebrant alone, the ordinary laity depend upon the lama to make these performances efficacious for them through a bestowal of his power (*byin-rlabs*, blessing). The evidence seems to suggest that the lamas are just as significant to the Sherpas as to other Tibetans. We might also note that whereas in Ortner's description of the ritual called Nyungne (*sMyung-nas*) the emphasis is on the independence of the celebrants from the religious officiants, Fürer-Haimendorf observes that in the example which he witnessed, a hermit who was a 'greatly revered personality' presided over the proceedings and ended them with a rite called 'the *Tshe-wong* [*Tshe-dbang*], the rite of "Life Consecration"', one which Samuel cites specifically as requiring the lama's mediation to be effective.

A book which could hardly be further removed from the above in its objectives and methods is *Bhutan: The Early History of a Himalayan Kingdom*. The author, Michael Aris, until recently a research fellow of St. John's College and now of Wolfson College, Oxford, spent five years from 1967 in Bhutan as private tutor to the Royal Family and as head of the Translation Department. His own objectives and sympathies are made quite clear at the outset, where he explains his reasons for not succumbing to the temptation of writing an account of his experiences at the Bhutanese court, maintaining that he was instead 'inclined towards a more objective approach to the country by examining its past, one in which subjective impressions could be kept under control and used to profit in interpreting the written word'.

² 'For a Sociology of Tibetan-Speaking Peoples', *Central Asiatic Journal* XI, no.3, 1966.

The picture of early Bhutan that emerges from this book is very different from that drawn by Olschak, for Aris demonstrates that none of the issues is simple or straightforward, and any conclusions that he draws are painstakingly wrested from a tangled web of myth, legend and embellished history. The first thing that strikes the reader is the tremendous advantage the author has in his considerable knowledge of both literary Tibetan and the Bhutanese vernacular. The bibliography includes some seventy Bhutanese and Tibetan primary sources, and we might take as a generalisation his comment, which actually concerns the 'retreat' of the first *Zhabs-drung*, that 'to make any sense of the whole business we have to rely on literary detection'.

The book is divided into five - in fact, as we shall see, four - parts, following a useful introduction in which the author includes an indication of the scope of his study. A significant proportion of the difficulty in producing such a work is clearly the task of distinguishing myth from fact, the discrepancy between which is a theme that recurs throughout the book. However, lest the author be accused of ethnocentrism in attempting what may be regarded as an artificial separation of myth and fact, it must be emphasised that he in no way underrates myth for its own sake but attempts to combine what he regards as the two possible approaches available to a student of the subject:

He could by dint of hard labour search for historical fact embedded in the legends which constitute the myth or he could study the myths themselves to appreciate the psychological attitudes of the society for which the myth acts as a statement of truth.

Instances of such a simultaneous consideration of a people's external environment and their subjective response to it - reflected in a diachronic study of literature as history and myth - are unfortunately little in evidence in the anthropological material on Tibet.

Among the subjects included in Part I, covering the seventh to the ninth centuries and constituting about half the book, is the enigma of the 'Sindhu Rāja', whom Olschak takes for granted as 'King Sindhu who came from India', a view which, to be fair to her, is accepted by both Das (*The Dragon Country*, Delhi 1974) and Mehra (op.cit.) while Rahul (*Modern Bhutan*, Delhi 1971) does not mention him at all. Aris too makes use of his biography (*rGyal-po sindha ra-dsa'i rnam-thar*), but on the strength of earlier textual material concludes that the word 'Sindhu' is a much later form of the original, Se-'dar-kha, the name of a local ruler native to Bhutan; the homonymy with 'Sindhu' as the ancient form of 'Hindu' is probably fortuitous.

A significant consideration in the concluding chapter of the first part (which incidentally does not appear in the table of contents) concerns the potential for other disciplines such as archaeology (some evidence for an early lithic culture is given in the introduction), linguistics and social anthropology, suggesting that the latter's methods may be particularly useful in the study of oral tradition and social change.

It may be hoped that these disciplines will one day produce solid grist to the historian's mill which has here revolved almost exclusively around the fragile husk of the texts available to date.

The second part of the book is an account of the Tibetan Buddhist sects (among which the Bön-po are rightly included) and subsects and their fortunes in Bhutan before and after the establishment of a church state under the 'Brug-pa. Aris does not allow himself to be drawn into a discussion of doctrinal development and differences which would be a digression and, if justice were to be done to the subject, one far too extensive to fall within the scope of the book. His expressed concern is however 'simply to establish something of the human record of these schools', although he acknowledges that the necessary separation of the mundane and the spiritual is 'an invidious task' which tends to produce a distorted picture of the culture, but is necessary if we are to penetrate beneath 'the shared world of Tradition where one century looks like any other and where human motivation is always simple as in a fairy story'.

Part Three, covering the seventeenth and eighteenth centuries, deals with the foundation of the 'Brug-pa theocracy and reveals what Aris calls 'an old skeleton in the Bhutanese cupboard', a metaphor which has surely never been more aptly used. The first chapter outlines the life and achievements of the *Zhabs-drung*, the founder of the church state, but the remainder investigates his mysterious 'retreat' which, according to most accounts, lasted from about 1651, when he was over fifty years old, into the early years of the following century. Nearly all modern writers, including Olschak (who devotes a chapter to the *Zhabs-drung*), omit to mention the matter, while Das states that 'Umze concealed the death of Nawang Namgyal for five years'. Aris's conclusion, on the strength of literary investigation, is that 'The death of *Zhabs-drung* Ngag-dbang rNam-rgyal was apparently kept a state secret for more than half a century' - in order to permit the consolidation of theocratic rule in the absence of an undisputed successor, among other reasons. He also reveals how the practice of concealing a ruler's death was comparatively commonplace in Tibet and Bhutan. One example is that of the Fifth Dalai Lama, whose retreat of fifteen years coincided with that of the *Zhabs-drung*, giving rise to 'the odd situation that during these years the Tibetan and Bhutanese states were both ruled by corpses, in a manner of speaking'.

In the fourth part, which is by no means a detailed account of the period after the *Zhabs-drung*, Aris considers some general issues such as that of succession in relation to the problem of 'multiple reincarnation' and the varying fortunes of British influence in the country.

The presentation of the last part emphasises the author's avoidance of a 'cosmetic' treatment of his sources. It comprises four texts from different periods, three in Tibetan and one being the report of the Portuguese Jesuit Cacella's visit to Bhutan in 1627, where he met the *Zhabs-drung*. The Tibetan texts are edited and copiously annotated by Aris and presented with parallel translation. From one of them he reconstructs a genealogy (to be found

in Part I) of the six ruling clans of Eastern Bhutan, corroborated as far as possible by the oral tradition which is alive in that region as well as by other texts which mention certain of the members. Presumably acting on the theory that only a minority of readers will be interested in them, and to minimise costs, the texts are not printed in the book but are available separately on microfiche from the publishers.

I hope the above will have given some indication of the depth of scholarship and breadth of research revealed in this book, but mention should also be made of the author's lucid use of English, which contrasts well with the opacity of his subject. While he is not given to the effusiveness of other writers, who are apparently stultified by their encounter with Bhutanese royalty, his style is by no means arid but echoes his acknowledged admiration for the writing of the eighteenth-century visitors to Bhutan above those of the nineteenth, whose 'dull invective' he attributes mainly to a deterioration in Anglo-Bhutanese relations but also partly to 'a decline in English prose style'. This appreciation of differences in style and tenor is extended to the Tibetan sources, affording him a greater insight into their authors' background and intentions. Nowhere, however, is scholarly intuition given a completely free rein, being subject always to the restraining influence of intellectual precision and the essential trappings of literary or other evidence.

The methods of the historian and of the social anthropologist are not the same, but ethnographers of Tibetan communities would perhaps do well to follow Aris's example of balancing a people's historical record beside their affective response to it without permitting either dimension to assume undue proportions. Of course, it is not to be assumed that the anthropologist must perform an exhaustive study of the historical literature, but a reasonable grasp of the religion - if that is our particular concern - in its historical context as well as its relationship to current practice (and here the ethnographer comes into his own) is clearly necessary before we are equipped to hazard interpretations of ritual. In this context we should perhaps heed the warning offered by Goody in his *Literacy in Traditional Societies* (Cambridge 1968):

We cannot expect to find the same close fit between religion and society that sociologists often perceive in non-literate cultures when the reference point is not some locally derived myth subject to the homeo-static processes of the oral tradition but a virtually indestructible document belonging to one of the great world (i.e. literate) religions.

C.A.E. RAMBLE

TIBETAN STUDIES AND RESOURCES IN OXFORD

When the High Lama asked him whether Shangri-la was not unique in his experience, and if the Western world could not offer anything in the least like it, he answered with a smile: "Well, yes - to be quite frank, it reminds me very slightly of Oxford."

James Hilton, *Lost Horizon*

I do not propose to dwell on any imagined qualitative resemblance of Tibetan scholarship to that of Oxford although it is perhaps worth recalling that the collegiate structure and tutorial system of Oxford find close parallels in the great monastic universities of Lhasa which have so recently disappeared. Rather I intend briefly to discuss the valuable holdings in Oxford of material in Tibetan as assembled since the early 19th century. The collections are small by comparison with those of the British Library and the India Office Library but contain many items of singular interest. Together they bear witness to a continuing, if somewhat sporadic, devotion to the field of Tibetan Studies on the part of individual Oxford scholars. The collections also reflect the Bodleian's capacity to acquire material relating to important disciplines which are otherwise not represented in the University at large. The time is right for a summary appraisal because recent years have seen the dissemination of many thousands of Tibetan texts reprinted by refugees in India under a scheme administered by the US Library of Congress known as the PL480 Tibetan Comprehensive; before attempting to grasp the full dimensions of this extraordinary *embarras de richesses*, surely it is important to take stock of the older collections among which those of Oxford certainly deserve consideration.

To date the Bodleian Library possesses 188 Tibetan MSS and 329 Tibetan xylographs (designated 'blockbooks'). It has 248 primary sources in western editions and these are complemented by

a further 125 items of a similar nature under the same roof in the Indian Institute Library, which also possesses 330 works of secondary literature on Tibet. In addition to complete runs of the relevant journals, the two libraries between them have full sets of the *Serie Orientale Roma*, the *Asiatische Forschungen* and the *Sata-Pitaka Series*, all of which are important for Tibetan Studies. The Pitt Rivers Museum has four sets of Tibetan MSS, a token to the magpie nature of many army officers and civil servants in India who left their acquisitions to this 'museum of a museum'. Unfortunately most of the texts are in fragments but among them is a fine example of a Tibetan musical score, the *dByangs-yig tshangs-pa'i rol-mo* ('The Musical Notation [entitled] the Music of Brahma,' Beasley Collection 357). The library of the Oriental Institute, the Balfour Library and the Tylor Library also possess small collections of secondary literature, as do some of the college libraries. Although I shall not deal now with the collections of Tibetan art at the Ashmolean Museum (the Scratton Collection particularly), St. Antony's College (the Hailey Collection) and the Pitt Rivers Museum, the comprehensive holding of books on Tibetan art at the Library of Eastern Art in the Ashmolean should be mentioned.

The Bodleian's collections are by far the most significant. Although some work remains to be completed on their cataloguing, by and large their contents are known and accessible. This is due in great measure to the labours of Mr. John Driver, formerly of St. Antony's College, who prepared a description and report on the MSS and 'blockbooks' acquired up to c. 1970, thus furnishing material for any future descriptive catalogue. In recent years Mr. David Barrett of the Library has been engaged in preparing a complete card catalogue and this was finished in April 1980. It would have been difficult to undertake the present contribution without these materials to hand.

Turning to those items which have struck me as particularly interesting and significant, in the approximate order of their acquisition, in 1806 the Bodleian gained possession of what were probably the first documents in Tibetan to enter the country. These are found among the papers of Samuel Turner (1749-1802) who was sent by Warren Hastings in 1783 to the court of the Panchen Lama. His *An Account of an Embassy to the Court of the Teshoo Lama, in Tibet* (London 1800) is rightly regarded as the great classic of Tibetan travel literature, but the preservation of some of his papers in Oxford is known to few people outside the Bodleian. Among one set of 28 documents bound together (MS. Tibet.a.8.) is found the original of the letter from 'Jigs-med Seng-ge, 18th Deb Raja of Bhutan (regn. 1776-1788), which Turner reproduced on Plate XIII of his *Account* as an example of the Tibetan cursive hand. Included in this set are one further letter from the Deb Raja and three sent by the steward of the infant Panchen Lama. MS. Tibet. a.7 (R) consists of an incomplete history of Tibet written in cursive on a scroll of 189 lines. Lacking a title and colophon and with no indications as to date or authorship, the work was perhaps hurriedly copied for Turner

at Tashilhunpo in response to a request for historical information. It covers in brief the entire period from the 7th to the 14th centuries and opens with a typical cosmology. Although representing a late and developed tradition, the work has undoubted value. MS. Asiat. Misc. a.4 contains a further 49 items, mostly correspondence received by Turner up to four years after the date of his mission. Nearly all the documents were written by munshis in the employment of the Bhutanese government in a heavily Persianised medieval court Bengali and stamped with various seals in Tibetan, Mongolian, Persian and Bengali.

In 1809 the library acquired a true oddity, only recently identified as Tibetan. This is the *Inscrip.[tio] Calmucor.[um] Sanskr.[itica]* (E.D. Clarke Or. 41), in fact a Kalmuk prayer-flag consisting of parts of two sutras containing protective *dharaṇīs* written in Tibetan on red linen. It had been obtained in a Kalmuk camp on the north-east shore of the Sea of Azov by the Rev. Edmund Daniell Clarke (1769-1822), a great traveller, antiquary and mineralogist of his day. Unintelligible to the Kalmuks at that time, the elders of the tribe had faithfully copied it out and presented it to the eccentric gentleman with much ceremony, saying it contained the essence of their religion.

This strange event on the westernmost fringe of the Tibetan cultural empire points to the source of several of the Bodleian's most valuable documents, for it was in the old region of Western Tibet, and British-administered Ladakh in particular, that the modern discipline of Tibetology was really born. The founding figure is reckoned to have been the Hungarian scholar Kőrösi Csoma Sándor (1784-1842) who set off in 1819 on a romantic quest for the origins of the Magyar people. He ended up spending about eight years in the monasteries of Zangskar and neighbouring regions, during which period he prepared his *Essay Towards a Dictionary, Tibetan and English*, published in Calcutta in 1834. Perhaps the most poignant of the Bodleian's treasures is a MS volume containing the first draft of half of the Tibetan entries in his famous dictionary. The hand is very likely that of his collaborator, the 'Brug-pa lama Sangs-rgyas Phun-tshogs, who presumably gave this draft its Tibetan title of *Bod-skad-kyi ming-mdzod* (MS. Ind. Inst. Tib. 2.).

The next acquisition by the Indian Institute does no credit to the way it was procured by its donor. It is sufficient to give the title: *Manuscripts and impressions ... taken from the interior of an idol in the Buddhist temple at Namtchee in Independent Sikkim in Feb. 1861, on the march of Colonel Gawler's force into the country ...* (I. a4(8)). From the number of mantras and yantras in this collection it seems the desecrated image was probably large and highly venerated.

It was not until 1885 that the Bodleian showed a decisive interest in Tibetan with the purchase of the large and important Schlagintweit Collection for which Emil Schlagintweit produced a MS catalogue ten years later in 1895. The collection contains some 207 works in 118 items. Perhaps the single most important item is a history of Ladakh (the *rGyal-rabs gsal-ba'i me-long*, MS. Tibet. c.7) in pursuit of which Hermann Schlagintweit had

conducted subtle negotiations in 1865 with the ex-king of Ladakh, 'Jigs-med rNam-rgyal. Emil published his own edition of this crucial document in 1866, and it was incorporated much later by A.H. Francke into his critical edition of 1926. Many of the Schlagintweit texts are standard central Tibetan blockprints and it seems most of these were obtained through the offices of a famous political monk of that time, the Chibu Lama, who acted as the representative of Sikkim in Darjeeling. Included in the collection, however, are a set of 13 Persian MSS which the Schlagintweit brothers had acquired from one Haider Khan, the hereditary Moslem ruler of Shigar, the northernmost district of Baltistan (often called 'Little Tibet'). Among these are found histories of both Baltistan and Shigar itself; as far as I am aware, none of these documents have ever been properly studied.

For me the most pleasing memento to the travels of the Schlagintweits is found outside their collection, in the Indian Institute Library which possesses a set of linen (?) proofs (Fc 4(18)) of the illustrations for Emil's *Buddhism in Tibet, illustrated by literary documents and objects of religious worship* (Leipzig 1863). The last item in the proofs never found its way into the book. It shows a so-called 'Bhutia Map' of the commercial route from Assam to Lhasa by way of Tawang, complete with mythological beasts, snow mountains and temples. It was drawn for the brothers by the 'Kauang Rajah' at Narigum in 1856. It holds a special appeal for me as I followed the same route as far as Tawang with my family last winter.

Until this century Tibet remained virtually closed to foreign scholarship and it should cause no surprise that the Bodleian's acquisitions in the 19th century all came from the fringe of the Tibetan world. In 1904 the Younghusband Expedition forced its way to Lhasa to counter an imagined Russian threat from the north and Lt.-Colonel Austine Waddell accompanied the mission to collect works of Tibetan scholarship. His activities appear to have been sanctioned as a result of the recommendations of F.W. Thomas, Librarian of the India Office Library and much later Boden Professor of Sanskrit at Oxford, whose Tibetan interests always tended to outweigh his devotion to Sanskrit. At all events, Waddell procured a huge mass of original Tibetan material, presumably by purchase though this is not clear. The collection was somewhat arbitrarily, but munificently, divided and presented by the Government of India to the British Museum Library, the India Office Library, Cambridge University Library and the Bodleian Library, the latter receiving some 131 volumes. Included among these were 91 volumes of the Tibetan Buddhist canon, the Kanjur (*bKa'-'gyur*), representing an incomplete set of the Narthang edition prepared in 1732 during the reign of the VIIth Dalai Lama. Unfortunately several of the volumes were badly mutilated by rodents, and it is not clear whether these were of the Tibetan, Indian or English variety. It is to be hoped that the Library will eventually restore the set to its full complement. Meanwhile the Derge edition has just been purchased in an Indian reprint edition and this will perhaps be followed by the acquisition

of a full set of the Tenjur (*bsTan-'gyur*), the commentaries on the canon, from the original blocks at Derge. The Chinese authorities have recently announced the reopening of the huge printery there.

The Younghusband Expedition also produced valuable results in the form of collotype facsimile copies prepared by our own Clarendon Press of two letters addressed by the Chinese Amban in Lhasa to the Tibetan public at the time of the expedition, and a letter in reply from the Tibetan cabinet (MS. Tibet. c.24). They were donated by Sir William Herschel in 1908 and still await study.

Even after the Younghusband Expedition it was really only on the western periphery which lay beyond the reach of the Tibetan government that foreign scholars could pursue their interests. The German Moravian missionaries at Leh and other centres in the Western Himalayas were all prodigious scholars. An extremely rare product of their efforts can be seen in the Bodleian's copy of A.H. Francke's [*First and Second Collections of*] *Tibetan Historical Inscriptions on Rock and Stone from West Tibet* (Tib. d.37 (1-3)). These were mimeographed in Leh in 1906 and 1907 on a portable press which now lies under dust in the old mission library, surrounded by piles of those tracts in almost every Central Asian language which used to be sent from Leh along the old caravan routes that are now closed. Francke's pioneering collections of ancient inscriptions have never received the attention they deserve. This is partly due to the importance of the Tibetan discoveries of Sir Aurel Stein and Paul Pelliot at Tun-huang. It was at Merton College that Stein wrote his classic account of the discovery of the sealed polyglot library in his *Ruins of Desert Cathay* (London 1912). Few at that time realised the effect his discoveries would have on the rise and development of Tibetan studies. For the first time authentic documents dating from Tibet's dynastic period which ended in the 9th century became available to modern scholarship. Stein unfortunately had a very tenuous formal connection with Oxford. His great collections remained in London where F.W. Thomas and others soon began to glean the Tibetan treasures. Late in his career Thomas came to Balliol to occupy the Boden Chair of Sanskrit, as already noted, and here he continued to pursue his first love to the neglect, it seems, of his official bride. He is still remembered by many in Oxford today and by all students of Tibetan as the foremost British Tibetologist of the first half of this century. Some of his Tibetan texts were donated to the Bodleian and at his death the Oriental Institute Library acquired part of his personal library.

Meanwhile Dr. W.Y. Evans-Wentz, Fellow of Exeter College, was doing more than perhaps anyone to disseminate popular interest in Tibetan religion through his famous translations of Tibetan Buddhist texts, published and many times reprinted by Oxford University Press. (Indeed, the bible apart, his *Tibetan Book of the Dead* (1927) appears to be one of O.U.P.'s all-time best-sellers. It should also be recalled that the Press has also

published major works of Tibetan scholarship by Sir Charles Bell, Herbert Guenther, Hugh Richardson and David Snellgrove). Reading the works of Evans-Wentz today one is forced to admit that their virtues derive mainly from the contribution of his chief collaborator, the great Sikkimese scholar Dawa Samdrup, who assisted several foreigners who were themselves unable to cope with the Tibetan. Some of Dawa Samdrup's draft translations passed to the Bodleian with the death in 1964 of Evans-Wentz who bequeathed his papers to it. Among a total of 54 items are found MSS and xylographs of some value.

This briefest of surveys has concentrated more on the major collections than on the single acquisitions, and it necessarily reflects my own historical interests. Much more could be said about the liturgical, ritual and philosophical texts in Tibetan though I do not believe there are many of exceptional rarity. It should be said, however, that the Bodleian does possess the first and most beautifully illuminated volume of the collection of *rNying-ma-pa* tantras known as the *rNying-ma rgyud-'bum*, purchased from Sotheby's in 1909 (MS. Tibet. a.24(R)). Most of the remaining volumes of this unique MS edition appear to be preserved at the India Office Library.

In the event of Tibetan being introduced as an undergraduate option in the Honours School of Oriental Studies, the student will certainly find himself well served by the resources of Oxford. The graduate students of Oriental Philology, Buddhist Studies and Social Anthropology who have been attending my classes in literary Tibetan at the Oriental Institute for the last four years find the local libraries ample to most of their needs. Last summer about seventy scholars from many countries gathered at St. John's College for the International Seminar on Tibetan Studies. (The proceedings will be published in 1980 under the title of *Tibetan Studies in Honour of Hugh Richardson*.) In their case, however, the attraction of Oxford lay perhaps not so much in the materials for their subject assembled here as in the sentiment expressed in the quotation with which I began. Several of my foreign colleagues even went so far as to endorse it wholeheartedly.

MICHAEL ARIS

[*This paper was delivered at the annual conference of the SCONUL Group of Orientalist Libraries held at Wadham College, Oxford, on 11 January 1980.*]

Postscript Since the above was written my attention has been brought to the facsimile of a Tibetan passport issued to an Armenian merchant dated Earth Dragon (1688?) reproduced in Thomas Hyde, *Historia Religionis Veterum Persarum*, ..., facing p. 521 (Oxford 1700). The original document is probably still in Oxford.

COMMENTARY

TEN YEARS OF JASO:

1970 - 1980

When one compares the appearance of early issues of *JASO* with the present handsome format it is a little hard to recall why it remained so long in its old stencilled form. The reason was, I think, not unrelated to its main achievement, that of actually surviving, issue after issue, for ten years (despite the decadal symmetry of the figures 1970-1980, this is of course the *eleventh* year of publication). It should not be forgotten that this is a journal edited by graduate students, and that the last ten years have covered the complete period of transition from the enthusiastic attitudes of the 'sixties' to that period of relative sobriety and constraint in which we live today. The old format had the advantage that it could be produced with the minimum of assistance and instruction, and was adapted to the very varied levels of business acumen shown by the editors, and the wild fluctuations in student support. At the very worst the whole thing could be produced by the editors and one or two *ad hoc* assistants. The typing was usually done by a standing arrangement with a secretarial agency at a relatively low price, related to the fact that sometimes the typists were learners themselves. The heaps of sheets were collated by the time-honoured process of walking round tables on which the pages were laid out in a circuitous route. In the first years this took place in Paul Heelas' house in Cardigan Street in the Jericho district of Oxford, where the night-time tramp of feet added to the 'freaky' reputation of that well-known address!

There were for long no departmental facilities for this enterprise, and only in the latter part of the period has there even been a room to work from. Now there is a small office, and headed paper, rubber-stamps, an arm-chair, and a general air of tidiness. But usually the journal has been the beneficiary of the characteristic Oxford tolerance - individual enterprise, no funds, with no-one too seriously opposing it, and generally ignored.

That is the background to those tightly-rolled scrolls that travelled very soon to 250 and more addresses. The opening of the issue without tearing off several pages was a skill in itself. Holding the issue flat for long enough to read, before the wrists weakened and the thing sprang back into a complex paper sculpture, was another. The pierced thumbs from the ill-flattened staples

were due to the difficulty of borrowing for long enough a big enough stapler to fasten the often enthusiastically fat issues. A subscriber has said that until he met JASO he had no convincing response to the MacLuhan cliché 'The medium is the message'. No message could possibly embody that medium.

Economies of time, effort and money in the production were imposed on the Journal if it was ever to appear more than once or twice, and was to meet its termly deadline. The temptation to give issues away for nothing, and to send them 'for sale' (often these ended as free gifts) at other Universities, led to the Editorial Adviser being owed £80 after a year. Malcolm Crick by an almost monastic regime wiped out this deficit. Small grants were received from various sources, including the Radcliffe-Brown Memorial Fund, and the Wenner-Gren Foundation. From 1971 to the present the Journal has received no further subvention and has even made a profit sufficient for the present improvements to be attempted.

The very name of the Journal expresses a strategy to acquire funds. In order to qualify for any assistance from the University's Clubs' Office (some typing and other facilities were optimistically hoped for) the Journal had to be under the auspices of a recognized University club. The long-standing Oxford University Anthropological Society was asked to lend its name for this purpose - but the Journal itself was given the name of a non-existent institution - the Anthropological Society of Oxford - to preserve its independence. In fact, JASO very quickly exhausted the meagre resources and patience of the Clubs' Office, occupying the typing facilities during the Crick economy period to such a degree that it was no longer a practical proposition. Vol.I no.3 (1970) and Vol.II no.1 (1971) show signs of all the problems of the time. During the winter power-strike the stencils were typed by candle-light, which meant practically blind, and corrections were particularly difficult. There are tales of female tears splashing on the text. The flickering type-face remains as a record, beneath the confident tone of the articles.

The necessity for an Editorial Adviser arose from the Proctors' regulations of the University, which required a 'senior sponsor' and I suppose that is still the case, despite JASO's present relative self-sufficiency. For practical purposes I have been treasurer of the Journal, monitoring its financial transactions, and despite occasional head-aches (as when most of an uncollated issue was thrown out from a College office in mistake for rubbish! - the College paid for a reprint), that side has not for long presented a problem. The Editors have always selected or rejected papers, and copy-edited them totally independently. It is quite usual for me to read the contents for the first time only on publication day.

There have been similar journals during the last ten years from anthropological departments in other Universities, but most seem to manage one issue and then go into decline. Too much concentration on smart production makes it difficult to meet deadlines. The difficulty of maintaining a succession of editors and reliable helpers is another obstacle. Certainly the editors of

JASO have usually had something to say and some kind of dedication even in hard times, of which there have been a few.

The Journal was always encouraged by Evans-Pritchard, who was still Professor for its first two issues. He contributed frequently until his death. There were articles by him on Ferguson, Condorcet, Lord Kames, John Millar, and Montesquieu. He allowed JASO to re-print in its first form his classic paper 'The Intellectualist (English) Interpretation of Magic', originally published in Cairo. His article on 'Social Anthropology at Oxford' (I:3) is still of great interest. In the year of his death he contributed 'Some Reminiscences and Reflections on Fieldwork' (IV:1, 1973). On the question of entering into the thought of other people, he wrote:

I wonder whether anthropologists always realize that in the course of their field work they can be, and sometimes are, transformed by the people they are making a study of, that in a subtle kind of way and possibly unknown to themselves they have what used to be called 'gone native'. If an anthropologist is a sensitive person it could hardly be otherwise. This is a highly personal matter and I will only say that I learnt from African 'primitives' much more than they learnt from me, much that I was never taught at school, something more of courage, endurance, patience, resignation and forbearance that I had no great understanding of before. (IV:1, p.5)

He concluded, on the subject of books:

A certain degree of abstraction is of course required, otherwise we would get nowhere, but is it really necessary to just make books out of human beings? I find the usual account of field research so boring as often to be unreadable - kinship systems, political systems, ritual systems, every sort of system, structure and function, but little flesh and blood. One seldom gets the impression that the anthropologist felt at one with the people about whom he writes. If this is romanticism and sentimentality I accept these terms. (*Ibid.*, p.12)

Other contributions of historical interest throw some light on early anthropological figures. Phyllis Kaberry (who died tragically in 1977) supplied a lively reminiscence of Malinowski:

The atmosphere of Malinowski's seminars was exhilarating, but to begin with overpowering for diffident postgraduates, and most of us were that. The first few weeks were agonizing because, inexorably, would come the question 'What do you think of that Miss K?'. Paralyzed I would utter something barely audible and articulate, and then would be asked 'to develop' what was in many cases a non-existent point. However, after the initial stages of 'arrested development' we did venture on criticism and the occasional frivolous remark. (V:2, 1974, p.105)

Soon afterwards she 'fell from grace' after refusing the honour of becoming Malinowski's research assistant. Later while writing *Aboriginal Women* she wished to dedicate the volume to him, stating, however, that she disagreed with his theory of derived needs and the nature of 'institutions'. Malinowski's letters to her on the subject in 1939 and 1940 were good-humoured and magnanimous and are printed in her article. There is a sentence in one of them in which certain names were replaced by letters:

I would be very glad if the criticism came from you in a free and courteous way rather than from some of my pet aversions in the anthropological world, a X, a Y, or some other Boasinine peep-squeak [sic]. (*Ibid.*, p.107)

'X' was in fact 'E.P.' in the original. 'Y' was an American, possibly Margaret Mead.

Later *JASO* published two letters from Radcliffe-Brown to Evans-Pritchard supplied by Godfrey Lienhardt, and dated about 1950. Radcliffe-Brown was rather testily defending his view of what a 'law' was in social anthropology:

I have written two criticisms of your BBC lectures. I do hope you will not find them too severe. I think a little severity might be called for but I prefer to leave that to the *Economist*. So I have been as tender as I can in all honesty and sincerity. I feel you have recently been somewhat led astray, and scientific methodology is something you are not very strong on. (VIII:1, 1977, p.50)

There have been other contributions over the years by senior academics like John Beattie ('Has Social Anthropology a Future?' I:3), Rosemary Firth ('Anthropology within and without the Ivory Towers', II:2), Lucy Mair ('Recent Writings on Witchcraft', III:1), David Pocock ('Nuer Religion: a Supplementary View', V:2), and others. At all times however, the bulk of the contributions have come from within Oxford and mainly from within the group of graduate students.

It was Brian Street (now at Sussex and author of an important book on the treatment of the primitive in British fiction) and Paul Heelas (now at Lancaster) who started the Journal. There were complicated reasons why the need was felt for it, and memories gradually fail. My own recollection is that Oxford students felt that the Institute had not received full recognition of its recent intellectual contribution. In those days, it is hard to recall, structuralism and other newer waves were relatively unfamiliar in Britain outside one or two centres. One must tread carefully here as no two Oxford persons would agree on what the Oxford contribution was then or is now. But, whatever may be said about that, *JASO* embarked on a course of continual debate and discussion that was felt to be essentially modern at the time.

The first issues of the Journal opened up topics that still are unresolved. The very first article in 1970 was entitled 'Meaning for Whom?'. At its birth *JASO* expounded such topics in

a critical manner. In the first year all the names that now seem a roll-call of the 'trend' appeared: Winch, MacIntyre, Lévi-Strauss, the British structuralists, Althusser and the structuralist-Marxists, Castaneda, the linguistic philosophers, Lacan, Derrida. *JASO* was on to the 'Modern Masters' almost as if it had supplied the names for Fontana in advance. When looking back on the early issues I expected to find an embarrassingly uncritical treatment. But, as I have already hinted, there was born with *JASO* a note of dissatisfaction with heroes. It is true that the Journal discussed people and topics that two or three years later became, in other places, merely trendy. But usually by then *JASO* had already assimilated the message and had moved on. The Journal certainly started with its own set of 'goodies' and 'baddies', but it soon moved beyond such simple football-team values.

Jairus Banaji, then a student at the Institute, contributed two articles in the first year, as well as writing a critique of British Social Anthropology from a Marxist point of view - the latter while still in his Diploma year. It is now a standard work of Marxist social anthropology - part of the 'New Left Critique'. It might just as well have been named the '*JASO* critique'. Its response to an earlier article by Goddard was to show that British Marxists did not then even know what social anthropology was about. The Banaji article carries an internal critique of its own critique which has outlasted the fashion for the structuralist-Marxism it helped to popularize. By the end of the decade the Marxist approach in social anthropology had been discussed as exhaustively as I have seen in a Journal not explicitly of the tendency. Can it be wondered that it has been viewed with puzzlement and concern - plundered but rarely cited on this as on many other topics. Jenkins (VI:1, VIII:3), Dresch (VI:3, VII:2) and Ovesen (IX:1) are some of the other contributors to this stream of the *JASO* discussions, whose views have sometimes caused pain in quite sophisticated Parisian circles. I single out the critique of Marxism because many local observers of *JASO* became impatient with it. Yet it was *JASO*'s own, in a way that some of its other interests were not entirely. I single it out also because there are several stereotypes of *JASO* as 'idealist', 'semiological' or the like. Occasionally for an issue or two a particular needle may have become stuck in a groove. Yet a perusal of the texts shows the continuous emergence of new topics and themes. It is, however, a feature of journals like *JASO* (and like small literary magazines) often to be collected rather than really closely read.

Needless to say, everything connected with women's studies took off quickly. *JASO* was never the primary source at Oxford for these developments, but it was a useful model for self-help, and it must be seen in retrospect as no coincidence that the local style of women's studies has had the same kind of intellectual stamina.

The continuous examination of language and meaning echoed wider Oxford concerns. I do not intend to go into detail in this often misunderstood area, but over the period the move was in the direction of less and less trust in formalistic approaches and increasingly towards more simple expositions. *JASO* began, as we

saw earlier, by supporting a handful of famous names, the 'modern masters'. Ten years later it is as if these names have been consumed. Two excellent books were produced by *JASO* editors on the theme of meaning and identity which express some of this process: Malcolm Crick's *Explorations in Language and Meaning* (London 1976) and Malcolm Chapman's *The Gaelic Vision in Scottish Culture* (London 1978). The field-work insights of *JASO* contributors have been quite considerable despite the early editorial note that articles should concentrate on analysis rather than description (already questioned in the second year by Tonkin - II:3). The contributors even came by an Einsteinian paradox to see that their search for a semantic anthropology was remarkably like that search for the noble savage, or the pure Celt, or the feminine, which in other phases of their research they had identified as chimeras (Chapman VIII:2). (Thus often does a theoretical approach raise the question of whether it is itself accounted for by its own theory. The point at which such a question arises usually represents a new point of advance.)

Maryon McDonald threw her own light on some contradictions ('Language "At Home" to Educated Radicalism', IX:1), which reminds us that there has always been a little leaven of wit in the Journal. Her translation of the satirical article 'GERTRUDE: A Postface to a Few Prefaces' (IX:2), with its parodies of the great French masters' approaches, as applied to the (in themselves hilarious) published acknowledgements of ethnographers to their wives, is a classic. McCall's humorous analysis of the supposed quadripartite structure of English society (I:3) was reprinted later in a national weekly. Joan Leopold contributed ('Tylor's Solar Sixpence', IV:1) a parody of Max Müller from the Tylor archives. But *JASO* has never had or encouraged cartoons or student jokes, or anything savouring of the house-magazine. (Just to prove me wrong, there is an excellent cartoon in the present issue!)

What of the readership? What indeed? Perhaps this article will encourage any long-standing overseas contributors to write in and say what they have got out of *JASO* for ten years. The whole issue is virtually sold on subscriptions, and a large increase came early on from a notice in *Current Anthropology*. It has stimulated other enterprises in other departments - *Semesterskrift* (Copenhagen) and *Journal of the Anthropological Study of Human Movement* (New York), among others. It is now taken by 125 University Libraries all over the world, about 40 being in the U.S. and 21 in the United Kingdom (one in the State Lenin Library of the USSR at Moscow). The subscription list, both corporate and private, could probably have been doubled at any time in the last few years, but the tiny production structure in the past could not have coped with twice as many issues. After all there are plenty of professional journals. We know for a fact that xeroxing *JASO* articles is one of the smaller pleasures resulting from scarcity (the sale of back-numbers has, however, always been a steady source of income).

The Journal has been singled out for mention here and there. Professor Victor Turner on symbolism in the *Annual Review of Anthropology* referred to 'the excellent *JASO*'. Mr. Keith Thomas

JASO EDITORS from 1970 to 1980

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| 1970 | Vol. I, Nos. 1 & 2 | Paul Heelas and Brian V. Street |
| | " " No. 3 | Paul Heelas and Malcolm Crick |
| 1971 | Vol. II, Nos. 1, 2 & 3 | Paul Heelas and Malcolm Crick |
| 1972 | Vol. III, Nos. 1, 2 & 3 | Paul Heelas and Malcolm Crick |
| 1973 | Vol. IV, No. 1 | Paul Heelas and Malcolm Crick |
| | " " No. 2 | Paul Heelas, Malcolm Crick and Martin Cantor |
| | " " No. 3 | Paul Heelas and Martin Cantor |
| 1974 | Vol. V, Nos. 1 & 2 | Paul Heelas and Martin Cantor |
| | " " No. 3 | Martin Cantor, Tim Jenkins and John Ryle |
| 1975 | Vol. VI, Nos. 1 & 2 | Martin Cantor, Tim Jenkins and John Ryle |
| | " " No. 3 | John Ryle and Tim Jenkins |
| 1976 | Vol. VII, Nos. 1 & 2 | Tim Jenkins and Malcolm Chapman |
| | " " No. 3 | Malcolm Chapman, Roger Rouse and Diana Martin |
| 1977 | Vol. VIII, Nos. 1 & 2 | Malcolm Chapman, Roger Rouse and Diana Martin |
| | " " No. 3 | Malcolm Chapman, Roger Rouse and Maryon McDonald |
| 1978 | Vol. IX, Nos. 1 & 2 | Malcolm Chapman, Roger Rouse, Maryon McDonald and David Scobey |
| | " " No. 3 | Roger Rouse and David Scobey |
| 1979 | Vol. X, Nos. 1 & 2 | Roger Rouse and David Scobey |
| | " " No. 3 | Jonathan Webber and Elizabeth Munday |
| 1980 | Vol. XI, Nos. 1 & 2 | Jonathan Webber, Patricia Holden and Steven Seidenberg |

noted that 'even the avant-garde' *JASO* had absolved him of the charge of Frazerianism (Randal Keynes' review of Thomas's book, III:3, was an excellent contribution). Elsewhere the bibliographies and sometimes the arguments of *JASO* articles have evidently been of use to other writers in the occasionally recondite areas covered by its contributors. On the other hand the contributors have also avidly gathered all that they could, and perhaps not all that they might. The charge can well be made that *JASO* is untypical of social anthropology at Oxford. It would certainly be misleading if any other impression were widespread, for the title should make it clear that the Journal is independent. *JASO* represents only a fraction of the work and interests of Oxford graduate students, the great majority of whom have never contributed to it. There have been times when it has been difficult to get an issue together. And yet it has succeeded in not always being disowned in its own land. Its occasional deficiencies are not due to any overridingly rigid editorial policy but largely reflect the way the papers come in. They are not (perhaps this should be made clear) really the proceedings of the fictive 'Anthropological Society of Oxford', nor (despite the onomastic connexion, and the good relations now subsisting with it) even of the real 'Oxford University Anthropological Society'. There is no guaranteed supply of contributions. It is perhaps credit enough that a pigeon-hole (shades of Lady Bracknell's '*a handbag!*') should have for so long been the incubator from which so far-travelled an object should have so regularly issued (and good money to be paid for it as well!).

At various times there have been thoughts of improving the format (by 1978 the Journal was at least being sent flat), but we owe it to Jonathan Webber and his colleagues for taking on the task of organizing the office and business side to make this a possibility. The rapid rise in the cost of subscriptions that has been necessary seems to have been more than balanced by the gains in readability and attractiveness. So far only a half-dozen subscriptions have been cancelled since the rise. The feel of work-in-progress should not be lost - it would be a pity if *JASO* simply became an 'ordinary' journal. It ought to retain its air of 'urgent provisionality', while being less of a physical trial to its readers. The continuity, as I said at the beginning, is the great achievement of *JASO*. It has travelled light, and stuck to some kind of theme, through the theoretical shoals of a far from easy decade. I would like to offer my personal congratulations to all the editors and their assistants, past and present. Their names are listed opposite.

EDWIN ARDENER

B O O K R E V I E W S

WENDY JAMES, *'Kwanim Pa: The Making of the Uduk People. An Ethnographic Study of Survival in the Sudan-Ethiopian Borderlands*, Oxford: Clarendon Press, Oxford University Press 1979. xi, 304 pp. £15.00.

This book is an interesting addition to material on the peoples of the Nile Basin, partly because it deals with a group of people who are not easily definable, linguistically or culturally, within the usually accepted methods of academic investigation. The Uduk are a border people, regarded by the more powerful groups who write and make history as a source of slaves and cheap labour. The survival of the group depends upon the assimilation of *cinkina* (waifs) who become the founders of birth groups but who continue to view themselves as lost people or outsiders. The word Uduk is linked with the Arabic *'attuq*, which can be used of a person saved from illness or death, or of a child born to his mother after she has lost one or more children. The Uduk, thus, regard themselves as freed slaves spared by God. The chief success of the book lies in the way that it effectively demonstrates how the Uduk have accommodated themselves to this self-image and have transformed it into one of 'self-respect'. This is achieved by the motifs of the *cinkina* and *gurunya* which, as images, impressively dominate the book.

The Uduk are described by the author as an 'ethnographic remnant'. She attaches the name Uduk to them rather hesitantly, not only because of the diversity of ways in which people in this area refer to themselves and others, but also because of the infrequent occurrence of monolinguals which makes accepted methods for identifying and classifying ethnic groups inadequate. The book draws attention to the ability of people constantly to re-define ethnic use and the consequent problems of 'outside' definitions. The word 'Uduk' itself belongs to the language of missionaries and colonial administrators.

The chief subject of the book are the 'Kwanim Pa, the Northern Uduk, 'the people of the homelands' who have survived against powerful odds. The theme of survival is expressed in social organisation, trading relations and in myth and ritual. The significance that they attach to certain historical circumstances which fragmented and almost destroyed them has created in the Uduk a self-awareness that survival in the present is integrally bound up with their past and essential to their continuity. This theme is expressed in the interchangeable images of the *cinkina* and *gurunya*. A *cinkina* is a waif, a foundling or a lost one brought

in from the wild, whose survival hangs in the balance. She is dependent on the hosts' protection, but her existence imposes an obligation because the survival of the community depends on it. The *gurunya*, the blue-black starling, is also a creature of the wild. The use of shiny blackness in rites of liminality, in contrast to that of red-ochre in those of incorporation, reflects the historical struggle between the bush and the village. The survival of the group for the Uduk is centred on women. Physical continuity is seen as achieved through women, although men are equal contributors to the process of nourishing the child both in the womb and while it is growing. The myth of the origin of marriage depicts women as the original cultivators of sorghum, and men as *gurunya* first seen in the trees. Men are thus also creatures of the wild incorporated into society to ensure fertility.

James says that the book does not represent a comprehensive report on Uduk society; instead it attempts to deal with the nature of the link between the past and present in Uduk society. She stresses the importance of historical themes in every aspect of Uduk life. Memory of the past, including genealogies, does not go beyond the upheavals of the late nineteenth century when people were brutally subjected to slave raids with the resultant acute shortage of women and children. People were forced to hide in the forest and wander in the bush. The imagery used by the Uduk in their mythical accounts of the early days of mankind closely resembles that used to represent periods of destruction. It is in the rites associated with the *gurunya* that there is the clearest expression of the historical themes that shape the Uduk. The rites are performed to protect a child whose mother has already lost children. The child is depicted as vulnerable, as a non-person brought in from the wild but who will ultimately be incorporated into the group. This is the 'Kwanim Pa - fragile, fragmented, existing on the edge of other, more clearly-defined groups yet surviving by assimilating an amalgam of other lost groups, and adapting social and cultural institutions in the process.

The author says that this depiction of history could be regarded as a collective representation of social reality with no reference to anything outside itself. She finds it necessary to refute this by showing that what appears to be an isolated pocket of matrilineal organisation has emerged as the result of adaptation to historical pressure. The material presented in the book as a whole has indeed been carefully selected and structured so as to make this readily apparent.

The book's concern with anthropology and history, and with history and myth, will inevitably be of interest to those who find that all three are at times indistinguishable. The import of '*Kwanim Pa*' lies in the implicit view that 'folk' memory is an integral and neglected aspect of the 'official' view of history rather than a distortion of it. Self-images created by people out of their own history operate in all societies, although in some they may be based on events of more intense historical impact than in others. The book successfully articulates these important issues.

PATRICIA HOLDEN

JOHN A. BARNES, *Who Should Know What? Social Science, Privacy and Ethics* [1979] Cambridge etc.: Cambridge University Press 1980. 188 pp., Bibliography, Index. £7.50 (also available in Paperback).

Barnes' concern is not primarily to present a set of moral answers that research workers should adopt. Instead he presents 'an historically-based framework within which the resolution of ethical questions may be considered and debated'. The book starts with a history of sociology and anthropology from a fascinating viewpoint, that of its relevance to ethical issues. Barnes sees four groups as involved in most research: the subjects, who Barnes suggests are better called citizens; the scientists, who are citizens as well; the sponsors; and the 'gatekeepers', those who can control access to the citizens themselves, such as a colonial administrator. In the nineteenth and early twentieth century, citizens were seen as passive objects of study; sponsors had relatively little power as many social scientists were rich; and gatekeepers assumed that their interests and those of scientists coincided. Social scientists saw themselves as providing objective knowledge that would contribute to enlightenment and thereby improve everybody's situation. Barnes traces the breakdown of this structure as knowledge comes to be seen more in terms of power and property and less in terms of enlightenment. The changing conception of knowledge and a lessening of the scientist's power in relationship to the other groups involved in research are what have led to an increasing concern with ethical questions, Barnes argues.

Ideally the diversity of interests among the four groups involved in research should be reconciled by honest negotiation. Informed consent by citizens and gatekeepers is, however, made impossible by the scientists' need to withhold information. To explain fully the research objectives in advance, including the hypotheses or the details of behaviour to be tested by sociologists or anthropologists, would in many cases defeat the purpose of the research. Barnes argues that the ethical solution to this problem is for the scientist to seek acceptance as a scientist by the citizens and gatekeepers. A scientist's role has to be understood as involving 'professional insatiable curiosity' that makes inquiry difficult to limit. At the same time, the scientist keeps personal information confidential if it can harm the person in any way. In this connection, Barnes raises without answering the question of what limits should ethnographic films observe, for almost of necessity the medium precludes confidentiality. He asks another intriguing question in parenthesis here - what will the effects be of mass-media sponsorship on anthropology? Whatever solutions to the numerous ethical issues raised by the case-studies of the research process which Barnes presents, the sociologist or anthropologist must make them himself. Professional associations provide little guidance. While these associations have codes of conduct, these are like other professions' statements of principle - vague and mainly relevant to in-fighting within the profession.

The preceding summary has been concerned to highlight two main strengths of the book. First it provides an interesting approach to the history of sociology and anthropology and integrates an analysis of the two disciplines far more thoroughly than many other books. Secondly its type of solution to the problems of the contemporary research process - getting citizens to accept the scientist as a scientist is, in my opinion, the right one. To these merits should be added a clear, jargon-free, pleasant style and a fairly comprehensive bibliography of sociological and anthropological discussions of ethical issues in the research process.

However, the book is written within severe limits, some of which if transcended might alter the book's conclusion. It restricts itself largely to the world of English-speaking academics and this helps define a manageable problem. Barnes deals with ethical questions of the research process on the basis of various accounts researchers have provided of particular problems. The focus is on the form of research, not the findings produced. This leads Barnes to overlook issues raised by recent literature on the effectiveness of evaluation research. The following case illustrates a major type of problem relevant to Barnes' analysis. An evaluation was done of the effectiveness of volunteer and professional probation workers. The volunteers proved more effective. As the professionals had the power, the effect of this study was to lead the professionals to eliminate the volunteers to remove a threat to themselves. Neither favourable nor unfavourable results could have helped the volunteers to survive. People can have good reasons to be suspicious of scientists as scientists. In addition, it seems possible to base an analysis on a broader data base than Barnes uses. For instance, an examination of the response rate to random surveys could establish whether people are becoming more suspicious of sociologists. Some systematic data on gatekeepers could be gathered. For instance, what countries impose restrictions and of what type on anthropologists?

Finally, we do not need to be so defensive about the research process itself. Answering a set of well-organised questions often provides people with a chance to come to increased self-awareness. Research is listening. Research is drawing people out. Communicating research is often giving a voice to those who have not spoken out, a written record to those with none. These things in themselves make the research process one where a good social scientist respects citizens.

KEN MENZIES

ERNEST CASHMORE, *Rastaman: The Rastafarian Movement in England*, London: George Allen and Unwin 1979. 263 pp. £10.00.

The Rastafarians emerged in Jamaica in the 1930s following the coronation of Ras Tafari as the Emperor Haile Selassie of Ethiopia. Drawing heavily on the inspiration of Marcus Garvey and the Bible, it is a movement of religious and political protest with a core of beliefs of the utmost simplicity: a recognition of the divinity of Haile Selassie and an assertion of Ethiopia as the homeland to which all the believers will be eventually returned. Since the thirties the movement has blossomed, with more than 100,000 supporters in Jamaica and surfacing throughout the Caribbean as well as in the USA, Canada, France and Britain.

Cashmore's book arrives as the latest in a flurry about the Rastafarians and about Jamaicans in Britain. His contribution is another reflection of the increasing visibility of West Indians, and Rastafarians in particular, in Britain. *Rastaman* is valuable in that it is something more than the latest off-spring of the reggae music business. It is a scholarly attempt to dissect the emergence of the Rastafari cult in Britain, that phenomenon which makes it, as Cashmore says, an outrage to tens of thousands of West Indians to call them anything but African. The achievements of the book are manifold: a well-documented account of the origins of the cult in Britain and its relationships with Black Power and the 'rude boys', an authentic examination of the likely experiences which shape the typical Rastaman, and a careful explanation of the way in which the symbolic structures of Rastafari provide a coherent perception of West Indian experience in Britain. In short, it is to date the most comprehensive and persuasive account of the significance of Rastafari in this country, or indeed in Jamaica.

One of the key problems facing the anthropologist or sociologist is that of objectivity - particularly with respect to the selection of data. In the case of the Rastafarians the Jamaican literature suggests a wide range of belief. Cashmore has, properly, attempted to evaluate Rastafari within its own symbolic terms by considering particular aspects of the belief system (e.g. the wearing of locks and colours, the continued use of a modified Jamaican *patois*, etc.). In this he has been only partially successful. Yet he could have pursued an even more rigorous semantic approach and moved beyond a portrayal of belief and ritual as symbols of group identification to an understanding of their role as conceptual structures, firmly based within semantics. As Crick has put it they are expressive reflections of human beings as 'meaning-makers' (*JASO* VI, no.2, 1975, and later expanded in his *Explorations in Language and Meaning*, London 1976).

However while the aspects of the Rastafari belief system that Cashmore has chosen to emphasize are very important, the Jamaican literature clearly suggests a much wider range of belief: special Rasta festivals, proscribed foods, the importance of marijuana, characteristic attitudes about (and on the part of) women, hostility

towards western medicine. Even given the fact that the movement in Britain is relatively young and that it is characterized by 'epistemological individualism', Cashmore, at least, cannot be unaware of this more extensive belief system. It is strange then that he chose not to comment on it, especially as there is evidence that the distinctive diet, for instance, is quite widely observed among Rastas in Birmingham, the very locale where Cashmore did the bulk of his research (see J. Plummer, *Movement of Jah People*, Birmingham 1978). The most serious of his omissions is his paltry two-page treatment of women in the movement (pp.78-9). Such brevity is only partially excused by choosing to title the book *Rastaman*. The pattern of belief within the movement is highly male-centred and mother-denying and yet, by his own admission, women comprise more than a quarter of the movement. This surely requires more detailed examination.

An attempt to simplify the typical process of becoming a Rasta has produced a model that is perhaps too linear. Cashmore remarks that

... the process of becoming a Rastaman can be divided into four broad phases; first the apprehension of racial disadvantage and the fresh symbolic meaning this brought to blackness; next the loss of plausibility of the parents' beliefs and the structure they seemed to support; thirdly the drift to Rastafari; and lastly, the acceptance of Haile Selassie as the divine redeemer of the black peoples.

This programatic rendering of the process seems to me to be unhelpful and unnecessarily simplistic; indeed precisely the type of *post hoc* rationalization that Cashmore seems to believe the sociologist should avoid. While each of the four 'phases' may be an important strand in the experience of the young West Indian, they will continually interact and overlap to produce a total cultural experience that engulfs any phasic arrangement. This methodological flaw re-emerges in the arrangement of the book, because in attempting to segment Rasta experience Cashmore is forced within each section continually to attempt to reconstitute a total picture if the importance of each 'phase' is to be understood. Similarly, by considering the external world only in passing and only in so far as it affects the formative experience of the Rasta, Cashmore has presented the process of becoming a Rasta as inexorable, despite his protestations to the contrary. Are there then no cultural alternatives to Rastafari for the young West Indian in Britain today? In this respect a more detailed numerative approach would have been welcome. What proportion of West Indian youth join the Rastafarians in an area like Handsworth, say, and what are the reasons of those who do not?

This though is a question for further research and it would be harsh to criticize Cashmore for failing to answer all the questions when he has addressed himself to so many. Overall his book is a competent and convincing account of those 'dramatic and comprehensive changes in consciousness' occurring within the

West Indian community of England during the 1970s. While it is in many respects a defence of the Rastaman against the mounting attacks of 'Babylon', it also serves to demystify him. As such the book can only be of value to all those working within the West Indian community and, indeed, to all those interested in the tenacity of religious beliefs under the assault of secular Western values. Yet, having said that, the time has surely come to question the common assumption that the West Indian community is a culturally homogeneous entity. The fact that all West Indians are black and working class does not in itself provide them with a uniform culture. As Foner has remarked in *Jamaica Farewell* (London 1979), 'Other status distinctions and inequalities among them ... tend to be overlooked'. I believe that if Rastafari is to be understood the description must be rooted firmly within a historical context. In this respect it is only compounding difficulties to assume that all West Indians are somehow historically equal without reference to their island of origin. Cashmore notes early on in the book that the majority of the Rastas he worked with were of Jamaican descent (p.70), but thereafter he refers to them all simply as West Indians. Are we to assume then that the origin of the cultists is unimportant, that it is subsumed by the force of Rastafari? Yet in his opening chapter Cashmore clearly suggests that he recognizes the importance of the link of religious dissent in Jamaican history, from the Native Baptists through Bedward and Garvey to the Rastafarians. But that link is also evidence of the *continued* importance of religious symbols to the mass of Jamaicans in structuring everyday experience. I believe that Cashmore should have concentrated on the Jamaican element within the Rastafari movement in Britain and in a more determined manner drawn these historical threads into the present experience of Jamaicans in Britain. There is evidence that he recognizes the continuities between generations, between Pentecostalism and Rastafari, but as it stands his opening chapter, while interesting in itself, remains almost entirely unrelated to the rest of the book. Indeed approached from a historical angle we might say that the crucial point is not, as Cashmore suggests, that there is a radical disjunction between first and second generation Jamaicans in Britain (in the sense of a growing impatience on the part of the youth with the quiescence of their parents), but rather, that of the overwhelming similarity in the continued efficacy of religious symbols.

SIMON MELLOR

ALAN R. BEALS, *Gopalpur: A South Indian Village* [1962], Eastbourne: Holt-Saunders; & New York: Holt, Rinehart and Winston; 1980 (Fieldwork Edition). xii, 125 pp. £3.25 (Paper).

The first edition of Alan Beals' *Gopalpur: A South Indian Village* (New York 1962) was enthusiastically received in the professional press. As Marriott wrote in *American Anthropologist* in 1963, 'By

far the most vivid, comprehensive, and unified picture of Indian peasant life in the English language is this modest-appearing community study from Mysore....' It is an indication of just how well this text has stood the test of time that these same words can be echoed with equal force now, 18 years later, in response to this new edition.

The current edition of *Gopalpur* represents the amalgamation of two works published individually by Beals; to the original 1962 text of *Gopalpur* has been appended a chapter entitled 'Fieldwork in Gopalpur' which first appeared as an article in George D. Spindler's *Being an Anthropologist: Fieldwork in Eleven Cultures* (New York 1970). The marriage of the two works is fortuitous as the addition of the chapter detailing some of the author's experiences in the field is illuminating both in its own right and in its amplification of various themes from the main text.

The first edition of *Gopalpur* was 99 pages long and in addition to the text contained one map, one glossary, four photographs, three tables and a section entitled 'Recommended Reading'. The new edition differs from the original in that the text has been extended to 125 pages (by the inclusion of the chapter on fieldwork), the original four photographs are now supplemented by a further six, and the list of Recommended Reading has doubled in size (from eight to sixteen items). With the minor exception of a prefatory paragraph entitled 'About the Author' (p.v), the first 97 pages of this edition are identical with those of the earlier edition: pagination, lay-out, maps, tables and text - even the typeface - are consistent with the earlier version.

It might well be asked why this book was re-issued when the original was so obviously satisfactory. As one of the Stanford University Case Studies in Cultural Anthropology, the original edition has been easily accessible to students of Indian anthropology. The inclusion of the chapter on fieldwork, while illuminating, was certainly not essential; those interested in it could locate it in its earlier source without excessive difficulty. As it stands now *Gopalpur* seems strangely frozen in a peculiarly constituted anthropological present. Undoubtedly were Beals to go to Gopalpur now the book he would produce would describe a village only vaguely recognizable as the subject of his 1959-60 study. The effects of two decades of government policy regarding the extension of irrigation, electricity, bus routes, education, and the Green Revolution (to name but a few) have surely resulted in profound changes in Gopalpur. However inadvertently, the re-issue of Beals' 1962 text can only serve to maintain the misleading notion of the changelessness of Indian peasant society. The village it describes can have only a partial resemblance to contemporary Gopalpur. Rather than the addition of the chapter on fieldwork, I feel that the book would perhaps have been further improved by the inclusion of a chapter on 'Gopalpur: Then and Now'. Presumably Beals has not had the opportunity of returning to Gopalpur since the original research was undertaken.

In the light of the overall value of this book, however, such criticism becomes immaterial. Although issue may be taken with the presentation of such dated material as if it were a contem-

porary field study, the book nevertheless remains a valuable contribution to Indian anthropology; taken as a historical document *Gopalpur* is clearly a classic of anthropological literature. Beals is an author of unusual perception. I found particularly effective his technique of interspersing his own narrative descriptions and explanations with local stories, songs and poems often quoted at great length. Beals' emphasis on the personal, human element (as evidenced by his treatment of such diverse topics as aspects of children's play, the role of food in the socialization process, and the resolution of personal conflict) invests this slim volume with an unexpected, but most welcome, depth. I similarly found the author's awareness of his own role in the events taking place around him most refreshing. Beals in no way tries to disguise his involvement in the daily life of Gopalpur while at the same time he never dominates the scene. In *Gopalpur* we get an example of participant observation in its most effective form - with balanced emphasis on both the participation and the observation. Moreover, because of Beals' artistry in the presentation of this material, the reader finds himself being caught up in the web of social life in Gopalpur much as the author himself was personally drawn in 20 years ago. The text is populated by 'real' people, displaying a multi-dimensionality that the author has managed to capture admirably - so much so that the reader comes away from the book with a knowledge not only of the social structure of a village in South India but also with the feeling that he knows the inhabitants of that village.

There are a number of minor criticisms which can be applied to this otherwise excellent text. Thus for example 'Beals, 1965', referred to three times in ch. 9, is left unexplained: it receives no mention either in the references at the end of the chapter or in the Recommended Reading list concluding the book. Similarly, an index and an up-dated Preface would have been appreciated. Mention of the desirability of a chapter describing contemporary Gopalpur has already been made. Criticism regarding the general lack of footnotes, references, caste names, or any attempt at a 'quantitative or systematic analysis of any topic' has been levelled in the past (thus Marriott, in his review of the book in 1963) - but I feel that such criticism is largely unjustified. The unity, comprehensiveness and coherence that Beals has achieved in the presentation of his materials on Gopalpur far outweigh such petty complaints. *Gopalpur* was never intended as the definitive work on South Indian villages. It was written as an introductory text designed to introduce the undergraduate to life in village India. In this task Beals has been remarkably successful.

STEVEN SEIDENBERG

J.M. TAYLOR, *Evita Perón: The Myths of a Woman*, Oxford: Basil Blackwell 1979 [Pavilion Series in Social Anthropology, general editor F.G. Bailey]. ix, 176 pp., Bibliography, Index. £7.95.

MUSICAL BOX OFFICE

EVITA is now the biggest grossing musical in showbiz history.

By next week there will be five productions of Evita on stages around the world—London, New York, Los Angeles, Chicago and Adelaide—taking a total approaching half a million pounds a week.

The breakdown: London (£70,000), New York (£125,000), LA (£125,000), Adelaide (£90,000).

The show about Evita Peron opens in Chicago on Monday where it is expected to take £120,000 a week, and it's now clear the musical has done something it's heroine, Evita Peron, could only dream of — conquer the world.

Daily Mail
May 21, 1980

- Evita Perón: Housewife, Superstar - as Dame Edna Everage/Barry Humphries might graciously have allowed her; not the attribution of an anthropologist, but of a comedian who also makes sense, and nonsense, out of myths of womanhood: both express the particular need for, and use of, myths and symbols in complex industrialised societies. Allusions to drama and 'showbiz' are inevitable, and they form an interesting theme in the life of Evita from her origins to the present day. The author's principal aim, in this instance, is to examine the living myth about a recent historical figure of such significance that she still captivates audiences today, in and beyond her native Argentina, for a host of different reasons. Furthermore, it is clear that, 'People have acted on what they believed to be the truth about Eva Perón, and they have acted

as well on what they believed that other people believed about Eva Perón' (p.9). In studying the implications of this, the book makes a contribution to any debate concerning the interaction of 'image' and 'reality': a debate central to anthropology. Appropriately, Julie Taylor has deliberately presented her material in such a way that, just as in myth itself, events and their analysis overlap.

The degree to which the style of subject-matter and analysis are merged here seems to be found only rarely in anthropology and although this book remains congruent with the other volumes in the Pavilion Series of Social Anthropology, of which it is the 'latest instalment', it is a new step, and up to a point does show a remarkable degree of imagination, in perceiving a society other than through the eyes of the 'traditional monograph' writer. The book has been developed from her original doctoral thesis, undertaken here at Oxford seven years ago. It is to be noted that it follows up her fieldwork, at the outset of which, her task was to 'define the working-class myth of Eva Perón and to discover its connections with Peronist propaganda'. This of course she does, and finds it to be far more complicated than she could have ever at first suspected.

Evita Perón, who rose from humble origins to international renown as First Lady of Argentina at the side of her husband Juan Perón, inspired such loyalty among her people that two decades after her death she remained the heroine of the working classes and political dynamite in Argentina (p.1).

Justification enough for an examination of her influence in anthropological terms.

After introducing her subject Taylor looks at the historical background, and the regimes of Rosas, Rivadavia, and Yrigoyen, in the light of the rise of Peronism. She finds in all the propaganda and ways in which events are perceived and described, the same oppositions - civilised:barbaric, man:woman, establishment:proletariat, rationality:intuition/instinct, culture:nature - and the same connections between these oppositions. These have also been present in Western European thought since the Enlightenment and the evolution of the 'noble savage', and are used in the same way to rationalize, classify and label 'revolution'. At this point it seems likely that further oppositions of right and left (political and directional), and 'normal' and 'deviant' sex could also be found, and later in the details of the mythical accounts they do in fact appear: Juan Perón is at times implied to be homosexual and Eva to be frigid, or rapacious, even vampirine.

Taylor goes on to provide us with a lucid biography of Eva Perón. She manages to present what at least appears to be an unbiased account, and it is undeniably an exciting and intriguing tale.

Both Peronism and its enemies gradually focussed a spotlight on Maria Eva Duarte de Perón. This woman, known only in Argentina, whose last publicity notice as a radio actress

had appeared at Christmas 1945, had involved herself before Christmas 1946 in all the complex roles which would make her a political figure of international importance in only six years. Thrilled or alarmed, Argentinians watched Eva's growing contacts with organised labour, her activities in social welfare, her feminist initiatives, and her influence in the press (p.40).

It seems that whatever the myths and the images may add or subtract Evita's power brought female suffrage, popular education and housing, hospitals and social welfare, and to some extent Juan Perón himself to the people of Argentina. Surprisingly these facts are often not emphasized in the most romanticised of images, such as that of the musical 'Evita', and are rarely pointed out even by feminist movements.

The information that Taylor has gathered makes it possible to see the process of the creation of a myth in progress, as she tells us how, after her death, Evita was kept before the public through the annual celebration of her birth, her renouncement of the vice-presidency, the pronouncement of her as Spiritual Chief of the Nation on October 17, and by her death itself on July 26 1952. The latter was constantly recalled by nightly news broadcasts announcing, 'It is 8.25, the hour in which Eva Perón passed into immortality'. It is not clear even today if her embalmed body has yet been interred.

This biography is then contrasted with the two major myths about her: 'The Lady of Hope' associated with orthodox Peronism, and 'that woman' (*esa mujer*) around whom anti-Peronists have constructed 'the Black Myth'. Later chapters examine the image of 'the masses' created by the 'myth-makers', and a further aspect of Evita's myth which sees her as a Revolutionary, wholly identifiable with the people (the derivation of this lying in the other myths themselves). In a chapter analysing the two myths and in these last chapters Taylor successfully demonstrates that there are common underlying structures and values, concomitant with the oppositions above, determining the symbolism of female power. Though, if in the light of the work of other writers (M. Douglas, V. Turner and J. Okely), we can set up 'power' as an opposite of 'order' or 'structure' - then what Taylor can be seen as speaking of are the symbolic representations of those factors which create 'power' itself; or of where 'power' is located in the social structure. The common underlying values that Taylor has consistently demonstrated are marginality, physicality and instinct, irrationality, illogic, intuition, Nature, approximation to a 'primitive' state, and, of course, their opposites. Of additional interest is the recognition of the association of them with the 'left wing', with 'the masses', as well as with 'Woman', providing good ammunition against the proposition that the images, almost universally associated with women, are in any way 'naturally ascribed'.

Taylor does make claims for her information regarding 'universal' notions of female power, and 'feminist' issues could be seen to 'cross-cut' the book. The classic images of woman as Virgin, Mother, and Whore, are implicit in the investigation.

Further material presented by the author provides useful evidence for the widely-supported association of 'Woman' with 'Nature', in opposition to 'Culture', raising questions about the stereotypes of women, of the working classes, and of the nature of images like that of 'The Lady of Hope' when they occur elsewhere.

However, despite the light that is also thrown on the recent history of Argentina, the brevity of the book's conclusion and its concentration solely on the idea of woman as anti-structure (in the Turnerian sense) and hence a source of power, arouses suspicions of the scale of the contribution that this book is really making. Taylor provides no information that could be used to call her models into question -justifiably perhaps, but it does show that she includes only certain sorts of information. This may be an unfair criticism, or it may just be an expression of surprise that a book can 'work' so well; the analysis in the end is a pleasing one.

Lastly, I do feel that the book goes some way towards demonstrating how the social world is created through the processes by which we make sense of it. In this instance by showing how the middle classes made a 'universal' idol (a myth not a person) in the image of their own values. As the accompanying newspaper clipping shows this myth-creation has not stopped with the Argentinian middle classes; the embodiment of all the features of Eva's rise to power provides an astonishing story and the base for an endless fascination for other human beings.

ALARIC PUGH

CHRISTOPHER R. HALLPIKE, *The Foundations of Primitive Thought*, Oxford: Clarendon Press 1980. xiii, 516 pp., Index. £17.50.

According to Professor Hallpike, anthropological investigations of mind have amounted to little more than 'amateurish speculation'. In this respect he is certainly correct, for they have been advanced in the absence of an empirically verifiable theory of cognitive processes. By assimilating Piaget's developmental psychology to the centre of the discourse, in effect, *The Foundations of Primitive Thought* has laid the groundwork for the emergence of a whole new discipline.

Piaget maintains that knowledge develops holistically out of the dialectical, self-regulating process of a subject's accommodation to reality and concomitant assimilation of experience to existing cognitive structures. The dialectic passes through a fixed sequence of stages ranging from the sensori-motor to the mastery of formal operations (in late adolescence normally). The latter stage is characterized by the capacity to 'think in a hypothetico-deductive manner solely on the basis of the logical implications of propositions, which are quite divorced from the

constraints of experience'. Prior to this stage, at which systems of relations and their transformations can be grasped, thought is based on and in sensorily perceptible properties and configurations, the concrete appearances of things. For example, if a child at the pre-operatory stage is shown

twelve beads, ten red and two blue, and is asked "Are there more red beads, or more beads?", he will answer that there are more red beads, partly because he cannot think of them as being simultaneously beads, and also red as opposed to blue, but also because he does not yet grasp the reversible relation that if $A + A' = B$, then $A = B - A'$.

Similarly, if the child is presented with two rows of beads in one-to-one correspondence, and then one row is spaced at greater intervals, the child will suppose that the longer row contains more beads. This is because he cannot imagine the initial and the visible state simultaneously, and therefore fails to recognize that the increase in length is compensated by a decrease in density.

Hallpike's basic contention is that the thought processes of most individuals in primitive societies are arrested at the late pre-operatory stage (about age 5 to 6). Only under 'favourable circumstances', such as having to navigate vast distances, do concrete operations develop.

This retardation is due to an environment less 'cognitively demanding' than the literate, industrial ambience we live in. Our schooling, for example, 'is based heavily on *telling* out of context rather than *showing* in context'. There is not the same dissociation in the learning process of the primitive. For him the word is inherently related to the thing he has been shown it signifies, and language remains a 'mode of action' (to quote Malinowski), not reflection. So too does the primitive's experience of the physical world tend to reinforce the tenets of pre-operatory thought. Given a limited technology, he has not the means of objectifying his sensations by perceiving things reacting on other things, instead of on his own body. He is also surrounded by 'organic processes [which], unlike machines, cannot be taken apart', or have their action reversed. Causal relations are thus not as immediately apparent to him.

Hallpike amasses a formidable array of ethnographic cases in illustration of his thesis. These range from collective representations of number and measurement to probability and accident, space and time. The primitive does not develop an operational concept of time because he reckons time in terms of social activities, such as the agricultural cycle (in which there are gaps, periods of inactivity between harvests), and other concrete indications, such as the *pars pro toto* 'sleep' for a 24-hour period, which are qualitatively unique. Because these processes are not homogenous, they are 'durationally incommensurate', hindering an operational understanding of simultaneity which is the very basis of the 'system of co-ordinations of successive spatial states' that is time. Primitive notions of magic and causation also come under scrutiny, and are found to be 'absolutist',

phenomenalist, psychologistic, irreversible, and static, lacking a real grasp of process'.

The above almost sounds like a string of profanities, and for those who hold the construct of the 'cerebral savage' sacred, much will seem like blasphemy. But the importance of *Foundations* lies in the staunchness of the way it displays the illogicalities embedded in the logic of the concrete, and the reasons for these. The cultural relativist position of Lévi-Strauss and Leach comes under a barrage of attacks. The world is not a continuum awaiting our language categories so as to be discriminated into things, for already by the end of the sensori-motor period (about 18 months), the child can conserve the permanent object, having formed an image, or 'interiorized imitation' of it. Nor can symbolism be understood on the analogy of language. For symbols are motivated affectively and perceptually in ways that linguistic signs are not. 'It is the specific characteristics of each symbol, not its relation with other symbols, that are fundamental'; stone is associated with power and permanence cross-culturally because that is the sensation it evokes in us viscerally. Further, with respect to classification, the primitive does not construct hierarchies of logical classes (for as the bead example illustrates, the quantification of logical class does not come readily) but Vygotsky's complexes, and

basic object categories, such as "dog", "hammer", "chair", etc., which are given their coherence by image-based prototypes which are ... reflections of real world structuration [i.e. frequency of occurrence].

Atop the ruins of the culture structured like a language model, Hallpike is able to construct a refined and extremely rigorous framework for the translation of culture not just verbally, but experientially. To the extent that the paradigm of pre-operatory thought holds, then such words as 'all' and 'some' - which can only assume their logical definitions if classes not complexes exist and are quantified - are better translated as 'very many' and 'a few'. The same goes for numbers; 'three objects' constitute 'a triad', a perceptual configuration, not an enumeration. But is the fit between the collective representations of primitive societies and pre-operatory thought really so exact?

There are moments at which the richness of the ethnography seems to escape the strictures of Hallpike's version of developmental theory. On p.276 he surmises that unschooled Kédang would be 'actual or latent conservers' in experiments involving liquid displacements, yet earlier (p.62) he rejected the concept of latent structures. This inconsistency is telling, and raises the question of whether the stages of cognitive development are simply reproduced wholesale, or are modified in the context of primitive society. It could be that different domains of social life affect cognitive structures differently. The Australian Aborigine, unable to conserve volume, must nevertheless conserve marital alliances in order to employ his kinship terminology.

Piaget holds that there are three levels to the way we know and represent the world; that of actions and their co-ordination, that of imagery (interiorized imitation), and finally that of verbal formulation. It is the last level, the *logos*, which dominates the collective representations of Western society. The rendering of a perspicacious verbal account of an argument or event is one of the paramount values of the tradition from which we spring. It is this criterion which is then used to judge the collective representations of the primitive, which are action-, and image-, not verbally-based, and to assign him to an anterior stage of our own cognitive development. But if the primitive values the image, the symbolic expression of relationship, over the verbal, then the last two levels are inverted. Hence, there can only be an approximate fit between representations, and the judgement is only accurate to the extent that it is circular.

Hallpike himself has stressed that

Non-verbal thought, based on action ~~and~~ imagery, is a fundamental aspect of cognitive processes, and in understanding primitive society the problem is the translation of non-verbal, image-based thought into verbal form.

It is instructive in this regard to consider the bead example. At the verbal level, the child's answer is false, but the child cannot act upon (manipulate) the beads falsely, or perceive (image) their arrangement wrongly. The problem is one of translation. For us in relation to the primitive, this problem is compounded by having to take human valuations into account.

Finally, when ontogeny within Western society is taken as the model for a phylogeny of cultures, a puzzling collapse of environmental differences takes place. It is tacitly assumed that our environment encompasses and is the logical resolution of all others, which is untenable.

The above reservations do not detract from *The Foundations of Primitive Thought*. Professor Hallpike is an iconoclast and the pellucidity of his style is exemplary. The reader will find the book a constant source of new and provocative insights.

DAVID HOWES

WILLIAM BASCOM, *Sixteen Cowries: Yoruba Divination from Africa to the New World*, Bloomington and London: Indiana University Press 1980. vii, 790 pp. \$22.50.

This book, published some forty years after his original fieldwork at Ile-Ife in 1938-39, shows William Bascom's long dedication to Yoruba studies. Its title suggests a companion volume to his masterwork of a decade ago, *Ifa Divination: Communication between*

Gods and Man in West Africa, but its scope is considerably narrower. The earlier book provides a comprehensive analysis of the Ifa system of belief and the sociological factors of its practice, as well as a substantial body of the ritual poetry. This new one presents only a brief introduction to what is essentially a collection of divination verses from one source.

Sixteen Cowries reveals a system of divination related to Ifa in its mythology and form, but different in several fascinating ways. While many scholars have investigated Ifa divination, practised in various forms by different ethnic groups throughout West Africa, few researchers have looked into this alternative system of sixteen cowries. Bascom suggests the reason for this may be that this is a simpler mode and held in less esteem in West Africa, although in the Americas it has spread more widely and is more frequently employed than Ifa. He attributes its popularity in the new world to its relative simplicity; its close association with Shango, Yemọja, Ọshun, and other Yoruba gods, whose cults have proliferated abroad; and the fact that it can be performed by both men and women (apparently women outnumber men), whereas Ifa priests are always male (*babalawo* means 'father of secrets').

The main value of this book is the publication of an entire corpus of divination verses known by a single ritual specialist, Salakọ. Born about 1880, he was apprenticed to the cult of Orishala at Ọyọ at the end of the nineteenth century. By the time Bascom met him, during a second period of fieldwork in 1951-52, Salakọ was already over seventy years old. He sensed the rapid changes taking place in Nigeria, noted that fewer people were now consulting diviners and that apprentices were no longer coming forth for initiation to the cult. He agreed to recite the oral texts stored in his memory so that the knowledge he had acquired over a lifetime would not be lost.

These verses are beautifully set out with the Yoruba version printed on one side and the English translation on the facing page. Yoruba scholars have assisted both in the transcription from the tape-recording and in the translation into English. This careful preservation of a body of oral literature deriving from a pristine era of Yoruba culture marks a considerable achievement.

That said, I confess to some unease about the title: it promises more historical and sociological interpretation of this system of divination than the introduction gives. True, Bascom shows confirmation of the divining figures from his own research in other parts of Yorubaland, from Maupoil's work in Dahomey, from Bastide and Verger in Brazil and from informants in Cuba. This has to do with the congruence of transfer of the operational system and not with the social patterns of its practice. Here evidence is lacking. Little is known about the sociological aspects of this method of divining: how widespread it is (was) in Yorubaland, whether its ritual specialists are clustered in particular geographical areas or why this system has had preference over Ifa in the new world. There are also questions about women's participation. Why should women be excluded from the priesthood of Ifa yet take part as diviners in this similar system? Are clients likely to be predominantly male or female?

Is it only outside Yorubaland - in Brazil, Cuba and Spanish Harlem - that women as practitioners are found in greater numbers than men?

Sixteen Cowries, then, is an important addition to the continuing work in Yoruba studies in Nigeria and abroad: as an enduring record for the archives and as a 'divining' of future lines of research.

HELEN CALLAWAY

OTHER NOTICES

FAREN R. AKINS *et al.*, *Behavioral Development of Nonhuman Primates: An Abstracted Bibliography*, New York etc.: Plenum 1980. 304 pp., Indexes. \$75.00.

As the publisher's blurb tells us, 'this bibliography contains references and abstracts for over 600 articles, chapters, and books dealing with the general area of nonhuman primate behavioral development. Emphasis is placed on those publications investigating the effects of early environmental stressors: social isolation, dietary deficiencies, stimulus deprivation, mother-infant and peer-peer separation, surgically and naturally induced neurological dysfunction, birth traumas, and various types of unusual rearing conditions. This is the most comprehensive compilation of this literature ever achieved, with references extending from the early 1900s to date. An extensive indexing system, which includes separate sections for authors, primate species, topical areas, review articles, monographs, chapters and books which deal with this material in a global, theoretical fashion, is included as an aid to the use of this volume.'

Will social anthropologists have any use for this volume? Will they, in other words, want to refer to deprivation experiments that show what goes wrong when monkeys and apes are not allowed to develop normally? I somewhat doubt it. Why not? Because, I suppose, of the lack of any clear evidence that the same kinds of developmental processes occur in humans as occur in monkeys. Humans become 'enculturated' at birth and their development is a cultural as well as an organic process. In the case of monkeys there are certain developmental stages characteristic of any given species and these underlie the special features that are situation-specific and acquired through learning.

This non-applicability of non-human primate data to man is not just relevant to whether or not anthropologists will use this bibliography but also to the *morality* of the studies abstracted for us here. In the early days of deprivation studies, in the 1950s when Harry Harlow was hitting the headlines, the rationale for these studies (and the reason they were funded) was that they would 'shed light on human development'. Have they done so? I'm not convinced they have. What has happened instead is that they have become a sort of scientific genre in their own right. Experimenter A shows that monkeys taken from their mothers at birth become depressed, mutilate themselves and reject other monkeys. Experimenter B asks: what would happen if an isolation-reared female had offspring? Experimenter C invents a 'rape rack' to which a female can be fixed while a male copulates with her. It turns out when she gives birth that she rejects and kills her offspring. The results are so dramatic that labs all

over the U.S.A. repeat the experiments: hundreds of monkeys are isolated, force-mated, and reject their offspring. The journals are full of the story. Books appear on it. Careers are built on it. Next, comes the question of *how long* a monkey must be isolated etc. etc.

All this is routinely practised in the cause of science, mostly in the U.S.A. Some would see it as the unacceptable face of primatology. This new bibliography can serve as a memorial to the thousands of monkeys involved, whose lives have been depleted in one way or another. What there is too little of is enlightened discussion of the relevance to man. Bowlby is one of the few to have really become involved in this issue: he has shown clear parallels between the non-human and the human cases. Even if we accept that homologous processes exist between man and monkey (and many psychologists do not), this still does not get us any further forward than we were before the monkey research began. All we seem to have done is to have found out a very great deal about monkeys. This was not the rationale. And the cost in terms of animal suffering has been, and continues to be, horrific.

V. REYNOLDS

Journal of the Oxford University India Society, editors Pavan K. Sukdev and Steven Evans, published once a term, price 40p per issue. Correspondence to the editors at University and Hertford Colleges respectively.

This year the India Society of Oxford has launched a new publication called, appropriately enough, the *Journal of the Oxford University India Society*. Published once a term, in a format that will be instantly familiar to long-standing readers of *JASO* (i.e. stencil-duplicated pages with pastel covers and stapled binding), the *Journal of the Oxford University India Society* fills a previously unexploited niche in Oxford. Defying easy categorisation (as evidenced by its editors' frequent alternation between calling it a 'Journal' and a 'magazine'), *J.O.U.I.S.* attempts, with considerable success, to be simultaneously an academic forum reflecting recent research on India and a topical domestic sounding-board for local events. The 'Diary of Events' contained in the *Journal* will provide Oxford readers with convenient information about lectures, seminars, films, concerts etc. pertaining to India. As the editors have expressed the intention of including diaries and notices from other parts of England, *J.O.U.I.S.* may soon outgrow its parochial nature. This may also be seen from the balance maintained between the local and external contributors of the academic articles, which cover a wide range of topics. The establishment of a new *Journal* is an ambitious undertaking. The editors of the *Journal of the Oxford University India Society* have made a successful start.

S.S.

Journal of the Anthropological Study of Human Movement [JASHM],
Vol. I, No. 1, [New York University] 1980. 69 pp. \$1.50.

The weekly series of seminars at the Department of Dance at New York University has generated sufficient good discussion in the field of the anthropology of human movement for two of its principal members to have taken the initiative to record its more important transactions in a new Journal. The first number of the new-born *JASHM*, which recently arrived at the office of *JASO*, makes interesting reading for anyone familiar with the latter. Quite apart from the fact that the editors of *JASHM* specifically indicate *JASO* as their model on page 1 - the compliment is gratefully acknowledged - even down to the details of (our former) format, the methodological preferences indicated in the contributions to the first issue bear witness to the diffusion of many *JASO* preoccupations across the Atlantic. These have principally been carried by Drid Williams, a former contributor to *JASO* who wrote her D.Phil. in Oxford some years ago and is now Editorial Adviser of *JASHM*; her work is unashamedly rooted in what some call semantic anthropology and her paper in the current issue is liberal with quotations from Edwin Ardener (reproducing his description of an Ibo handshake for the first time in print). Her principal colleague is Joann Wheeler Kealiinohomoku, of the University of Northern Arizona; the latter tends however towards the American cultural anthropological tradition, and she reiterates a dogmatic relativism, firmly rejecting 'western elitist standards' that would classify dance forms as universally 'artistic'. The graduate students' papers and reviews presented here reflect their enthusiastic sense of the complementarity of their two teachers or sources of inspiration, though whether fruitful reinterpretations of the meaning of the dance will emerge from such a symbiosis remains to be seen in future issues of the Journal. The students' own articles here are heavily ethnographic in emphasis; nor are there many anthropological references cited beyond those to Malcolm Crick, Shirley Ardener and the Oxford Women's Anthropology Group. Still, despite the morphological analyses and the profusion of notational sequences *JASHM* can certainly be recommended to any non-specialist concerned with carrying forward an interest in the anthropology of the body or the relationship between taxonomies of movement and the study of ritual behaviour.

J.W.

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FATHERS AND FATHERS-IN-LAW¹

In his recent and polemical review of the state of Mediterranean anthropology, John Davis is especially critical of achievements in the study of family and kinship, decrying throughout the looseness and vagueness which pervade the subject and particularly so when reference is made to 'the importance of the family'. Indeed, so ill does Davis take the imprecision of accounts dealing with the family's importance that he proposes largely to ignore that question, concentrating instead, for purposes of comparison, on the 'kinds of family, kinds of kinship, kinds of family-like tie' to be found in the Mediterranean, and defining such 'kinds' by reference to modes of residence and inheritance and, in general, to the structural variants of Mediterranean family and kinship organization.²

Since the two best books on Greece, John Campbell's *Honour, Family and Patronage* (Oxford 1964) and Juliet du Boulay's *Portrait of a Greek Mountain Village* (Oxford 1974), could both fairly be described as essentially concerned with family and kinship, or rather, with the household (and it is a significant ethnographic fact that in northern Mediterranean societies family and kinship tend to reduce to the latter), some may feel Davis' judgement to be a little harsh. Nevertheless I think he has a point. The imprecision of which Davis complains results not only from the professional inadequacies of Mediterranean ethnographers, but also from the actual nature of Mediterranean kinship and the part it plays (or fails to play) in social organization. For while it is an easy matter impressionistically to register the importance of the family and of family ties within Mediterranean

¹ Text of a paper delivered at the Institute of Social Anthropology, Oxford, on 17 October 1980.

² J. Davis, *People of the Mediterranean*, London: Routledge and Kegan Paul 1977, pp. 167.

societies, it is by no means so easy to specify what part they play in determining social behaviour or in structuring social life.

There can be little argument with Davis' suggestion that there are differences to be found between the 'experiences' of people whose domestic arrangements are variously nuclear or extended, uxorilocal or virilocal, or who inherit at marriage or at the death of parents. What is more problematic is whether these 'experiences', as they relate to kinship and family, are either exhausted by, reducible to, or comprehensible predominantly in terms of, 'the application of rules of residence and inheritance', or whether we must enter somewhat murkier fields. In fact it is arguable that Mediterranean kinship studies appear to lack the precision and perhaps even the interest of those conducted in, say, South America or South-East Asia. This is for a combination of reasons which are partly relative, partly absolute, but which in both cases result from the limited extent to which Mediterranean kinship is capable of being viewed in terms of rules, or of structure: relative, in that the familiar bilateral kindreds and descriptive cognatic terminologies of Mediterranean Europe do not allow us to dwell on the formal aspects of kinship with the same degree of fascination as that aroused by the alien intricacies of, for example, Crow-Omaha terminologies, or the practice of asymmetrical exchange; absolute, in that the general absence of any kinship-based corporate groups other than the nuclear or extended nuclear family, and equally the paucity of positive rules regulating or determining conduct between various categories of kin beyond those which might better be seen as a series of predispositions in accordance with a shared notion of 'good behaviour', effectively prevent the identity of kinship organization and social organization of the sort often claimed for 'primitive' or 'tribal' societies.³

And yet family and kinship remain important, both phenomenologically to the members of Mediterranean societies themselves, and, objectively I think, for any understanding of the nature of Mediterranean societies. But in that case what one is dealing with is predominantly a series of values, attitudes, and even - to use a word not popular in anthropology - sentiments, rather than a series of social rules or a variety of social structure. And whilst these may exhibit regularities (indeed, if they did not we should have to admit that they lay outside the bounds of sociological analysis), they are nevertheless by their nature uncertain, labile, and sometimes, from context to context, contradictory. Thus, although I would claim that Campbell, du Boulay, and others have described them quite successfully, it is not surprising that they have also generated those 'loose

³ Cf. J. Pitt-Rivers, 'The Moral Foundations of the Family', in J. Pitt-Rivers, *The Fate of Schechem, or the Politics of Sex*, Cambridge: Cambridge University Press 1977, pp. 71-93.

and vague' remarks about which Davis justifiably complains - for the attempt to specify with precision (or at least with brevity) things which are not precise is bound to lead to generalizations which are either vacuous or trite. On the other hand, to ignore the accretions of sentiment in favour solely of an account of modes of residence, inheritance, and whatever else can unequivocally be described, seems to me to leave the better part of what constitutes Mediterranean kinship unsaid, and consequently to leave a great deal of what constitutes Mediterranean societies unsaid.

Let me now turn to some few ethnographic observations, perhaps in themselves quite trivial, but which will, I think, illustrate the difficulties involved in studying Mediterranean kinship - at least I find them difficult to deal with.

When I arrived in Spartohori - one of the three villages on the tiny island (*pace* its name) of Meganisi, a dependency of the Ionian Island of Lefkas or Lefkada in Western Greece - I started to collect genealogies; or, if genealogies is too high-flown a word, since what I collected have no generational depth, and the Spartohorites themselves do not keep genealogies, I set about finding out who was related to whom and how. I did this partly as an act of faith - that something interesting might come of it; partly because I did not know what else to do and it seemed an anthropological sort of thing to be getting on with; but mostly because, whatever the intrinsic value of the exercise, I thought it not a bad idea to find out as quickly as possible who everybody was.

The task was not as easy as I had been educated to assume. First, politeness imposes in a Greek village roughly the same constraints on inquisitiveness that it does in our own society. One cannot expect to sit down for a drink with somebody and demand to be told the number of his siblings, the age of his spouse, and the extent of his cousinhood. At the very least it does not make for sparkling conversation and one is either a bit of an irritant or a bit of a bore. But, secondly, I have said 'roughly' the same constraints. In fact they are greater, for in rural Greece information is collected continually, but it is never collected innocently.⁴ When matters touch on oneself or one's own, evasion is an almost instinctive reflex. Lastly, and somewhat more specifically, if people seemed generally less than enthralled at the prospect of recounting all their numerous cousins, nephews and nieces (I do not mean to imply that such matters were unimportant; they were very important, but reference to them occurred in what we might call 'strategic circumstances'), or if they seemed less than enthusiastic about revealing details

⁴ On this point cf. J. du Boulay, *Portrait of a Greek Mountain Village*, *op. cit.*, and, by the same author, 'Lies, Mockery and Deceit', in J. Peristiany (ed.), *Mediterranean Family Structures*, Cambridge: Cambridge University Press [in association with the Social Research Centre, Cyprus] 1976.

concerning their immediate family, in some cases and with reference to some categories of relative (notably fathers and married brothers), responses were so dismissive as to make me think that what I was encountering was the particular avoidance of certain subjects rather than the generalized suspicion of all inquiries.

In the light of these difficulties I began to supplement increasingly indirect questions with direct observation - simply noticing who regularly associated with whom, and then checking on a presumed relationship when some conveniently casual opportunity arose. In the process I came across what seemed to me a rather odd phenomenon. The one exception to the general reluctance to talk spontaneously about family matters was to be found in connection with direct descendants. People were always ready to talk about their children, or rather about their sons. Old men continually regaled me with accounts of their offspring's virtues and of their worldly success (the two were not unrelated). And yet I suddenly realized that no amount of observation of public behaviour alone would ever have led me to suspect the existence of most father/son relationships. I would be told by some fond parent that his son, Georgos, was shortly to return from the ships; that he was an incomparable boy; and that certainly I should make his acquaintance. Days would pass. The old man would still be sitting in the *kapheneion* with his old cronies, or, more significantly, even with younger men; but no Georgos. 'Is your son back yet?' I would enquire. 'Yes, days ago,' would say the old man. And then, very quietly, 'That's him over there,' discreetly indicating someone at the other side of the room. 'Why don't you go across and talk to him?' But no effort was ever made actually to introduce me, and the father and son seemed to maintain a curious distance.

This sort of situation occurred so frequently that eventually I was led to ask a number of my old friends why it was that they never associated with their sons of whom, quite obviously, they were so proud. Interestingly, my assertion that they did not was always denied. On some occasions I persisted and adduced enough evidence of their avoidance to provoke some more elaborate reply. Generally this consisted of two remarks: inasmuch as they did not publicly associate with their sons, make a *parea*, a 'company', with them, then this was because (a) they saw quite enough of each other at home anyway, or (b) no doubt their sons preferred to form a *parea* with friends of their own age. All very reasonable too.

It is difficult for me to judge how far the first remark might be correct. Domestic arrangements in Spartohori vary greatly. Once, patrilocal residence and the importation of wives under the paternal roof where all lived as an extended family had been the rule. Nowadays neolocal residence upon marriage is the more common arrangement. But all sorts of permutations exist in-between: a single house from the outside internally divided into separate *spitia*; a new house built next door to and adjoining the paternal *spiti*; an entirely separate residence at the other end of the village. Degrees of commen-

sality also vary accordingly. Certainly when sons were unmarried and living with their parents, then no doubt they did see a lot of each other; but equally, there were many married sons who, so far as I could gather, saw precious little of their fathers even in the privacy of their respective homes.

The second comment, that sons preferred to associate in public with friends of their own age, was a very plausible evasion. On the whole it was true, and not surprising, that younger men had their younger friends and older men their older friends; but on Meganisi there is no radical generational split. Nor is there, as Margaret Kenna reports for 'Nisi' in the Aegean, an old men's cafe and a young men's cafe.⁵ In any case, what generational split one does find is between unmarried men and those who are married with a family, not simply between young and old on a purely age basis. Married men of thirty and married men of sixty or seventy would drink together happily, and this spans the ages of fathers and sons who nevertheless avoided each other. Indeed, when a group of younger men - say, in their thirties - decided really to celebrate, to make a *parea* and to pile the tables with beer (which frequently they did), then there very often was an older man with them, who appeared to be in every sense 'one of the boys', who laughed with them, joked with them, drank with them, danced with them, and who was their very intimate. Almost invariably this older man would be one of the company's fathers-in-law. The fathers, if they were there at all, would be at other tables. At the most they would look on indulgently. But they were never part of the proceedings. The contrast between the two, between parent and affine, between father and father-in-law, could hardly have been greater.

Now we can widen the context of this in the most general way. It is one of the commonplaces of Greek anthropology that the nuclear family, or the extended nuclear family, more simply the *spiti*, the house, containing a married couple, their children, and perhaps a grandparent or -parents, is the basic social, economic and moral unit of rural society. But for all the much-vaunted closeness of this unit, public exhibition of family solidarity is minimal. Indeed, external appearances would almost deny it; for however close and closed the family might be behind its doors (and certainly I am not denying this), in public it immediately fragments. Family unity is replaced by other orderings and groupings virtually as soon as the house's threshold is passed - and the more public the context, the greater the degree of family dispersal. Conventions of the public realm seem almost the converse of the private.

The pre-dawn caique from Meganisi to Lefkada provides a convenient setting. Families exit from their houses together. They even walk together through the night down the steep zig-zag road

⁵ M. Kenna, *Property and Ritual Relationships on a Greek Island*, Ph.D. thesis, University of Kent 1971.

cut into the face of the cliff on which Spartohori perches to the *molos*, the quay, some three or four hundred feet below. Passing families greet each other. But once the caique is reached, which, for a couple of hours becomes a sort of floating microcosm of the village, another ordering takes effect. Sexual division, of course, is primary. Even with fifty people packed into a fifteen-metre caique one never finds husband and wife together. The women congregate in huddled groups, knitting, gossiping, or being sea-sick; the men sit or stand together, talking, smoking cigarettes, or pacing from one end of the boat to the other. Little children pass between. But if the caique compresses the village into the confines of a few feet so that the two worlds of men and women, the *kapheneion* and the door-step, are revealed with fresh clarity as a result of their proximity, so too are other divisions. Brothers will congregate; brothers and brothers-in-law will congregate; fathers-in-law and sons-in-law will congregate, all as part of the general male *mêlée*. But, once again, one will never find father and son together. Somehow they contrive to be at opposite ends of the boat. Only when it docks in Lefkada will there be a reformation as the family reunites to go about its (commercial) business.

We might note briefly in passing that there is a sense in which the church contributes to these separations. In fact the Spartohorites are not church-goers and are staunchly anti-clerical. Only a handful of old women attend regular services. But when there are full or reasonably full congregations - at weddings, baptisms, or at Easter - then, in accordance with traditional Orthodox practice everywhere, men and women are separated in the body of the church, even entering by separate doors. Equally there tend to be separations within the sexes: young unmarried men, older married men, matrons and maidens, gather apart. Despite the fact that so much of Orthodox ritual, symbolism and morality is directed towards the maintenance and sanctity of the family, the church itself, belonging to the public sphere, is not a place of united worship. A family that stays together nevertheless prays separately.

Only on quite exceptional public occasions does the family present itself to the world as the unity it doubtless is: at the *panayiria*, the festivals celebrating one village saint or another, or at Christmas. Then the family is on show. In or outside the *kapheneion* generally reserved for men, families now sit in groups, each at its own table, the women dressed in their best, their children ranged around them, one family sending bottles of beer to another, but always through the medium of the waiter who names the donor to be acknowledged across the room. The men may become a little boisterous as drinking and dancing progress, but it seems that the more staid the proceedings manage to remain, the more people congratulate themselves afterwards. 'It was a very nice *panayiri*. All the families together. This is *politismos*, "civilisation".' I suspect that this attitude is in part a result of Spartohori's growing *embourgeoisement*.

Nevertheless, in a more low-key way, formality is of the essence of Spartohorian social life. This may not be immediately

evident, for it does not consist of pomp and circumstance. But, throughout, an undeniable etiquette is in force. The constitution of the *parea*, the group of friends drinking together, is a case in point. There are some seven places in Spartohori where one might drink, referred to simply as *ta magazia*, 'the shops', and in a village where, to be frank, there is not usually a great deal to do, sitting in the shops approaches for many a full-time occupation. Men casually drift from one to the other throughout the day. But not so casually. In fact, on entering a shop, only one of three things can happen: either there is an empty table, in which case one may sit down; or else those already seated at a table will cry '*katse na pioume*' ('sit down [with us] that we may drink'); or else, if all tables are occupied, even by one man, and no invitation is forthcoming, one must leave. I do not mean to imply that this last alternative creates a continual series of social *impasses*. Since people are always wandering in, wandering out, standing in doorways, rocking on their heels and walking in small circles as the day slowly passes, no obvious social embarrassment is caused (though in fact the degree to which one is or is not invited, greeted or not greeted, does enter into the never-ending computation of one's social worth). But what is certainly the case is that on no account may one simply sit down with others uninvited.

Such behaviour will not seem entirely strange to us. But in Spartohori the etiquette is carried further, or never relaxed. There are, after all, only about two hundred adult males permanently in the village, all of whom have known each other from birth, sometimes for seventy years; further, they may have been drinking together elsewhere not half an hour before. But on each occasion the invitation must be extended - and one of the resultant oddities is that one may enter a shop to find a dozen tables and a dozen men, one man at each table, all joined in mutual conversation, but everyone with, as it were, his inviolable space around him. It is this situation which makes so apparent the associations and avoidances of certain categories of kin. Father and son may well be at the same time in the same shop. They may even be engaged in the same conversation. But they will never be at same table. And if one analyses the composition of the larger *parees*, then they are very likely to consist of brothers, brothers-in-law, and sons-in-law and fathers-in-law.⁶

⁶ In the light of the discussion which followed the original presentation of this paper it is perhaps necessary to clarify a possible confusion. If a man will not make company with his father, then it follows that he cannot simultaneously make company with (a) his sister's husband and (b) his sister's husband's father-in-law, since that father-in-law will be his own father. He can, however, make company simultaneously with his sister's husband and his own father-in-law. Merely to put matters the other way round, he cannot simultaneously make company with his wife's brother and his own father-in-law, but he can simultaneously make company with his wife's brother and his wife's brother's father-in-law.

The fragmentation of the family in public, the etiquette by which it is apparent, forms the most general context for our purposes. But let us now consider what Davis emphasizes: the rules of residence and inheritance. The Spartohorites adhere to an arrangement quite familiar, though not absolutely standard, throughout Greece. Daughters receive dowries, usually monetary, at the time of their marriage; sons inherit the paternal property by equal division at the time of their father's death, of which the most important element is the family house. Furthermore, as I have mentioned, in the past sons brought their wives under the paternal roof until the time of their father's death and the division of the estate. This practice is dying out as neolocal residence on marriage becomes more the norm, and it was always, I think, a practice modified by the exigencies of poverty and emigration. But, as Davis suggests, the arrangement is a recipe for tension. Sons tended to remain economically dependent on aged or ageing fathers well into the time when their own families were making economic demands on their resources; they remained to an extent under his authority well into the time when, as grown men, their desire to assert their independence was imperative. Brothers, equally, found themselves with conflicting interests as their respective families grew. Resentment bred.

The effects of these traditional rules of inheritance are, I think, well documented. But before one asserts, following Davis, that such rules are always the crux of the matter, one should note that they have the effect they do only because they are at odds with something else less tangible but equally powerful (and of which, to be fair, Davis is fully aware): the desire of each man to stand independently at the head of his own household in a community of isolated individuals and equals where everyone is striving to be more equal than anyone else. The rules of inheritance and residence hold together by force what otherwise would fragment, and does fragment, as soon as the constraints are removed. The situation is paradoxical because each 'nuclear' family, whose integrity is writ large by accepted morality, contains within it the seeds of other nuclear families which must burst it asunder. The son is father to the man.

But one must not see this simply in terms of a conflict between 'practice' on the one hand and 'ideology' or 'values' on the other, as if the former were merely a doomed attempt to embody the latter. Values themselves are contradictory; or at least, since 'values' cover a multitude of sins, one would want to distinguish between values in the sense of what is held to be right, proper, and good, and values in the sense of what continually inform a person's apperception and expectancy of the world. It is right and proper that fathers and sons should be close and cherish each other. 'To love someone like a son' is a standard simile. An uncle may say it of his nephew. Similarly, brothers are held to be the paradigm of the affectionate masculine relationship of equality. '*Imaste philoi; imaste adherphia*,' 'We are friends; we are brothers,' is a standard coupling. And yet on other occasions men have little compunction about rueing the

absence of daughters, for a daughter will always care for you, while as for sons - what are they good for? Only the house and the name. And while brothers do fulfil their role as mutual friends and companions when younger, everyone waits for them to fall out. A screaming match takes place in the village over the infringement of property, and an old woman simply shrugs her shoulders and comments, 'Hah, brothers again!'

At this point it is worth recalling that all my informants at first denied that they did not keep company with their sons, for whatever it is that the rules of residence and inheritance create, it is not a set of ritualized, rule-governed actions or unambiguous concepts of social roles and requirements. Rather, they create a set of tensions and predispositions - sentiments which find their concrete expression within the possibilities supplied by the norms of good conduct or social etiquette. This being the case, the old men were not necessarily lying to me. There was no rule, there is no rule, concealed from me which states that father and son must not sit together or drink together or associate in public together. Rather, they feel a certain unease in each other's presence, an unease borne of a contradiction which, in public, places them simultaneously in both a hierarchial and an equal relationship: hierarchial in that they are father and son, equal in that they are both adult males of the village. This sensitivity to the situation, which, like a sensitivity to language, always exceeds the comprehension of its underlying structure or grammar, drives them to adopt a stance, or perhaps we should say a chair, whose isolation is already sanctioned by the daily practices of social intercourse. The assertion 'You never make company with your sons' could thus be denied, because there was no explicit formulation, no 'collective representation', which matched the generalization I had arrived at by the process of observation. If reflection on what I had presented to them as an account of their behaviour could in the end result in a modified form of assent, it was nevertheless modified because it might seem to challenge what was a deeply held moral idea - that fathers and sons should enjoy a close relationship. After all, the counter-instances of amity and cooperation within the household and even, on certain occasions, the public presentation of the family in its unity, could always and truthfully be adduced, even if, in other cases, tensions, quarrels and bad-feeling were real enough.

The existence of observable regularities on the one hand, and on the other hand their failure to congeal into a set of codified practices or explicit ideological statements of the sort we are pleased to call 'collective representations' is what, to my mind, makes the study of Mediterranean kinship both difficult and uncertain. For these regularities are but the manifestations of certain collectively-held and collectively-endorsed emotions, persuasions, predispositions, which, however much they may be nurtured within a particular social structure, are neither its necessary consequences nor, as organizing or basic 'concepts', its integral parts. The question of father and son, for example,

can be taken the other way around from the point of view of the latter. If traditionally, and as in so many parts of Greece, young men on Meganisi do not smoke in front of their fathers even within the house, nowadays many are beginning to do so, asserting all the time that they 'respect' their fathers and that it is precisely the closeness of their relationship with their father and his confidence in their affection and respect which allows them to do so. In the relationships between kin one approaches a morality which, like any morality, manifests itself in behaviour; but one does not encounter a series of rules which categorically determine the precise forms of that behaviour. If a father is offended by his son's smoking, then no good son will smoke in his presence; but if he is not, then smoking is permitted. What remains basic is the question of respect. And further, while there may be a definable 'concept' of respect, it is not the concept, but the sentiment, which informs behaviour.

Such sentiments are, as I have been ready to admit, nurtured within the context of specific social structures, within, for example, the rules of residence and inheritance. The degree of their autonomy, however, might be considerable. One can at least argue that they are capable of persisting historically beyond the desuetude of those rules. As I have already mentioned, neolocal residence upon marriage is now the norm; furthermore, except in very rare instances, on Meganisi sons are no longer economically dependent on their fathers or on their inheritances. They are now wealthy in their own right, and wealthy from an early age, as a result of their employment in shipping. It is their fathers, the generation of agriculturalists and fishermen, who are the poor, and very few young Meganisiotes have any intention of returning to their parents' way of life. Such an economic shift has its social correlates, notably a remarkable drop in the age of marriage, which, in turn, might be construed as involving a change in the nature of the relationship between father and son. For it is not only the case that men are now financially secure enough to marry young, but also that they feel the right to choose their own bride rather than to attend their father's choice. There is little doubt that some old men thus feel their authority to have been eroded and that some young men are frankly embarrassed by their fathers, but 'respect' for one's father, along with the habits of distance that I have described, still remain the idiom of the relationship. And if at times an element of hypocrisy on the one side and of self-deception on the other side have crept into that relationship, one might hazard that respect is somewhat easier to feign than love - which, of course, might always have been the case.

But for all that, it is not difficult to construct a quite plausible and neat explanation in terms of traditional social structure and the rules of residence and inheritance for the contrasting relationship of a man and his father-in-law. First, except in slightly abnormal instances, the case of the '*sogambros*', the man who marries an heiress, a brotherless daughter, and works her land and resides in her family's house, men were neither

economically dependent on nor under the authority of their fathers-in-law. Actually, during the nineteenth century and earlier part of this century the number of 'sogambroi must have been quite high, for one of the ways in which Meganisi's population built up was the gradual settlement there of shepherds from the mountain villages of Lefkada who seasonally transferred their flocks to the little island for winter pasturage. Such settlements seem usually to have involved a marriage. But in most cases of 'sogambroi which I know of, the father of the girl (or girls) was already dead by the time of the marriage. In any case it seems unlikely that the number of 'sogambroi would ever have been sufficient to alter the prevalent pattern of inheritance and residence, which was, of course, the importation of brides into the paternal household or, to put it another way, the exchange of women between agnatic households. Further, not only was a man not economically dependent on his father-in-law for his long-term wealth, his inheritance; he was also the recipient of an immediate prestation from his father-in-law: to wit, a dowry.

But there are, I think, factors more important than this, though still pertaining to the traditional 'social structure' of the village; for it would be a somewhat shaky general sociological law that proposed inheritance from a father to engender a distanced relationship while the immediate effect of receiving a dowry would be to create a relationship of affectionate and familiar amity. After all, why should not one feel beholden to one's father-in-law for both dowry and daughter, that feeling also being translated into a public attitude of respect? Rather, I think, we must reconsider the independence, economic and moral, of each male-headed *spiti* or household.

All Spartohori's village ritual of marriage, from the wedding songs and the pattern of after-service feasting to the bride's formal reception at the door of her new house by her father- and mother-in-law (a somewhat faked procedure these days given the prevalence of neolocal residence), not to mention the weeping, spontaneous enough, of the bride's mother, stresses the bride's departure from one closely-knit world, that of her parents' *spiti*, and her entry into a new and strange one, that of her husband's family. In fact the whole shift might be considered rather more 'conceptual' than actual, since the move might be no more than twenty-five yards down the road, and daughters continue to have a very close relationship with their natal family, both parents and siblings. Younger sisters turn out to be ideal baby-minders, and, as I have already mentioned, it is one of the clichés of at least one genre of discourse that daughters are better than sons because they continue to care for their parents while sons often do not. That mother and daughter never become estranged is a firm belief, and true also - for all I know. Indeed kinship reckoning is thoroughly bilateral, and with respect to the terminology, more so than our own. In practice, grandparents dote on and see as much of their daughters' children as of their sons' children, and the purpose of the dowry is often explained in terms of a benefit to one's daughter's children.

There is perhaps something of a contradiction between the bilaterality of Greek kinship reckoning and the agnatic composition of the individual *spitia*. Nevertheless, there can be no doubt that marriage is seen as representing the departure of a woman from one household and her entry into a new and 'foreign' one.

Taking that situation at face value, one could, in the best structural-functionalist tradition, see the amity between father-in-law and son-in-law as an attempt to deny, or at least mitigate, the loss that the one household and the gain that the other household have incurred. Their friendship forms, as it were, a bridge between their respective and self-contained families, between two of those independent economic and moral unities which Peristiany has termed 'social isolates moving in a field of common values'.⁷ The antagonism which always exists not far beneath the surface between one family and another, between one *spiti* and another, is ostentatiously suspended by the formation of a relationship which, though it overlays one of kinship (for the respective parties are, after all, affines, *sympetheroi*), nevertheless must take on an additional social form in order to unite entities whose isolation in general takes little account of the extensiveness of bilateral kinship: namely, the form of 'friendship'.

Here we should note that despite the manifold importance of kinship in many contexts, there is a strong and often articulated feeling that, at least as soon as one moves beyond the confines of the household, 'friends' are better than relatives. This lauding of friendship over and above kinship has as its basis the fact that one can after all *choose* one's friends, whereas one is stuck with one's relatives. However, if only to disconcert the literal-minded, the idiom of kinship shortly reappears. I discovered that two old men, fast friends throughout their lives, were in fact related. I reproached them for never having mentioned this fact, and was told that the omission was entirely justifiable since what was important was that they were friends, not that they were in some way related. 'Why, we're friends,' they said. 'Indeed, we're brothers.' By a permutation of the logic of this discourse, one could perhaps claim that affines, fathers-in-law and sons-in-law, can, owing to the weak nature of their actual kinship connection (for it extends beyond the household), thus approach through 'friendship' that ideal egalitarian relationship of 'brotherhood', whereas fathers and sons are doomed to be what they are: i.e. you can be a brother to your wife's father, but you're always just a son to your dad. Admittedly I never did hear things taken quite so far, but that a larger drinking *parea*, it will be recalled, did often comprise precisely brothers, brothers-in-law, and a father-in-law.

Indeed, in this context we might note the usage of some further extended or honorific kinship terms. For if it would be

⁷ J.G. Peristiany, 'Honour and Shame in a Cypriot Village', in J.G. Peristiany (ed.), *Honour and Shame*, London: Weidenfeld and Nicolson 1965, p. 179.

frankly impertinent to greet a stranger as a brother (that would be to imply indecent familiarity), one can greet a stranger, as I myself was greeted on a number of occasions, with the words 'koumbare mou' or 'sympethere mou', 'my godfather' or 'my affine' - both kinship terms of course, but both implying or proffering friendship, and implying or proffering friendship not, I think, only as a reflection of the actual state of affairs, i.e. that one usually is on good terms with a god-parent or affine, but, to reverse the logic, because both god-parenthood and affinity are by definition contractual relationships, relationships of choice; and in Greece at least, one neither baptizes nor marries one's enemies. In fact it was always said that, in the days before young men and women made up their own minds about such matters (something dubiously stressed to me in the interests of 'modernism'), it was the father-in-law who chose his son-in-law. They were the contracting parties. So if by definition one gave one's daughter to a 'friend', even nowadays, or in the face of what I suspect always to have been the truth of the matter - that women played the major role in match-making - one might still be well-advised to be a friend to one's daughter's husband. Indeed, in a society where there are no systematic marriages and no prescribed marriage partners, but where marriage is still deemed to involve more than the romance of a couple, affinity is the interface *par excellence* between friendship and kinship.

Such then, I hope, would be a plausible account of the relationship in traditional anthropological terms. But in the end I cannot help wondering if there is not yet more in play: further murky areas into which we are advised not to stray and about which, in any case, my villagers would not conceivably talk - the feelings that a man might have towards the person who sleeps with his daughter; even perhaps his own feelings towards the daughter herself. Less dangerously, our own saying at marriage might be even more apt for the (Meganisiote) Greeks: 'Don't think of it as losing a daughter, but of gaining a son'; that a man might enjoy with his son-in-law precisely the relationship which is at once enjoined and denied him with his son.

ROGER JUST

BYWAYS IN OXFORD ANTHROPOLOGY

Selections from the Minutes of the
Oxford University Anthropological Society

1919-1920: A Diet of Skulls, Women and Cannibals

Two current topics in anthropology, cannibalism and women, received an early airing in the history of the Anthropological Society. The minutes of the Society show that the debates had a slightly different flavour from today, as can be seen in a paper called 'Woman's Place in Nature' (October 30, 1919) in which Prof. J. L. Myres illuminated

... the ways in which economic give-and-take is affected for men and women respectively by regional controls of food supply and other external foods [factors?]. The reciprocal relationships of mutual service being in turn contrasted by the unreciprocal relationship involved in the perpetuation of the race.

The topic of cannibalism raised an intriguing idea on the applications of anthropology. The following minutes record a paper by Mr. Pearson Chinnery on February 5, 1920.

The paper was given with the object of proving that in certain districts of New Guinea functions such as cannibalism and head-hunting and other forms of homicide which formed part of the social and religious fabric of native life had been modified by anthropological methods which offered substitutes equally effective in perpetuating the institutions of the natives and bringing them in harmony with the standards of government.

One is relieved to find that such useful work was actively encouraged by the Australian Government but sadly one can only speculate on the nature of cannibalistic substitutes.

Cranial measurements were still the centre of heated discussion on November 5, 1920. Prof. F. G. Parsons argued in favour of 'social inheritance' in determining head form, as opposed to Prof. Flinders Petrie's 'geographical location'. In making his point Prof. Parsons gave this account of British heads.

He showed that the so-called long-headed north contained short-headed Lapps, and that the round-headed Alpine race had extended its head shape all over Germany, and in the last two hundred years had by peaceful penetration and the

power of working and breeding under conditions which other races would not tolerate, raised the cephalic index of the British Isles from under 77 to nearly 79.

This diet of skulls, women and cannibals swelled membership to 154 by the Michaelmas Term of 1920.

MIKE HITCHCOCK
Secretary, 1979-80
Oxf. Univ. Anthropol. Soc.



REVIEW ARTICLE

RITUAL AND EMOTION

GILBERT LEWIS, *Day of Shining Red: An Essay on Understanding Ritual*, Cambridge: Cambridge University Press 1980 [Cambridge Studies in Social Anthropology, General Editor Jack Goody, No. 27]. xvi, 232 pp. £10.50.

In *Art and Illusion* Ernst Gombrich concludes the chapter called 'Pygmalion's Power' with a photograph of a startlingly realistic Renaissance bronze, Riccio's *Box in the Shape of a Crab*. He writes:

If I had it ... on my desk, I might be tempted to play with it, to poke it with my pen, or to warn a child, most unpsychologically, not to touch any paper on my desk or the crab would bite it On the desk, in short, this object would belong to the species crab, subspecies bronze crab. As I contemplate it in its glass case, my reaction is different. I think of certain trends in Renaissance realism which lead to Palissy and his *style rustique*. The object belongs to the species Renaissance bronzes, subspecies bronzes representing crabs.¹

Gombrich intends this to illustrate the historical transformation from 'the archaic magic of image-making' to the subtler pleasures of modern artistic contemplation. But it is perhaps a better illustration of the appropriation of subject-matter in the human sciences: desk to glass case, bronze crab to bronze representing crab, living object to object of study. What in the midst of life bore significance, however mute and undoubted by reflection, must be laboriously reconstituted in quite a different medium, one in which the object enjoys none of its original transparent and intelligible immediacy. Yet the scholarly representation - this is the perennial problem - must in some sense remain faithful to the original, and we must have some way of judging whether it is successful in doing so.

Anthropologists encounter this problem most poignantly compounded in the attempt to understand ritual. The comparison with art history is profoundly appropriate: the art historian brings

¹ Ernst Gombrich, *Art and Illusion*, London: Phaidon 1960, p.98.

to the object a trained sensibility, and the raw material from which he works is not so much the object itself as the perceptions arising from the engagement of this sensibility with the object. The labour comes in achieving the sensibility, in learning the idiom which the object's creator and his public shared. By Gilbert Lewis' excellent account, an analogous sensibility must be achieved by the anthropologist. He writes:

... understanding, in the context of another culture, depends on learning. Learning, not intuition: the anthropologist is more a true connoisseur as he learns more about the range of expressive modes and media used in the ritual of a society, the ritual styles and genres, and how they relate to other aspects of life and experience in that society. (p. 28.)

In using such words as 'expressive' and 'connoisseur', in fact, Lewis wishes to argue for more than a loose analogy between anthropology and aesthetic scholarship. He wishes to show that, like the art historian and literary critic, the anthropologist discovers and demonstrates elements in ritual and symbolism which are implicit and not discursively articulated by the practitioners. Symbols may be straightforward and unambiguous, but they may also be mysterious, evoking deeply held but unspoken values. And in the latter case the anthropologist can gloss them, 'just as we could convey something about the identity of a perfume if we paid it close attention and had skill with words' (p. 24). It is even possible to demand rigour of this enterprise.

Furthermore, in insisting that ritual is more like expression than like communication, Lewis accepts gladly that a good deal of ritual has to do with the expression of sentiments and emotions. On his showing there is good reason for including these dark matters fully within the anthropologist's province. For so long as anthropologists affirm their close ties with scholars of the arts they have a special expertise in the elucidation of culturally explicable feeling. In putting a rather stronger defence of this view than Lewis' own, I will imply in this review article that he in fact does not go far enough: it is not merely in ritual, but throughout social life, that anthropologists can, should, indeed often do, elucidate the forms and significance of emotion.

One extreme view which Lewis rejects would prescribe that the explanation of symbolism cannot go beyond what people say about it. Of course, in the light of the practice of ethnographers and the uncertainty and negotiability of ritual symbolism in our own lives (the bestowal of a wedding ring, for example), it might be asked how one could seriously hold that symbolism is explained merely by recording statements about it. But the view has been seriously put forward, and it is not as simple as I have depicted it. Lewis quotes Nadel as an exemplar:

Uncomprehended symbols have no part in social inquiry, their social effectiveness lies in their capacity to indicate; and if they indicate nothing to the actors

they are from our point of view, irrelevant, and indeed no longer symbols (whatever their significance for the psychologist or psychoanalyst).²

This statement can be paraphrased, adding information from the context in which it appears, as follows. Ritual symbols refer directly; they point to, indicate, certain convictions about the world, its nature, and the sort of effective action which may be taken in it. They replicate discursive beliefs. Symbols must therefore in their nature be clear, explicit, and conscious, and since they are so they serve this deictic function for anthropologists as well as for the people themselves. The uncomprehended, the ambiguous, the mysterious - one cannot miss the pejorative - is meat only for psychologists and their ilk.

On further reading, in fact, Nadel seems to treat ritual symbolism as discursive beliefs with a little emotion thrown in for effect. I do not think it would be an exaggeration to say that, in this view, one would treat a performance of Beethoven's Ninth Symphony as a humanist tract set to music. We may dismiss this as wholly inadequate, but Nadel's general statement still possesses force and continuing interest, for quite another reason: because it posits a division of labour between sociology and psychology. This position, I can confidently testify from my observation among British anthropologists, is alive and espoused fervently by many practitioners in the United Kingdom today.

There are sound and persuasive reasons why it should be so. Evans-Pritchard put the case in a very illuminating way:

A man on trial for a crime is found guilty by twelve jurymen and is sentenced by a judge to be punished. The facts of sociological significance are here the existence of a law, the various legal institutions and procedures brought into play by a breach of it, and the action of the political society through its representatives in punishing the criminal. Throughout the process the thoughts and feelings of the accused, the jurymen, and the judge would be found to vary in kind and degree and at different times ... but these variations would not be of concern, or at any rate of immediate concern, to the social anthropologist On the other hand, to the psychologist, who is studying individuals, the feelings, motives, opinions, and so forth, of the actors are of first importance and the legal procedures and processes of secondary interest. This essential difference between social anthropology and psychology is the *pons asinorum* in the learning of social anthropology.³

² S.F. Nadel, *Nupe Religion*, London: Routledge and Kegan Paul 1954, p. 108.

³ E.E. Evans-Pritchard, *Social Anthropology*, London: Faber and Faber 1951, p. 46.

We can agree, I think, that this does represent, however roughly, the images of the two disciplines in the anthropologists' eyes; and it is certainly the case that anthropologists have had to learn to be careful not to assume too much about what goes on in other people's heads.

But in fact if this division of labour were applied rigorously it would consign to the inner darkness of unreadable thoughts a good deal that is both necessary and interesting to sociology. Let us retain the analogy of the trial. Is it not the case that criminal trials in our own society frequently direct themselves to determining 'feeling, motive, opinions' and the like? Is it not a sociologist's concern, not a psychologist's, to monitor and explain motive and opinion among judges, for example, which determine sentencing practice? And does not much of the conduct - as opposed to procedural rules - in a criminal trial address itself to swaying the feelings, motives, and opinions of the jury? A straightforward account of institutions and procedures, in other words, without an account of the opinions and sentiments which accompany them, or the emotions which they discipline, would be merely legal and not yet properly social anthropological.

I will return to these arguments about the trial shortly. But if for the moment we drop that analogy and use another, that of the healing ritual, it becomes obvious that motives, feelings, and sentiments are integral to what the anthropologist studies. The same is true for occasions whose procedure is less easy to specify: feuds, disputes, death, sexual intercourse. Mauss and Durkheim were prepared to admit feeling to social life, indeed to ritual in particular, under the admittedly curious title of *effervescence*, agitation; why should we impoverish ourselves by ignoring their precedent?

Nor do we so impoverish ourselves: whatever social anthropologists say they do or should do, in fact their practice is, must be, to account for feelings, motives and sentiment. Let us consider the context of Nadel's dictum. Despite his confidence that symbols are explicit and purely cognitive, he does in practice, in writing about Nupe religion, provide a good deal of information and interpretation concerning their implicit and emotional effects, and this interpretation is clearly vital to our understanding. He writes of the symbols - which range in his account from invocations through ritual postures to musical instruments - that they

produce a certain mood, made up of emotional tensions and satisfactions, and of given expectations. Upon this mood fall the words which voice the aims of the ceremonial, so that they gain weight through the very context in which they are spoken or sung; and this mood is further effective as any stirring experience is effective, in offering stimulation and catharsis, in providing sensory pleasure, and, ultimately, in leaving a mark upon experience. (*op. cit.*, p. 106)

This certainly displays a psychology, a view of how minds work,

though it is by no means either academic experimental psychology or psychoanalysis. It resembles aesthetic scholarship, and is drawn from Nadel's sensibility as one present and engaged in witnessing and exploring the range of a genre, Nupe ceremony. We may, of course, wish to revise his thought or query details in his presentation, but what is important is that this aspect of the explanation is both illuminating and indispensable. Without it we would be forever puzzled at the Nupe's choice to present their activities and cognitive beliefs in this peculiar package. Nadel would have failed to account for the immediately self-explanatory character that, I suspect, both anthropologists and participants experience, in their different ways, in ritual.

Why, then, do anthropologists do one thing in practice and in theory say they should do another? One answer is that they hold in theory a mistaken view about the nature of sentiment. In their programmatic statements Nadel and Evans-Pritchard locate sentiment wholly within individuals, and treat it as a private and fundamentally unknowable matter. They identify the study of sentiment, in other words, with methodological individualism, the assumption that human phenomena are ultimately explicable by reference to individuals' experience and feelings alone. They implicitly agree with Steven Lukes' reasoning in rejecting methodological individualism. Lukes writes:

... many features of social phenomena are observable (e.g. the procedure of a court) while many features of individuals are not (e.g. intentions). Both individual and social phenomena have observable and non-observable features. If [methodological individualism] means that individual phenomena are easy to understand, while social phenomena are not ... this is highly implausible: compare the procedure of the court with the motives of the criminal.⁴

But the opposition between the rules of the court and the criminal's motives is misleading. The rules, to be sure, must be observable and easily understood, for otherwise they would not serve their function. The motives of the criminal, on the other hand, are not observable; indeed they lie in the past with the act to which they were linked. The court nevertheless assumes that the relevant details of the act with the relevant details of the motive can be retrieved, that they can be made subject to public discovery, and that they are, to that extent, easy to understand. (They may of course be disputed in court, but so may parts of the court proceeding itself.) Furthermore, displays of emotion in court on the part of witnesses, for example, may be taken straightforwardly to reveal intentions and motives, such as hostility to the court or the intention to lie to save one's skin. Among the conventions which allow the court to proceed at

⁴ S. Lukes, *Individualism*, Oxford: Basil Blackwell 1973, p. 117.

all are those governing the expression - and the suppression - of emotions.

For the greater part of emotion - its occasions, its expression, its significance - is conventional, learned, and to that extent transparent and inferrable, though only by one who knows the culture well. Indeed the expression of emotion may require anthropological comment precisely because it is easy to understand in a particular sense: it is implicit, interpreted without articulate reasoning, too obvious to require such reasoning. The distinction between private, unreadable feelings, fit only for psychologists, and explicit public rules, to be studied by anthropologists, is a false one. Emotion and sentiment play a public and commonly intelligible part in social life, and to that extent we can and do study it.⁵

This is not to say that anthropologists possess a universal key, an algorithm, for determining the significance, especially the emotional significance, of expression in ritual. Lewis laboriously and to great effect rejects this view in the course of solving the thorny ethnographic problem which provides most of the illustrative material in his essay. The problem was this: the men of the Gnau, a people of the West Sepik District of New Guinea among whom he worked, cut their penises to make them bleed on certain ritual occasions. Other peoples in the area do so as well, and among them penis-bleeding is explicitly equated with menstruation and with ridding oneself of dangerous sexual pollution. The Gnau in their statements, however, do not equate it with menstruation at all. Yet the difficulty could not be solved simply by direct questioning, for the Gnau were unable to substitute an unequivocal explanation of their own for that of menstruation. For all that penis-bleeding was unmistakably of profound significance to them, their understanding of it was implicit and not clearly articulated. What was to prevent Lewis from producing a tempting and tidy explanation, substantiated by the Gnau's neighbours, to the effect that Gnau do *really* equate penis-bleeding with menstruation?

⁵ It has been put to me that, whereas the explicit and transparent demonstration of motive, honesty, is valued in American culture, the dissimulation of motive in favour of smooth and predictable social interaction, tact, is valued in British middle-class culture. The latter, it has been suggested, might be explained by the experience of the English Civil War, which was settled only at the expense of learning to hide one's religious sentiments in favour of public order. If accepted this would explain the taste among many British anthropologists for sharply dividing an inner and unexplained private world from an outer world of rule-bound behaviour. It would also explain the cold reception psychoanalysis has received in Britain. Ioan Lewis has puzzled over this in the introduction to *Symbols and Sentiments*, London: Academic Press 1977.

A great deal. Lewis compared the actions of the ritual, the verbal and material items which accompanied them, and the symbols and metaphors inherent in those actions and phrases with their appearance elsewhere in Gnau life. This is a slow and painstaking business, for neither the provenance nor the placement of each piece of evidence within the constellation of other evidence can be prescribed or predicted. He set the penis-bleeding, in other words, in the broadest possible context within Gnau culture. In the event his explanation veered steadily and decisively away from the specifically sexual, reproductive, and fearful connotations which penis-bleeding has among the other peoples. The Gnau practice has to do, not with gender and sexuality, but with the fostering of growth and maturation in individuals of both sexes, and with the application of ritual heat to that end. Rules for the application of penis blood do not have to do with the avoidance of sexual pollution, but with the orderly and caring bestowal of elders' maturing power on juniors.

Lewis' argument is convincing because it possesses, I think, a rigour which consists of two elements inextricably mingled. The first is that he has asked the right question, for in the final analysis it is a problem of the deployment of emotional accents within Gnau culture. Referring to Gombrich, he writes: 'the problem phrased more abstractly is to get classifier and modifier right: is *Don Quixote* a comic novel with tragic overtones, or a tragic novel with comic aspects?' (p. 132.) Given that handling the penis must have its sexual implications, is sexuality a major or minor theme in penis-bleeding? It is not the mere amassing of detail to support a case which renders an answer to such a question credible, for we are used to swallowing whole the most elaborate and alien systems of thought which, considered purely in themselves, could as well be attached to one set of emotional emphases and values as to another. As Lewis writes: 'When we consider other people's symbols from the outside, our own beliefs uninvolved, make play with exotic material and images, we are liable to miss their true seriousness.' (p. 198.) Thus, for example, Lewis shows that the sister's son who receives the mother's brother's penis blood is likened to a creature sheltering in a hole, a plant nourished by ashes, and the moon led into the sky by the evening star. What unifies these bizarrely different images is the emotional emphasis on nourishment and protection which the mother's brother offers his nephew. It is this 'true seriousness', like the 'experience of feelings which one's own life involves', that persuades us that Lewis' argument is securely anchored in Gnau life.

This might be thought to be an argument for an intuitive and universal reading of symbolism, but the second element of Lewis' rigour, his insistence that symbols be read from their cultural context, implies to the contrary that symbols are quite culture-specific. Indeed, he sees in the context-bound nature of symbolic expression an argument against the view that 'meanings are intrinsic to the actions themselves'. Rather (again following Gombrich) he argues that 'expression, and its interpretation,

depends on choice and selection within a particular context and structure' (p. 5). There are no grounds, in other words, for a theory that ritual symbols are naturally motivated, that there is an 'intuitive natural resonance' between expressions and what they express. Symbols are chosen within a spectrum of possibilities, and the breadth of the spectrum and the allocation of values within it cannot be pre-judged. Even tears, Lewis argues (pp.27-28), require collateral evidence, some knowledge of the alternatives, to be interpreted. There are no symbols with an unequivocal and universally intelligible psychological meaning - tears or laughter, penis-like tubers or vulva-like fruits, mandalas or hermaphrodites - and to that extent the elucidation of sentiment and its expression must lie with the anthropologist, not the experimental psychologist or psychoanalyst.

And by extension this is also an argument against the simply understood and concrete world by which symbolism is measured by materialists. (The case against them, and for the primacy of culture and its interpretation, has been put quite effectively by Marshall Sahlins in *Culture and Practical Reason*.⁶) They might retort, of course, that such a position as Lewis' and Gombrich's leads to complete cultural relativism: without some common intuitively understandable significance of a concrete sort, they would say, we could never find the key to others' symbolism. But Lewis and Gombrich must not be understood to espouse such a view. They accept that in the case of the human body, for example, there is a weak sense in which symbolism is universal, in that once we know what a gesture means, we can see that it does so clearly. We can see, in other words, its 'true seriousness'. They accept the potential intelligibility among all humans without which anthropology, the human sciences, and common life would be impossible. What they reject is a strong sense of interpretation: that expressions reveal an unambiguous material or psychological world apart from their cultural context.

I could go on expanding and approving Lewis' arguments, but I hope I have made my point: Lewis quite rightly denies various kinds of reductionism, views that ritual or symbolism can be simply, rather than densely, explained. Perhaps his greatest efforts are directed against the view that ritual and symbolism are a kind of communication. Nor is ritual simply a way of making things happen. Of the three views - that ritual is communicative, instrumental, or expressive - it is most useful to regard it as expressive, like theatre; for by so doing one includes at the very beginning the most difficult yet most interesting features. And this decision opens the discussion onto some of Lewis' most incisive insights. He argues in detail that what is necessarily clear, unambiguous, and rule-like about ritual is how to do it, whereas the meaning may well be ambiguous, polysemous, and myste-

⁶ M. Sahlins, *Culture and Practical Reason*, Chicago: University of Chicago Press 1976.

rious: ambiguous, in that it may offer two or more meanings simultaneously; polysemous, in that it may have discrete meanings to discrete categories of participants; and mysterious, in that it may be an invitation to contemplate the horizons of existence. He demonstrates these points decisively, through both abstract argument and the Gnau material, and I commend his work as a warning and an encouragement to field workers to face squarely the inherent complexity of rendering ritual into discursive prose.

MICHAEL CARRITHERS

REVIEW ARTICLE

ON THE CHANGING CENTRE OF GRAVITY IN POPULATION STUDIES

DAVID S. KLEINMAN, *Human Adaptation and Population Growth: A Non-Malthusian Perspective*, Montclair: Allanheld Osmun 1980. xiii, 281 pp., Index. \$23.50.

The study of population has long been organized by the idea of limits. Nowadays these are generally considered to be of two kinds. The first are analytic and abstract, and comprise the study of the formal properties of populations as a species of mathematical regularity. The second are a set of supposed regular material relations between population and resources. The two are, however, more commonly united in a pattern of reasoning, part qualitative and part quantitative, in which the properties of social groups are identified with their aggregate properties. English and Continental writers had experimented with social arithmetics for many years before Malthus made this dual idea of limitation the centre-piece of his *Essay on Population*. Henceforth it has been accepted that the mathematical limits of population ultimately circumscribe social possibility. Debate has never ceased, however, over the processes which regulate the relative growth of populations and economies, and over whether the question of ultimate limits is of practical as well as analytical importance.

As Dr. Kleinman remarks, Malthus's theory was basically about mortality. Poverty, misery and vice act as 'checks' on the growth of populations, chiefly through infant mortality; to remove these limits is only to delay the time at which a much greater and more catastrophic limit, that of the capacity of land to feed people, comes into force.

At a certain level the idea of a carrying capacity expresses merely an arithmetic of common sense: if there is only so much food, then you can only feed so many people. What Malthus noted was that this trivial truth could be used to bring into systematic relation a great many aspects of society, including relations of labour and capital, marriage and family structure, poor law relief, morality, and the invisible hand of divine and economic rationality. His is a 'total' argument, in the Maussian sense, and its systematic properties owe largely to his ability to express political, moral, economic, religious, familial and other

values all in terms of a single mechanism: a set of functional¹ relations between delimited series of population and resources.

Thus, Malthus's contribution was to base his theory on an isomorphism of material and mathematical limits, and to base the application of this theory on mortality, as a way of describing society as a totality. Obviously not all social theory, even of the partly mathematical varieties, may be traced to Malthus. And yet the elements of his theory recurred in the course of the nineteenth century, in ways that plainly influenced the formation of sociology.

An example of this was the reinterpretation of the perennial theme of factors contributing to the relative life or death, health or illness of society. Debate over Malthus's theory was at the centre of the reform movements of the first half of the century; the condition and ultimate significance of the poor was first the object of poor law inquiry and reform, and later a problem of vital statistics and sanitary reform. Where mortality, as a check upon the increasing numbers of poor, was for Malthus a regrettable but inevitable check upon social decline, its measure in the hands of later political economists, physicians and vital statisticians became the index of collective health and a guide to social improvement. Mortality provided a coherent set of limits within which a broad range of social problems could be organized and described. Malthus's pattern of reasoning about mortality, numerical indices and society was kept, even while conclusions opposite to his were advocated. In the 1850's and 1860's the first English movement to adopt the label 'social science' represented a hotch-potch of interests, of which political economy and vital statistics were the most highly developed. It was a political economist sympathetic to Malthus's method, Mill, who forcibly called British attention to Comte's writings.

In the same period, Darwin and Spencer drew directly upon Malthus's quantitative ideas about the collective significance of mortality, and their interpretations were taken up, in turn, in Social Darwinism, eugenics and comparative history. Even at the end of the century, Durkheim's *oeuvre* begins with the political economists' central problem of the causes of the division of labour, in which population pressure acts as a critical limit. Fundamental distinctions laid down in the *Rules of the Sociological Method*, for example, that numerical averages provide the best method of distinguishing 'normal' and 'pathological' social states, or that all aspects of social life have both 'physiological' and 'morphological' (we would now probably refer to 'quantitative' and 'qualitative') properties, are later glosses on the role of mathematical-material isomorphisms according to a vital imagery of total social description.

¹ I have in mind here the mathematical concept, i.e. neither later sociological notions of function, nor any particular mathematical technique or formulaic expression of the concept.

I have taken this very quick and partial dip into the history of social and economic science because I think the appearance of 'non-Malthusian' in Dr. Kleinman's title is not to be taken lightly. As yet we know little about the changing language and procedures by means of which the 'society' of the beginning of the nineteenth century was turned into the 'social', the 'sociological' and the 'social scientific' of its end. The close association and increasing importance of numerical representations and vital metaphors, to which Malthus gave the most provocative prod in this country, played a major role in this process. The *Essay on Population* is as good an early demarcation of the sociological period of our culture as we could wish.

The partly mathematical way of investigating society is, of course, a notion peculiar to the West. Scepticism of this approach has been a minority view for the better part of this century, and since the 1939-45 war has become a minority view in studies of non-Western societies. Even now active concern about the consequences of this ever-growing influence is due mostly to a small number of demographers and economists. They have been quick to point out the opportunity for a dialogue with anthropologists, which is now at last beginning.

Kleinman's book gives ample evidence of this. There are anthropological references, sometimes in profusion, in nearly every chapter. He is unable, however, to draw from anthropology a framework with which to organize his study. Thus, 'non-Malthusian' is a bit like 'post-structural': there is a fairly clear picture of the idea of structure we would like to modify, but the alternatives are as yet pretty sketchy.

'Non-Malthusian' nonetheless raises several aspects of the general malaise over structure in the social and human sciences, and (at least for anthropology) in a helpfully non-sectarian way. First, it makes clear the need for a historical perspective; there is little option but to return to the formation of an explicitly *social* science if we are to understand both our peculiar obsession with structure, and the peculiarity of the structures with which we have become obsessed. The neo-Durkheimian phase which has characterized recent social anthropology has, in this respect, proved no more satisfactory than the recent neo-Malthusian phase in population studies.

The second aspect is the centrality to the study of society of the isomorphism (or unexamined collapsing together) of material and mathematical regularities. This refers as much to the recent attempts of anthropologists to apply formal techniques to their typically ideological materials, as to the rather ideal measurement of the facts of life promoted by demographers. The ontology of the object is unimportant: it is the nature of practice of method that is at issue here.

The process of reasoning using both verbal categories and arithmetical regularities has, of course, many precursors, but was inscribed in the basis of social inquiry in the early nineteenth century. Later writers in that century such as Farr, Galton, Quetelet, Jevons and Durkheim did a great deal to fix

a certain idea of this process on social uses of mathematics. It is only recently that the extremely narrow band of mathematics actually applied to society has been recognized as such, and that exploration of alternatives has begun. The language which accompanies applied symbolic logics, particularly its semantic and rhetorical aspects, has been much slower to receive critical attention. This may have something to do with why formal experimentation is still rather disappointing. Whether the current recreation of various symbolic logics for sociological purposes need be or is best undertaken within the formal and statistical idea of analytical method is far from clear. A genuinely non-Malthusian theory would, it appears, at some stage have to contemplate this problem.

A third gain of a non-Malthusian perspective is that it calls attention to the effects of reading sets of analytical limits willy-nilly over a diversity of problems and cultures. The rapid expansion of socio-demographic programmes and research in the Third World (almost all cast in a neo-Malthusian mould) should provide many good examples. Some major cases stand out: the widespread attempt to measure population changes in terms of 'ideal family size'; the rebirth of the Victorian idea that the life table is an adequate evaluator of social medicine; the conflicting and confusing results of multivariate analyses; and tales of the conduct of censuses in rural areas. The influence of the results of such research on government policy and programmes is an issue likely to be as telling as it is as yet inaccessible.

The fourth aspect is the search for other organizing metaphors, hopefully ones which will accommodate the undoubtedly useful and powerful limit devices of existing approaches, without being completely reducible to them. Plainly, this activity must go hand in hand with analytical reconstruction, as it is largely in processes like metaphor that analytical intuition is based.

It is in the third and fourth of these areas that the contribution of Kleinman's work lies. The early chapters give many examples of population theories, a few (alas!) due to anthropologists, which propel narrow material forces supposed to cause or be caused by population growth (e.g. changes in protein levels, agricultural monoculture) into explanations of large-scale catastrophes (war, pestilence, famine). Later chapters include references to demographic trends in this century that preferred methods of projection have tended to obscure, and also to some short-lived orthodoxies of the family planning area. As Kleinman points out, radical limit arguments have been proposed in a more or less mechanical way, they are more like caricatures than theories. We might add that they are almost invariably less subtle and interesting than Malthus's own formulations. It is both to counter the pattern of hasty generalization, and to provide a means of reorganizing the field which has a coherence of the same level of sophistication as Malthus's theory, that Kleinman proposes that we think again about the idea of adaptation.

Without doubt the most important features of *Human Adaptation and Population Growth* are its scope and deliberately moderate tone. Among Kleinman's major allies are recent works in social and demographic history, and anthropology; he has tried to set the often too technical debate over population growth in a broad historical and cross-cultural framework. The result is a more balanced perspective than one usually gets of this immense field, and the most thoughtful introduction to it anthropologists could hope to find.

The stage is set in the opening chapter by a brief resumé of Malthus's limit thesis, and Kleinman cites some more recent, trendy, computerized versions to illustrate the continuing vogue of mechanistic arguments. The balance is immediately drawn in the second chapter, which relates how different social structures mediate resource and population changes. Much of this chapter is a sensible application of social anthropology to the problem: resources in goods and people are socially defined, that is, they reflect not so much brute material scarcity or surplus as local questions of status, power and prestige; different family systems have a considerable capacity to adjust to short term economic problems; archaeological evidence and the population dynamics of contemporary primitive groups do not support the thesis of a natural human condition of uniformly high fertility checked by high morality. Indeed, Kleinman's concluding remark that 'the problems of people in coping with their environment were small compared to the problems of coping with each other' neatly states the basic anthropological view of the matter.

In the next three chapters Kleinman uses this perspective to show how three of the demons usually invoked in arguments about the pressures of population upon resources - war, famine and pestilence - result in explanations either too simple or inconsistent. He uses his early chapters to gradually draw out a set of linked themes about the nature of adaptation, which seem to me to form the basic theses of his book. At the risk of over-simplification, these may be summarized:

1. What are called 'population problems' are basically problems of the unequal distribution of goods, not utter dearth or superfluity.
2. It appears, therefore, that rural peoples face even greater difficulties than are usually recognized. On the one hand, they are subject to the production demands of the landlord, the neo-colonial regime, and to the considerable vagaries of a market economy. On the other hand, they must somehow maintain their families and communities - with all the internal dissension such forms imply - with what is left over.
3. There are a variety of solutions to this dilemma, but it is in the end the economic implications of family organization and local values that comprise rural peoples' capacity to adapt. Fertility, as a key determinant of the size of the labour force and of consumption, and as the

source of children and the many functions they perform, is one of the principal means of this adaptation.

The human context Kleinman has in mind appears to be a kind of socio-demographer's view of life in Hardy's Wessex.

The core of the book, then, consists of the two middle chapters on the logic and consequences of low levels of subsistence. Kleinman notes in particular that there are good reasons, from the peasants' point of view, for not increasing production, and these lie in the continuing fact that the peasant himself is the least likely to benefit from it. Attention is given as well to some of the troubling concomitants of this situation: soil depletion and deforestation; and the physiological effects of low subsistence on human growth, maturation, mentation and reproductive capacity.

The remaining two-fifths of the text take up the theme of fertility as a principal adaptive mechanism in this social and economic context. Fertility research, however, has become a simply immense and complicated field; the chief problem on any issue has become how to see through the dense (and, as Kleinman notes, generally ethnocentric) sociological, demographic, economic and psychological literature. The main attribute of this literature is that it is very inconclusive. Kleinman's procedure is, in effect, to turn the usual approaches around and say, 'so what have we learned that fertility is *not*?'. He proceeds by setting the various models, theories, cases and so forth against each other, and they for the most part duly cancel each other out.

The result is a very reasoned review of the development and present state of the field. The statistical approach in social psychology, for example, has yet to account adequately for fertility changes, even in the rather untypical case of the United States, for which it was designed. The attempt to describe social behaviour in terms of models of capital accumulation has turned out to be even more unrealistic. The basic organizing framework for the study of social fertility, the demographic transition, needs major adjustments if it is to account adequately for national and ethnic variations in European demographic history. As it was designed originally for the European case, its exportation to the Third World, as a means of accounting for population changes there, seems even more tenuous.

The last six chapters deal chiefly with non-industrialized areas. Here Kleinman is concerned principally to illustrate a plurality of adaptive responses. The idea that industrialization and modernization are everywhere prerequisites of fertility decline is discounted, for example, by recent declines in some areas which have been much more rapid than the Western experience, and which are not confined to industrial areas or to elites. Although facts of family structure are plainly important in these cases, no clear and recurring relation between family form and demographic change has yet emerged. People appear to adapt, for example to economic problems, before such problems are expressed in generally recognized collective representations, and before there are regular attitudes of the kind measured in

surveys. Sociological reasoning is as yet not very suitable for conceptualizing these changes.

Kleinman appears to side with some recent studies² which have tried to reverse the usual approach to the timing of population changes; since little success has been met in the attempt to account for the onset of fertility declines by correlating them with the influence of standard variables (education, female labour-force participation, income, etc.), the question is turned around: close examination of the timing, it is hoped, will reveal a configuration of influences. Such influences will of necessity be defined locally, and hence the priority of anthropological considerations in exploring adaptive processes is clear.

Kleinman finds in anthropology something like the beginnings of an approach; for example in Douglas's idea that questions of reproduction take their place in a more general concern about whatever social resources are defined as scarce in a given society.³ There is a considerable body of circumstantial evidence that birth limitation is practised in many societies, not only in response to problems of subsistence. What Kleinman has noticed is that one consequence of considering scarcity as socially defined is that the idea of relative limits is subsumed thereby in relations of equality and inequality. The apparent superfluity of people in any given instance is then an issue of unequal distribution, in which questions of marriage, prestige, inheritance, and reproduction all take part. 'Adaptation' thus suggests itself as a kind of bridge which enables us to combine simple formal representations of relative inequalities, such as sets of rates or matrices, with the less formal but still abstract ways in which anthropologists limit their objects of study in order to portray them systematically.

'Adaptation' may thus prove a useful organizing idea. It has, however, one liability at present, which is implicit in Kleinman's procedure of allowing arguments over population to cancel each other out. The 'victory' of 'adaptation' is not, as it were, that of a conquering idea, which provides a programme for the methods and concepts of future generations. It is, rather, snatched from a muddle of competing notions, a mixture of the long-standing plausibility of an alternative approach (Smith and Godwin are among those cited as precursors) and the apparent malaise of the neo-Malthusian schools. For the 'perspective' which Kleinman advocates to grow into a theory, 'adaptation' will have to prove as fruitful in shifting the metaphorical or isomorphic grounds of analysis as it appears to be in relocating the idea of vital limits within a plurality of value systems.

² J.C. Caldwell, 'A Theory of Fertility: from high plateau to destabilization', *Population and Development Review* 4 (1978), pp. 553-578.

³ M. Douglas, 'Population Control in Primitive Groups', *British Journal of Sociology* 17 (1966), pp. 263-273.

Given the difficulty of this task, however, the clarity gained already in *Human Adaptation and Population Growth* is all the more to be prized.

Current trends in the population area, of which this book gives evidence, have important implications for social anthropology. Anthropologists could find themselves, effortlessly, in the forefront of the attempt to reinterpret the notion of structure as applied in socio-demographic and economic studies of non-Western peoples. This would be due to processes largely independent of anthropology; chiefly the fact that Western models and ways of thinking about society are not completely general, representing as they do a particular historical experience. The direct prod behind this convergence, at least thus far, has been the substantive difficulties experienced by population and development programmes. In the amalgam of approaches which is emerging, some quintessentially Western modes of thought such as demography and basic ideas of political economy will doubtless remain. But they may very well be recast as part of a more fundamental framework of social and cultural variation. Are anthropologists ready and willing to have their perspectives taken so seriously, and to take on the responsibilities this implies?

PHILIP KREAGER

ON CALLING OTHER PEOPLE NAMES:
A HISTORICAL NOTE ON 'MARCHING RULE'
IN THE SOLOMON ISLANDS

In the years immediately following the end of the Second World War, the British Protectorate Government of the Solomon Islands reported the activities of a 'quasi nationalist' movement (*Annual Reports* 1948:26). Called 'Marching Rule' by the Administration, these activities have been treated in all general works dealing with Melanesian 'cargo cults', as an interesting example of a people's developing political response (Worsley 1957, Jarvie 1964, Cochrane 1970, Wilson 1973). The transition, from responses based upon traditional religious assumptions to those of a supposedly more modern, pragmatic and secular kind, has been noted by all the authors; Wilson (1973) calls the process a 'rational mutation'.

But the events in the Solomons which have been gathered together under this name (and other names from which 'Marching' is supposed to have derived) remain poorly documented. Reports that some Solomon Islanders awaited the ancestors and deliveries of wealth during this period have ensured their post-war activities a place in the 'cargo cult' literature. Some authors, notably Worsley and Keesing, have argued that such reports have given too much emphasis to the millenarian aspects of the Solomon Islanders' activities and argue instead that these activities are more properly regarded as anti-colonial politics. The evidence that this is so rests upon an assumption that what has been called 'Marching Rule' was an organised, potentially nationalist, movement. While undoubtedly there were 'anti-

colonial' activities in the Solomons (as indeed anywhere else in the colonial world), the historical record, as far as it is presently known, does not justify the assumption. The evidence for the existence of a 'Solomon Islands Nationalism', quasi, embryonic or otherwise, remains unconvincing. An examination of the term itself may be helpful.

The name 'Marching Rule', with which this note is principally concerned, was regarded quite early on as something of a misnomer. Belshaw (1950) noted that 'Marching' was only one of several Anglicizations, used by Europeans to mistranslate an indigenous term. Other terms included 'Mercy', 'Marxian' and the name of a sorcerer held responsible for the deaths of two officers of the Administration in 1927 (*ibid.*: 127).¹ According to Belshaw, the indigenous term was 'Masinga'. 'I am told', he writes, 'by one who was in close contact with the movement, that the word means "brother" in the Ariari dialect' (*ibid.*: 127f).

Belshaw's authority is possibly Allen (later Sir Charles Allen), who, like Belshaw, served in the Solomon Islands Administration in the post-war period and whose own account of 'Marching Rule' was published in 1951. Certainly all subsequent authors credit Allen with making the connection between the name of the movement and the Ariari (Are'are) word for 'brother', though Allen has this word as 'Masina' (1951: 93f). Worsley, referring to 'Marching or Masinga Rule' (1970: 184), seems to have been misled by Belshaw on this point (*ibid.*: 184f). The confusion is understandable in view of the many other versions, some of which Worsley notes from contemporary sources: 'Masinga Lo, Maasina Rule, Martin Lo, Masinga Law, Marssinga Rule, Mercy Rule, Masian Rule, etc., etc.' (*ibid.*).

Keesing, in his reappraisal of the events in the Solomons during this period, confirmed Allen's earlier opinion, albeit with modified spelling. He refers throughout to 'Maasina Rule', 'The Rule of the Brotherhood',² noting,

¹ This man's name was probably Basiana, who was hanged for the murder of Bell in 1928 (Keesing 1978: 261).

² The Are'are term has been consistently translated, since Allen, as 'Brotherhood'. Some of the English usages of this word are appropriate, others more doubtful. In the sense of meaning 'association of equals for mutual help, support, protection, or action' it should be noted that this usage is both modern and Western. Whether the Are'are word can convey this meaning is for those fluent in the language to say. On the face of it, it does seem unlikely that Are'are sibling relations were ever expressed as an association of equals as in, for example, 'The Brotherhood of Man'. One appreciates that authors have found this translation attractive; somehow 'Rule of the Sibling Relations' does not have quite the same ring to it.

The word 'maasina' in the Are'are is the reflex ... of Kwiao 'waasina', Lau 'ngwaasina', etc. - terms that relate to the reciprocal relationship between siblings. In this respect Allen is accurate (1978: 49f)

Both Keesing and Allen, from their different viewpoints and experience of the Solomons, represent a considerable authority and their agreement on this point might be thought to leave little room for query. Cochrane's alternative suggestion, that the name 'marching' came originally from an evangelical mission hymn (1970: 95), is regarded by Wilson as a 'more probable' view (1973: 469), though neither he nor Cochrane have offered any evidence for it.

There is one other opinion to be considered, which is contained in an unpublished memorandum, written by a former District Officer in the Solomons' administration, D.G. Kennedy (1967). In Kennedy's view the name 'Marching Rule' derives from 'Marchant's Rule', the name which he gave in 1944 to a pre-war experimental local-government scheme. This scheme had been organised by Kennedy there in 1940, on the authority of the then Resident Commissioner, W.S. Marchant. Keesing regards this to be '... possible, as an ultimate derivation for the term', though 'unlikely' and, in any event, not relevant. Keesing is certain that 'maasina' was used at the time by his Malaita informants (1978: 49f). But if Kennedy is right, and Keesing and Allen both allow the possibility,³ then his evidence may well assume a greater importance than that of providing historians with a tidy footnote. As it has not been published, I should like to consider it in some detail.

Kennedy states that when he arrived in the Solomons in 1940 the Resident Commissioner, W.S. Marchant, asked him to investigate the possibility of introducing some form of local government in the Protectorate's Central District (Tulagi, Savo and Gela). Kennedy had some eighteen years' experience supervising local government in the Gilbert and Ellice Islands. Marchant, who had himself been recently posted to the Solomons (he had served previously in East Africa), was concerned about the District's peoples' attitude towards the Administration, reflected, in Kennedy's words, in 'apathy' and 'passive resistance'. The new District Officer spent the following six months investigating the social, political and economic situation of the people of Gela and Savo.

By early 1941 he had arrived at a tentative outline for an experimental scheme, designed to address the problems uncovered in his investigation. These were, Kennedy reported to Marchant, that the people of Central District were hostile to the government because the sub-district headmen appointed by it, together with the peripatetic Police who visited settlements, were guilty

³ Sir Charles Allen has seen the Kennedy memorandum and does not disagree with Keesing's comments on it (personal communication).

of 'habitual extortion and corruption'.⁴ The government-imposed poll-tax was resented, both as a tax and for the opportunities it gave to headmen who were empowered to recommend exemptions.

To remedy this situation, Kennedy proposed to dilute the authority of both headmen and Police, upon whom the Protectorate's Administration was based, by the establishment of monthly Sub-District courts comprised of elected delegates. These courts were to discuss any matter of interest and forward their resolutions to the District Officer, but more importantly they were charged with the duty of enquiring into all alleged offences, prior to Police action being taken. They had the power either to acquit an accused person, or to remand them for trial. They were also to hear all petitions for exemption from poll-tax and to recommend accordingly to the District Officer. Finally, the courts were to attempt to organise agricultural production, both to ensure a stable food supply and to provide a surplus for sale at a market which the District Officer would set up.

This was the scheme which went into operation in mid-1941. It quickly became established and by the end of that year it was functioning satisfactorily. Marchant inspected the scheme himself, and Kennedy had little doubt that the Resident Commissioner intended to extend the scheme throughout the Protectorate.

There had been a marked improvement in the Gela peoples' attitude towards the Administration and only those who had lost some or all of their former authority were obviously disgruntled. These latter were local village mission functionaries, the itinerant Police and the headmen (although two of the latter had continued to serve after the other two in the District resigned). The experiment was encouraging but it was shortly overtaken by events.

The Japanese bombed Pearl Harbour in December, and by May the following year had invaded the Solomons and occupied the capital at Tulagi. The Administration, including Marchant, declined evacuation. The Resident Commissioner retired into the bush of Malaita Island with a wireless transmitter. Other officers, including Kennedy, became 'coastwatchers', remaining behind Japanese lines to report enemy movements to the Allied forces.⁵ The Solomons became a major theatre of war. Kennedy notes of this period (during which he became something of a legendary

⁴ Kennedy gives some emphasis to the manner of his reporting to Marchant. 'The plan was then verbally explained to the Resident Commissioner who gave it his verbal approval for an immediate trial. It would seem that it was Marchant's intention to commit nothing of this to writing until a later stage. He did not request written reports' (Kennedy MS., 1967)

⁵ A full account of the coastwatchers' part in the Pacific campaign may be found in Feldt (1946) which contains particular reference to Kennedy's exploits.

figure himself) that he was unable to maintain other than random communication with the Gela, or to pay the District any particular attention - something of an understatement in the circumstances. However the experimental system of courts continued to operate in the District without his supervision.

When the Japanese forces were driven out of the Central Solomons, and while Kennedy was still on active service in the Northwest, a skeletal Administration was re-established in 1943. But the officers concerned, knowing nothing of the experimental system, simply re-imposed the former system of supervision through Government-appointed headmen: the now discredited 'Police Rule'. There was an immediate popular reaction and the officers in charge, some of whom were seconded from the army and who were without Administration experience, were unable to deal with it. Marchant had to leave the Solomons an invalid in the middle of 1943, and Kennedy was sent on recuperative leave in September of that year. Since the experimental scheme had not been the subject of written reports, with Kennedy's departure no one in the Administration knew what the Gela were objecting to.

When Kennedy returned to duty in the Solomons early in 1944, the newly appointed Resident Commissioner, O.C. Noel, directed him to investigate the trouble on Gela. A local leader named Alogobu had started a movement resisting the re-imposition of 'Police Rule' and had gained widespread support. In some places Kennedy's name had been used to justify defiance of the Administration.

Kennedy sent out a personal message to each of the 1941 delegates to the experimental courts, asking them to meet him. There was an immediate response, and the largest public gathering ever recorded in the District up to that time heard him address the assembled delegates. It was on this occasion that he first used the phrase 'Marchant's Rulu', when during his speech he summarised the Gela experience.

Prior to 1941, Kennedy told them, the system in force had been referred to as 'Police Rulu', which had been followed by the new system, 'Marchant's Rulu'. This system had continued throughout the Japanese occupation, though unsupervised by the Administration. Following the Japanese withdrawal, an attempt had been made to re-establish 'Police Rulu' and this had led to the local rebellion, which he called 'Alogobu Rulu' after its leader.

He then asked the assembled delegates to express themselves as to the system they wished to have on Gela. There was unanimous support for 'Marchant's Rulu', or, in its earliest Pidgin form, 'Rulu longa Masini', as Marchant's name was rendered, according to Kennedy.

'Masini' (pronounced *mar-sini* with the stress on the first syllable), could sound sufficiently like 'marching' to English speakers lacking an adequate knowledge of Pidgin usage, especially as the Pidgin *mas* ('march') could be mistakenly associated with it. Pronunciations nearer the original ('marchin' for example) could persuade Cochrane, in good faith, that Solomon Islanders

said '... marching ... correctly in the English manner' (1970: 95). That Are'are speakers had a ready-made term of their own with which to mistranslate the rumoured name for desired change has been made evident. Nor can there be any doubt that the Gela experiment and Marchant's name were both known on Malaita. A senior NCO of the armed Constabulary had accompanied Kennedy during his investigation on Gela in 1941 when the scheme was being set up. He was one of those imprisoned after the war for his part in the so-called 'Marching Rule' activities on his home island of Malaita. The Resident Commissioner had chosen Malaita for his wartime Headquarters following the Japanese invasion.

This is not to suggest that the much-reported activities which took place on Malaita after the war had necessarily any direct reference either to the Gela experiment or to Marchant. Many Malaitans, including some involved in what they called 'Maasina Rule', may honestly insist on their ignorance of both. But if 'Marchant's Rule' became the general name for desirable change, as defined by leaders and communities to whom rumour carried it during the period, the discussion of so wide a variety of reported activities becomes rather more simple.

The name which spread, according to Allen (*op.cit.*: 93), throughout Malaita to Guadalcanal, Ulawa, San Cristoval, Florida (Gela) and Ysabel Islands, cannot plausibly have been an Are'are term. If 'Masina' ('Maasina') expresses a relationship between siblings in Are'are and related languages, it cannot be seriously suggested that it means anything of the kind to members of different language-groups. Nor is it clear why traditionally independent-minded Solomon Island communities should accept an Are'are term for a relationship which their own languages were perfectly well able to express.

It seems therefore reasonable to suggest that the term which spread throughout the different islands and language-groups, was expressed in the Pidgin lingua franca. It is noteworthy that all of the names reported are either some sort of 'Rule' or, less frequently, some sort of 'Law'. *Rulu* and *Lo* are Pidgin expressions, and why even Are'are speakers would include either in a name which they had invented is a mystery. To Are'are speakers the addition of a Pidgin word to make a hybrid phrase seems both curious and unnecessary, while Pidgin speakers would not know what the phrase meant.

Kennedy's opinion alone remains plausible and matches the facts as these are known. For if Marchant's Rule meant 'Marchant's system of local Government' to the Gela, as it certainly did, and to speakers of Are'are (and related languages on Malaita), their attempts under various leaders to organise themselves in order to make their marks in the changing postwar scene, other responses by different Solomon Island communities are to be expected. That some awaited the ancestors, or the wartime Americans' return, and called their preparations by a name which usage had associated with desirable expectations, however these were defined locally, is hardly surprising. And all of these activities need have nothing more in common with each other, or

the events on either Gela or Malaita, apart from the rumour of hopeful expectancy and the name for it.

It is beyond the scope of a short note to consider the ramifications of Kennedy's view, which further research must confirm, that the name which supposedly spread from Malaita did not in fact originate there. The studies that have been made, starting with Belshaw and Allen, have been heavily biased towards the Malaita activities, and we know virtually nothing about what went on elsewhere in the Solomons during this period. But in gathering together such reports as there are, lumping them together under a name which confers upon them a suspect unity, and then attributing all of them to Malaitan influence, or even leadership, as the authors have consistently done, may well prove to have been a dubious enterprise. In any event, Kennedy's memorandum raises some doubts, and those who insist on calling other people names, in this case other people's names for them, have a duty to proceed with caution.

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FINGERS AND NUMBERS

The British Resident at the court of the Sultan of Java and later President of the Ethnological Society of London, John Crawfurd, once declared (1863: 84) that, 'The social condition of a people is ... in good measure indicated by its number system.' Crawfurd's generalization provokes some irreverent considerations. By Crawfurd's standard French culture is more primitive than that of the Kédang in Indonesia; for the French use a mixture of methods to name multiples of ten, while the Kédang language, like English, applies a single procedure consistently from ten to ninety. Judged by their cumbersome numbers, the Danes stand at the very threshold of civilization (see figure 1).

Kédang lapses from the perfect rationality of the decimal system only in its name for nine, which Kédang makes up by adding four to five, a feature which Crawfurd and others interpret as showing influence from a primitive quinary or five-based system of counting. Kédang children however have their own set of distinctive names for the numbers one to ten (figure 2).

No doubt there are several phonological manipulations worthy of note in the list of Kédang children's nonsense numbers, among them rhyme (*béang*, *méang*), metathesis (*letéq*, *telu*) and vowel contrast (*sekiq*, *sukoq*). Of the possibly meaningful elements, I can identify only the following: the child's number one (*telu*) is the adult number three, *letéq* means 'to erect', *béang* is a crow, and *méang* differs only slightly from *méan*, a superlative. But I shall have to leave these matters to qualified linguists. Perhaps I should record that my source for Kédang children's numbers was an illiterate man aged over seventy years, but whose youngest daughter was ten.

Figure 1: Number Naming as an Index of Civilization

| <i>Danish</i> | <i>French</i> | <i>Kédang</i> |
|--|-----------------------------------|-----------------------------|
| 10 tier | dix | pulu |
| 20 tyve | vingt | purun sué (2 x 10) |
| 30 tredive (3 x 10) | trente | purun telu (3 x 10) |
| 40 fyrretyve (4 x 10) | quarante | purun apaq (4 x 10) |
| 50 halvtredsindstyve ($\frac{1}{2}$ [of 20] from 3 x 20) | cinquante | purun lemé (5 x 10) |
| 60 tresindstyve (3 x 20) | soixante | purun enéng (6 x 10) |
| 70 halvfjerdsindstyve ($\frac{1}{2}$ [of 20] from 4 x 20) | soixante-dix (60 + 10) | purun pitu (7 x 10) |
| 80 firsindstyve (4 x 20) | quatre-vingts (4 x 20) | purun buturai (8 x 10) |
| 90 halvfemsindstyve ($\frac{1}{2}$ [of 20] from 5 x 20) | quatre-vingt-dix (4 x 20 + 10) | purun lemé-apaq (9 x 10) |
| 100 hundrede | cent | ratu |

Figure 2: Kédang Numbers

| | 1 | 2 | 3 | 4 | 5 |
|--------------------------|-------|--------|---------|-----------|--------|
| <i>Adult Number</i> | udéq | sué | telu | apaq | lemé |
| <i>Children's Number</i> | telu | lubaq | letéq | lapéq | béang |
| | 6 | 7 | 8 | 9 | 10 |
| <i>Adult Number</i> | enéng | pitu | buturai | lemé-apaq | pulu |
| <i>Children's Number</i> | méang | melang | sekiq | sukoq | lubong |

After a haphazard search, I have uncovered only two reports of alternative number names, one by Floyd Lounsbury (1946) and the other by Edwin Ardener (1957). Lounsbury recorded alternative numbers from a variety of North American peoples, but could explain them only as relics of lost languages. The alternative numerals of the Kpe of Cameroon Mountain on the other hand are used by children and compare therefore precisely with those of Kédang. There is no telling how many neglected lists of children's numbers might be collected from languages spoken by peoples living between these remotely situated communities, if only ethnographers looked for them. According to Crawford's principle, Kédang children obviously have attained a more superior state of civilization than have Kédang adults, since the children use a separate name for each of the first ten numbers.

Kédang children employ numbers in many of their games. I have already described (1975) *pan motiq*, the Kédang version of the widely distributed mancala, which in Kédang exploits the distinctive connotations of odd and even numbers, particularly those touching upon life, death and the transmission of souls. Children also recite an elaborate number chant when playing hide-and-seek.

A Kédang game similar to jacks called *hode-tohéq* gives characteristic attention to the distinction between odd and even numbers. Odd, or as the Kédang put it incomplete, numbers are propitious, and in several contexts the Kédang will either avoid even numbers or else arbitrarily convert them into odd numbers to achieve a desired symbolic aim (see Barnes 1974, in preparation). While playing *hode-tohéq*, a Kédang child will throw a handful of candlenuts into the air, trying to catch them on the back of his hand. If he catches an even number, he must take one away. He throws the odd-numbered remainder into the air again and tries to catch them in his palm. This time, if he misses any his turn is over. If he does not miss, he picks up the remaining nuts and continues.

In other games, children group candlenuts into bunches (*bouq*) of specific number. In one example, called *huang bouq* or *huang miréq* (the *bouq* or candlenut game), the children place a series of *bouq* in a row and a single nut, called the *raja*, in a line with them but further away (figure 3). The shape of the

Figure 3: The Kédang Candlenut Game



bouq is three nuts with a fourth on top. The players take turns throwing a nut at the raja. The first to hit the raja gets all the *bouq*, and the other players have to come up with more nuts. Each player throws one nut. If no one hits the raja, the players stand at the raja and take turns throwing one nut at a time at the various *bouq*. If anyone succeeds in knocking the top nut off a *bouq*, he gets that *bouq* and all *bouq* closer to the raja.

Seidenberg (1962: 9) identified what he calls the ritual division of numbers into odd and even among many peoples on all continents; certainly all Indonesian languages recognize it. Since Seidenberg places 2-counting at the beginning of the history of counting, his doctrine might be completed by deriving the distinction between odd and even numbers from a binary base. Dantzig (1930: 14) claims that Australians who have a binary system will rarely notice that two pins have been removed from a row of seven, but will immediately see that one pin is gone. According to Dantzig, the Australian's sense of parity is stronger than his number sense. By number sense Dantzig means the ability to perceive missing members of a set of objects, before the capacity to count is present. Whether Dantzig, and the ethnographer Curr from whom Dantzig derived his information, correctly interpreted the Australian indifference to missing pairs, I cannot determine. At any event, the Indonesian habit of designating odd and even numbers as incomplete and complete does suggest an underlying binary mode of thought.

A binary tendency underlies the procedures of multiplication and division which was practised in ancient Egypt and continued in Europe until the fifteenth century, when printed arithmetics introduced the modern techniques (Dantzig 1930: 26; Karpinski 1925: 3, 130). Multiplication was a succession of duplications; division was mediation, or continuous splitting of a number into successive halves.

There is a relationship in Kédang language between duality and indefinite multiplicity, which is exhibited in certain expressions concerning time and in a feature of etiquette having to do with commensality. To ascertain when something happened, a person must ask *weng pié deq?* (how many days ago?). To enquire when something will happen, the Kédang will say *luqa weng pié?* (tomorrow how many days?). *Weng sué* (two days), which might answer either question, may just mean several days. *Luqa weng sué*, 'two days from now', is a common way of speaking vaguely about future action. *Ewéng weng sué*, 'two days ago', often means no more than 'already several days ago'. *Numin-ewéng sué*, means 'two or more days or nights ago'. *Lumin weng sué* means 'two nights ago' or 'several nights ago'. However, when these phrases are used with any number other than two they always have the specific sense conveyed by that number.

Only through the preceding observations and evidence have I arrived at a satisfactory explanation of why the Kédang always say they are drinking their second cup of palm wine, no matter how long a feast may draw into the night. The puzzle is one that I have often had occasion to ponder, while sitting and drinking with

my friends until dawn. Kédang custom does not permit an individual at a feast or other common meal to lift his cup and drink without inviting all others present to do likewise. Then everyone must drink together. A meal is punctuated periodically by such invitations to drink in unison. There are various means of phrasing the call to drink, depending in part upon the stage the meal has reached and on the state of general inebriation achieved. On the first occasion the host or other leading figure says *tin té* (let us drink). Thereafter the usual phrasing is *sué té*, the general import of which is 'let us drink for the second time'.

By about the fifteenth cup, the Kédang are easily provoked into a conversation about the inappropriateness of continuing to call each cup the second one. For them *sué té* is a kind of conventionalized joke. According to the Kédang an external government (for some the Japanese, for others the Dutch) once ordered them never to take more than two drinks of palm wine at a meal. Thereafter, they have observed this directive in word only, participants calling out *sué té*, so that any official who chances to be passing by will not become suspicious. Whatever the historical truth of this explanation, old men have told me that when they were children (that is before the Dutch came in), their fathers used the same felicitation. The habit continues today, despite the fact that the present government places no restrictions on their drinking. The true explanation in my opinion is that in this context, as elsewhere, *sué* means simply indefinite multiplicity.

Perhaps the most startling of Seidenberg's claims is that the number base of a language corresponds to the number of persons in the basic ritual. At least he departs radically from the normal habit of deriving number systems from fingers and toes. Having asserted that the first such system was binary, he draws the consequence that counting did not start with finger-counting. Whether or not Seidenberg's theory carries conviction, it may appear to have value of a kind when juxtaposed to Crawford's complacent belief (1863: 111) that the decimal system is natural and that most of us would have had a duodecimal system, if man had been born with six fingers instead of five. Some traces of duodecimal counting do survive, giving occasion for the query whether a six-fingered race may once have succumbed to the five-fingered men of today.

The Kédang counting system is thoroughly decimal, but the names of numbers may be interpreted as giving evidence of quinary and quaternary scales. The Ende word for four is *wutu*, for eight *rua butu* (i.e. 2×4). On this exiguous evidence Crawford (1820: vol. 1, 255) attributed a former quaternary scale to Ende, Flores. Only by reference to Ende and the neighboring Ngadha language is any parallel to be discovered for the Kédang numeral eight. Eight however is not as might be expected *butu sué*, but *butu rai* (*rai* means 'many'); so the clarification to be derived from this ethnological comparison is incomplete. Whether there was ever a four-based system in the region is even less certain. It would be easier in any case to explain a quaternary base, following

Seidenberg, by reference to four ritual officers (commonplace in Flores), than in Crawford's fashion by referring to fingers, unless we presuppose that the base derived from a race with only four fingers on a hand.

No doubt the more sensible conclusion to draw is that not all of the simple number bases may be compared to features of human anatomy. But the notion of a four-fingered hand is not entirely improbable. While in the field, I attempted to get as complete a list of the parts of the human body as I could manage. My attempts to be thorough foundered however against one unforeseen obstacle: there was no name for the fourth finger (figure 4).

Figure 4: Kédang Names for Fingers

hand -- ling

finger -- ling utun

fingernail -- ling urung

thumb -- ling inan (mother finger)

index -- ling kurkata (meaning unknown in Kédang)

middle -- ling maq-molan (witch finger)

ring --

little -- ling éken (meaning unknown in Kedang)

ling anaq (children fingers) -- all fingers but the thumb

ling datén (bad finger) -- the middle finger

ling diqén (good fingers) -- all fingers except the middle finger

ling tubar (head fingers) -- the middle three fingers

Many Indonesian languages, including Malay, distinguish the thumb from the other fingers as mother and children (in Kédang *inan*, *anaq*). But, as Fox (1971: 221) remarks of Roti, for languages in the vicinity of Kédang, the pair *ina* and *ana* is a chief

means of contrasting 'large' and 'small' for objects of a similar or familiar kind. Of more immediate interest is the antithesis between good and bad fingers. Malay calls the middle finger *jari¹ hantu*, *malang* or *mati*, that is the ghost, unlucky or dead finger. The Kédang say that the middle finger is the witch finger because it is longer than the others. Professor P.E. de Josselin de Jong alerted me to a Javanese quatrain which exploits Javanese finger names, and Mrs. Noes Carey and Dr. Peter Carey kindly supplied me with one of the several versions which exist. I give below only their English translation (with appropriate acknowledgement and thanks).

[The index-finger says to the little finger:]

Little Finger, Little Finger [let us] kill Middle Finger!

[The little finger replies:]

What is Middle Finger's Crime?

[The index-finger answers:]

His crime is that he surpasses us others.

[The thumb says:]

Don't, younger brothers, don't! Your elder brother is

[already] heading for misfortune.

With such conflict in the family, perhaps there are advantages in being neglected. Human beings attribute special qualities or virtues to their fingers in other places in the world too. According to MacCulloch in Mexico warriors favoured as an amulet the middle finger of the left hand of a woman who had died in childbirth. In Italy three joints of the ring-finger of an assassin are reduced to powder, mixed with a liquid, and sprinkled on the road between a lover's house and his sweetheart's, in order to bring back her affections. The Germans, typically, used to hang a man's finger in the beer-cask in order to cause the beer to sell fast (MacCulloch 1913: 495-496). It was with great relief that I read August Friedrich Pott's appendix on fingers in his book on numbers. Although Pott's book has been frequently plagiarized, his appendix on fingers has been neglected since

¹ 'The magician ... stated that when Azrael stretched forth his hand to take the Heart of the Earth, the Earth-spirit caught hold of his middle finger, which yielded to the strain, and thus became longer than the rest, and received its Malay name of the "Devil's Finger" (*jari hantu*)' (Skeat 1900: 20).

publication.² The Kédang failure to name the fourth finger is not an isolated phenomenon, and therefore I can satisfy myself that by recording nothing for it, I have indeed completed my ethnographic chore. Pott reveals that in many languages widely scattered through the world this finger is actually called 'nameless'. Pott lists Sanskrit, Tibetan, Mongolic, Ossetic and Lithuanian as displaying the relevant feature. The fourth finger was called the ring-finger by other languages already in classical antiquity, but also the doctor finger, supposedly because the doctor used it to stir medicine (Pott 1847: 257, 284).

In the preceding remarks, I have presented stray ideas and scattered bits of fact left over from, and left out of, an essay on Kédang number use. The Kédang decimal system permits the Kédang to count as high as the ten thousands, and their use of numbers has led Professor C.R. Hallpike to exclude them from a list of societies whose concept of numbers is primitive. Despite not having concentrated specific research on these matters, I did collect enough information about numbers so that one book and one article have not exhausted all I know. I could not say for certain that there is not much more to learn. A good deal of the information presented in this essay is comparable to the subject-matter of the many books on primitive numbers, the sort of data, as Hallpike noted, that is easily accessible to amateur ethnographers. Crawford, who was an original and scholarly man, derived some of his views on primitive numbers from some languages of the Flores region, but there is no reason at all to think that Ende and Ngadha mathematical thought was less developed than that of Kédang.

The social anthropological study of numbers has not advanced much beyond these early efforts. When Hallpike prepared his own assessment, the only counter-example he could find to his generalizations about primitive number conceptions was my own at that time still largely unpublished material. My three efforts on Kédang numbers may make the point that even in a simple culture there are likely to be a variety of ways in which numbers are employed. To fix upon a few of the apparently more primitive of these without comprehending the rest is just as mistaken as it would be to judge the English conception of space alone by reference to units of measure such as inches, feet, yards and rods. The distinction between odd and even numbers is perhaps the most important principle in the Kédang system of classification, directly related by the Kédang to life and death. At the same time, the Kédang use their number concepts in addition, subtraction, multiplication and division to manage their very complicated transactions in the exchange of alliance prestations. None of these relatively advanced aspects of Kédang culture would be revealed

² Hertz (1960: 157) however did refer to Pott's appendix, and it was through Hertz that I found my first clue in the case of the missing finger.

by a study devoted exclusively to the connection between number names and fingers..

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COMMENTARY

THE CULTURAL USAGE OF SPACE; TOWARDS SOCIAL RECONSTRUCTION IN ARCHAEOLOGY

There are few archaeologists who do not, at some level or another, aspire to the reconstruction of past social conditions on the basis of data recovered through archaeological fieldwork. This data includes material from field surveys and excavations, from aerial photographs and in some periods, from literary sources as well. The archaeological record offers a very broad range of evidence, but all of it is partial, often minimal. It is frequently very difficult to fix particular material remains in time. The problems of the partiality of material recovered and of the temporal resolution possible are the greatest obstacles to social reconstruction inherent in the material base upon which archaeology focuses. That which is potentially recoverable from any particular period in the past will diminish with time, as more disintegrates beyond recognition or is destroyed by modern activity. Temporal resolution, on the other hand, ought to improve with the development of more refined and accurate dating techniques.

The development of social theory within archaeology has always had to take cognizance of these two factors. Over recent years archaeological theory aimed at the problems of social reconstruction has fallen into two occasionally overlapping, but essentially separate, approaches. The first has been the consideration, adaptation and development of various theories of society so as to bring them to bear more specifically on archaeological questions. The second has been the emergence within archaeology of what is termed 'middle range theory'.

The 'theories of society' approach can be subdivided into three theoretical frameworks which are usually seen as offering competing models of social formations, though to some extent it

is obvious that each, in fact, sees different aspects of social formations as primary and therefore aims its analysis specifically at the reconstruction of these aspects. Two of these frameworks have been developed from bodies of anthropological theory; the neo-marxism of people such as Terray, Bloch, or Godelier, or the structuralism deriving in the first instance from Lévi-Strauss, but taking further inspiration from the structural linguistics of Chomsky, Saussure and others.

Marxism is, of course, an obvious source of inspiration for archaeologists because of its essential materialism. The step from material items and their patterning through space (i.e. the primary data of the archaeologist) to modes of production and economic networks is one which seems theoretically justifiable and also practically achievable. Structuralism offers what initially seems a much lesser potential for the archaeologist. Aspects of this theoretical framework such as the concentration on the symbolic and on the structure of meaning patterns not only confront the archaeologist with new concepts, but also seem to be so far removed from the material as to be inapplicable. However, in both instances, the archaeologist is attempting to interpret material observations within a theoretical framework derived essentially from social observations, and the far greater numbers of Marxist archaeologists reflects the practical problems of this inferential process rather more than the relative merits of the two social theories.

The third framework within the theories of society approach is qualitatively very different in being derived from the physical rather than the social sciences (although it might be claimed that it represents an attempt to find a level at which it is possible to encompass the social within the physical). The systemic approach to human societies and its application in archaeology (most notably by David L. Clark and A.C. Renfrew) takes *interactional* relationships as regular and knowable on a theoretical level. It then becomes a matter of achieving a sufficient level of detail to reconstruct specific parts and of minimising the number of parts about which little or nothing is known. From this one can produce not only a social reconstruction, but it should be possible, if the social conditions at two points in time are known, to simulate the processes which have intervened between the two system states. Thus systems theory also offers us an improvement on the use of comparative 'slices of time' for dealing with long-term social change, by being essentially dynamic. Here lies what is perhaps the greatest attraction of systems theory, and the greatest criticism from systems theorists of those operating within the anthropologically derived social frameworks is that they lack the analytical power to cope with long-term social change. Against the seductive prospect of being able to explain social change are the problems that the appropriateness of a systemic model to human societies remains questionable (and there has been no exhaustive attempt to test its validity in non-archaeological situations) and that in many archaeological situations the level of data recovery is far from

adequate for a systems analysis.

For those archaeologists trying to develop appropriate theories of society it is the level of data recovery in archaeological contexts which constitutes the most severely limiting factor. And it is at this point that the exponents of 'middle range theory' have been focusing their attentions. There are three important transforms effecting the differences between the material culture of a society in the past and the material assemblage available to the archaeologist. These are patterns of deposition, of survival and of recovery. Middle range theory is concerned with what factors effect these patterns and whether these can be viewed systematically and therefore be built into any analysis of archaeological material. Ethno-archaeology has developed specifically to facilitate these investigations and to test hypotheses generated by this approach in ethnographic situations.

The disjunction between the two approaches can rather crudely be expressed as the separation of theories of the social from theories of the material. This disjunction is of crucial significance within archaeology, because if social reconstruction from the essentially material remains of the archaeological record is to be possible the relationship between the material and the social must be the pivot of the interpretational process. Because the archaeologist must rely almost exclusively on the material for his reconstruction he has tended to view this as an image (though a distorted one) of the social, rather than as an aspect of and integral to the social. Similarly, he has always tried to infer social patterns directly from material patterns; a material pattern is not an image of a social pattern, but is, in the first instance, a part of a social pattern.

The medium for archaeologically recognised patterns is space; space is also the medium though less explicitly so for ethnographically recognised patterns. The difference between the archaeological and the anthropological concepts of space is that archaeologists tend to regard space as an abstract framework within which social formations and material objects alike exist; it is essentially a physical and constant phenomenon. For the anthropologist space is more something which is created and used, and it is social and cognitive or symbolic as well as physical. If the archaeologist adopts and adapts this concept of space and attempts to analyse and interpret the spatial patterning available to him in the archaeological record as the products of human actions and of intra- and inter-group interactions, the disjunction of the material as image of the social should be obviated. In order to do this the development of a theoretical framework in which the material and the spatial are integral to the social is necessary.

A first step in the development of such a framework is the testing of the hypothesis that the use of space has culturally specific patternings. To do this the usage of space as apparent in the archaeological record must be considered on a multiplicity of levels: from formal and decorative aspects of individual artefacts and artefact assemblages, to the differentiation of

activity-specific areas within sites or sites within regions. The hypothesis is comparable to the assumption implicit in much ethnographic work that relation structures of the social and the cognitive (including such things as kinship categories and the symbolic meaning structures behind ritual action) can be considered to be culture-specific. While the anthropologist takes as given the cultural entity within which his analysis is to be applied so as to isolate the features and structures which are exclusive to it, the archaeologist must work through identification of comparable patterns back to the cultural entity itself.

If this hypothesis can be shown to be correct the archaeologist will be in a position more closely comparable to that of the anthropologist; he would then be able to interpret social formation and intra- and inter-group relations within the context of known cultural groupings. It is necessary to achieve this sort of comparability if social reconstruction within the theories of society frameworks are to be possible. This is true for systems theory as well as for the more explicitly anthropological approaches, for the starting point of systemic analysis must be a social entity, though this is not necessarily a cultural one. Within these proposals it is not intended that the considerations of 'middle range theory' should be neglected; these are essential to the practicalities of testing the hypothesis outlined above. The sort of theoretical developments that it is hoped such an approach may achieve should avoid the disjunction between the theories of the material and the theories of the social at present apparent in archaeology.

VIRGINIA ALLON

BOOK REVIEWS

J.C. WINTER, *Bruno Gutmann 1876-1966: A German Approach to Social Anthropology*, Oxford: Clarendon Press 1979. x, 243 pp., Selected Biblio, Index. £14.50.

J.C. Winter has written an important book. The main topic is the impact of preconceived theories upon the organization and presentation of Chagga ethnographic materials in Bruno Gutmann's writings. For the German-speaking *Völkerkundler* it is good and refreshing to hear that Bruno Gutmann was not just a missionary writing incoherently about various ethnographic topics and therefore conspicuously ignored by the ethnological profession in Germany. He developed an original approach to social anthropology and his influence has been visible in recent years in the work, for instance, of Mary Douglas and Victor Turner, and earlier in the writings of L. Lévy-Bruhl, W.I. Thomas, C. Lévi-Strauss and Franz Steiner. For example, it was Gutmann's theory of totemism, the corner-stone of his conceptual system, that seemed to Lévy-Bruhl to provide a basis for his own theory of mystical participation.

Yet Bruno Gutmann is hardly known outside the few specialists interested in East Africa. From 1902 to 1938 Gutmann lived among the Chagga of Kilimanjaro. He wrote several hundred articles and over a dozen books on the Chagga, and his experience in applied anthropology has a special relevance to the policy of Ujamaa in modern Tanzania.

Gutmann was not interested in developing a theory from his experience of Chagga life. On the contrary, he already had a theory, and Winter shows convincingly the various roots of Gutmann's thinking. Gutmann wanted to present his ethnography to the reader in a form that seemed to him meaningful. He took his theoretical assumptions and their implications for granted, and did not care to formulate or to discuss them explicitly. Gutmann was therefore rightly called by Evans-Pritchard 'a social mystic'. Winter's book is an anthropological study of an anthropologist's work. He reconstructs the fundamental concepts of Gutmann's anthropological theory from a wealth of descriptive contexts where they lie hidden: using every scrap of evidence¹

¹ For example, 'Frau Gutmann's ... father ... owned a little farm in the Erzgebirge and operated a petty transport service Little Bruno developed a strong attachment to his grandfather's draught-horses and later recalled that the memory of his experience with these animals decisively influenced the conception of his theory of totemism.' (p.30)

the author organizes his material in five chapters: I. Introduction: The 'Depth View', II. The Life of a Missionary: 'Outsider-ship and Integration', III. Social Origins: Totemism. The Creative Emotion, IV. Vitalism and its Cancer: The Theory of Animism, V. Living Social Structure: The Sociology of the 'Primal Ties'.

For Gutmann ethnography was the ideal vehicle for spreading the gospel of his anthropological theories. They were the justification, the motive for his ethnography. Gutmann's theories do not necessarily become any better once their axioms are understood. But a knowledge of their fundamental assumptions will certainly dissolve some of the mist which the mystical verbiage creates between the Chagga and Gutmann's readers.

According to Winter's analysis Gutmann organizes his ethnographic and sociological material in accordance with a vitalist and symbolist approach. His sociology is based on the theory of *Gemeinschaft* (organic life) and *Gesellschaft* (imaginary and mechanical structure) developed by Ferdinand Tönnies, whom Gutmann never mentions. Although Gutmann was critical of fascism, he used some of the *Blut und Boden* concepts. The reader becomes reconciled, however, by the title of one of Gutmann's books *Dichten und Denken der Dschagganeger* which, by its allusion to the bon mot *die Deutschen, ein Volk der Dichter und Denker* openly challenged in 1908 the general colonial preconception that Africans were primitive, savage, uncultured, and at best half human. Winter discusses at great length the very personal, indeed odd, style of Gutmann, which is 'indigestible for non-German readers' and tries to explain terms such as *Spruchrasen*, *Rippler*, *Gliedwesen*, *vergliedert*, etc.

Malcolm Crick writes in *JASO* (X: 3 (1979), p. 143) that anthropologists have an ability to make the strange very familiar. In Bruno Gutmann's writings the ethnographic facts about the Chagga seem very familiar to the reviewer, but Gutmann's implicit theorizing about them reads rather strangely. Winter's sympathetic analysis helps us understand what theoretical use Gutmann made of Chagga ethnographic facts and increases our admiration for Gutmann's *oeuvre*.

ANDREAS KRONENBERG

JOSEPH G. JORGENSEN, *Western Indians: Comparative Environments, Languages, and Cultures of 172 Western American Indian Tribes*, San Francisco: W.H. Freeman and Company 1980. xi, 625 pp., Appendixes, Bibliographies, Indexes, Maps, Illustrations. £29.50.

Western Indians is subtitled 'Comparative Environments, Languages and Cultures of 172 Western American Indian Tribes'. Jorgensen however makes no mention of many famous tribes of the Wild West;

for his sample excludes the Great Plains and takes in only the reasonably well recorded peoples of the ca. 250 'ethnic units' west of the Rockies from Yakutat Bay in Alaska to northern Baja California. This concentration permits intensive statistical examination of a sample consisting of 443 variables, ranging from 'salt added to food' to 'charms and magic used to influence gambling'. Jorgensen's project continues a statistical style of comparative studies which derives from Tylor. As such it runs up against familiar difficulties, among them 'Galton's Problem' (what are the statistically independent units suitable for valid comparison) and quandaries about the proper definition of variables. Jorgensen knows where the traps lie and takes steps to avoid them, but even so certain problems in the method may have no solutions. Jorgensen's variables tend to be conventional, terminologies for example being assigned to the Crow and Omaha types. The procedures sometimes lead to odd results: one of the maps demonstrates that for much of the inland western United States, no sea mammals were hunted. But if the reader wishes to know what kinds of digging sticks were used or whether first- or second-cousin marriage was approved, he will find that the relevant maps display visually informative and fairly clear representations of these traits. At the most general level, Jorgensen and his associates found expectably that economic organization, technology, and subsistence economy were much more closely correlated than were other areas of human activity. Politics, warfare, spirit quests, shamanism and so on were most subject to local variation and correlated least well with the other selected topics. Environmental factors influenced but did not determine cultural patterns, which maintained themselves in diverse environments. Furthermore source material demonstrated the 'embeddedness' of economic patterns within other aspects of culture. 'For an anthropologist, no experience can be more instructive about the intertwined nature of economy, kinship, polity, and religion among tribal peoples than reading several hundred ethnographies on 172 tribes.' The reliability of the tests of course depends upon the researchers' ability to accurately extract the information which they read, as well as upon the thoroughness of the ethnographic record. The bibliography in *Western Indians* is perfunctory, but the author claims that the team read every available ethnography and that each researcher independently read practically every source on every tribe in detail (and this in a mere one and one half years). Jorgensen avers that the region may be the best reported in the world, but the reader must write to the author to find out how much information is available for each of the variables for any given tribe. The author, the project editor and the manuscript editor have done their best to assure that the text is acceptable, but the prose, distributed through double columns on 313 large pages, does lie rather heavily on the stomach. The book, which is the first of several volumes, is best used as a reference work. The several appendices, indexes, figures and over 250 maps enhance its usefulness, but there are so many maps and other illustrations that a table of figures should have been provided.

JOHN T. HITCHCOCK, *A Mountain Village in Nepal* (Fieldwork Edition), New York etc.: Holt, Rinehart and Winston 1980 [Case Studies in Cultural Anthropology, general editors George and Louise Spindler]. vii, 143 pp., Select Bibliography. £3.25.

This is a reissue of *The Magars of Banyan Hill* which came out in 1966. It is unchanged except that an extra chapter (previously published in *Being an Anthropologist: Fieldwork in Eleven Cultures*, G. Spindler ed., New York 1970) is added in which Hitchcock describes the fieldwork that was the basis of the rest of the book. His research proposal is also added in an appendix. The book as it originally stood was an excellent, well-written description of life, Magar life in particular, in the more fertile and prosperous parts of the middle hills of Nepal; but it was unlikely to have interested anyone who did not want to know about Magars, Nepal or Hinduisation. The extra chapter changes all that. In the first place, it adds the moving story of the difficulties of fieldwork and the death of the Hitchcocks' 18 month old son. We are also told how it became clear that the original research proposal was misconceived. And finally it provides us with material on the northern Magars which helps to put the Magars of Banyan Hill (who are southern Magars) in context.

As a general rule it would surely be unfair to hold anthropologists to their research proposals. But in this case it is worth examining the proposal in detail because it contains a notion of the tribe as a self-contained culture, a common but misleading view. Its inapplicability to Nepal (as indeed to much of India) is, I think, the real reason that Hitchcock's proposed 'retrospective experiment in adaptive variation' failed; it was not simply that the initial conditions turned out not to hold.

The inspiration for the research project came from Marshall Sahlins' *Social Stratification in Polynesia* (Seattle 1958). Sahlins assumed that the Polynesians had come from the same source with the same culture which had then adapted to 'environments differing as much as high volcanic islands and low atolls that barely rose above the sea'. Differences within their culture could then be traced back to these differences of environment. Hitchcock's idea was to duplicate this situation in Nepal. He assumed that 'divergences in cultural cores will be related to four major variables: (1) cultural genesis of the group; (2) cultural features "offered" by diffusion ... (3) internal innovations; (4) environment.' He would try to hold the first 3 variables constant

by taking a single small tribe as the unit of investigation ... by selecting communities which share the history of the same isolated region, and by selecting communities which at present all are roughly equidistant from important foci of 'outside' influences ... by choice of communities which always have been in fairly intimate contact with each other.

There is some ambiguity in the term 'cultural core'; but the crucial thing is the idea that if the above conditions are satisfied, the differences between the two communities chosen will be the effects of adaptation to different environments.

However, as Hitchcock himself describes, the Nepalese middle hills have been open to influences from the great traditions of India and Tibet for over a millennium, and they have been part of the officially Hindu kingdom of Nepal for 200 years. Where there are cultural models which the tribe is well aware of, and which certain members of the tribe self-consciously imitate, Hitchcock's model of an isolated entity determined by 4 separate variables seems quite inadequate. Features coming from outside (variable 2, above) are not discrete items, such as a new way to make a hammer, but a whole way of life that is perceived as superior. (It may be replied that the term 'cultural core' was meant to refer only to those elements of the culture which are determined by the environment and its exploitation, but this would make the avowed aim of the research - 'to test the hypothesis that two [or more] divergent cultural cores will be found to vary functionally with two [or more] types of environment' - quite trivial.)

Hitchcock describes how the headman of Banyan Hill and certain other Magars have recently been converted to Vaishnavism, a way of being more pious than many Brahmans in the area. But all the Magars in the area have long since accepted Hindu ideas of ritual pollution and caste institutions. His description of the Small Dasain festival and the relation of 'sacred and secular, Brahman and headman' which is implied and reasserted in it, fits the model of *Homo Hierarchicus*.

It is therefore clear that the Magars have not been an isolated community for a long time. Nevertheless, Hitchcock spent much time and trouble in trying to find a community of Ranas and/or Thapas, the sub-tribes represented in the southern Magar community of Banyan Hill. Eventually he had to settle for Monal, a village of Ghartis and Puns. Once there he realised that

these people were too unlike those in Banyan Hill to believe the difference could be accounted for mainly by a single variable - adaptation to differing environments. They spoke a Tibeto-Burman tongue Hem [the southern Magar research assistant] could not understand Another of the bedrock assumptions I had wanted to make was that the different Magar groups studied would be aware of one another's ideas. Even this assumption was shaken when I saw how surprised and shocked both Hem and Krishna [also a southern Magar] were to discover that these people would eat a cow if it had died a natural death.

The element that is lacking from the theoretical discussion in the book, though it is there in much of the description of Banyan Hill, is the concept of Hinduisation (also known as Sanskritization). This is surely crucial for understanding the culture of any group in Nepal (with the possible exception of the Bhotiyas and the Sherpas, who have an identity defined by their

relation to a different great tradition, the Tibetan). It would be fascinating to have a detailed comparison of the degrees of Hinduisation of Monal and Banyan Hill. It might then become apparent that environment does indeed have a crucial role to play, though perhaps more of a 'prohibitive' or 'permissive' one than the 'creative' one Hitchcock was interested in investigating. It is likely to be crucial because it determines both geographical proximity to the local representatives of the Hindu great tradition and the wealth that people have, relative wealth being an important asset in the attempt to have one's new and purer identity accepted by others.

DAVID GELLNER

HAIM HAZAN, *The Limbo People: A Study of the Constitution of the Time Universe among the Aged*, London etc.: Routledge and Kegan Paul 1980. vii, 186 pp., Index. £10.50.

Issues concerning the old in our society are sensitive. The non-aged have consequently relegated the aged to another category of species. Certain activities (like sex) are classified as the prerogative of the non-aged. The knowledge that we will all have to cross the boundary into old age without the necessary aids to adjustments in our social relationships or to our inner psychological states is unwelcome.

The Limbo People explores and confronts some of these problems. The book, which is the result of work carried out in a London Day Centre for elderly Jewish people, shows that stereotypes of the aged, held by the non-aged, are entirely different from the reality they experience. The aged are seen as being in a static condition - in the peaceful Autumn of life - waiting for death and dwelling nostalgically on past events. In reality, Hazan says, the aged are experiencing the most changeable period of their lives 'a freezing of their social condition'. They suffer bodily deterioration, ill health which often forces them to give up life-long occupations and hobbies, and there are drastic mental re-adjustments to be made. The reciprocal relations usually shared with family and friends are often lost. They are no longer expected to participate in 'dynamic interaction' with the outside world.

In the Day Centre, described by Hazan, the participants re-create a new social environment in an effort to counteract the effects of the outside world, the 'Limbo World' of his title. Time, as it is experienced in the outside world, is rejected and past history is re-constructed. Some nostalgia for past events is allowed but participants are largely discouraged from talking about the past. Photos of grandchildren are discouraged. Death, and departure from the Centre, are seen as the same, and those

involved in either are quickly forgotten. Care and help diminish noticeably in the Limbo World and so, in contrast, in the Centre they become the basis for social relationships and the criteria for the incorporation and acceptance of new members. Ironically these qualities require no reciprocity as they do in the outside world.

Hazan points out that the Centre does not solve the basic problems; society's stereotyping of the aged persists and the old continue to deteriorate physically. He suggests, however, that helping the aged to re-construct a world that naturally continues the previous one and which entails some kind of 'dynamic interaction' with it might be more beneficial than providing physical havens where time is marked by the arrival of the next meal or bedpan.

The book raises interesting questions concerning the awareness of time constructs within our own society and varying perceptions of time within each individual life-span. The main weakness of the book lies in the inadequacy of the Day Centre as an ideal 'model' for examining the problems of the elderly; the age range is wide and some have been admitted because of their disabilities. They are, of course, related by their inability to participate in the 'Limbo World' any longer. The Centre creates a mirror image of the outside world yet, in the book, the 'Limbo World' remains shadowy and insubstantial. Although the book concerns elderly Jewish people its findings would be relevant to any similar group. However, I found it disappointing that its treatment of the adjustments of Jewish people to both past and present did not contain some consideration for the almost obsessive Jewish concern with history - not only because Jewish religion is rooted in a historical covenant but also because of the 'accidents' of history of which Jews seem to be the perpetual victims.

These criticisms are minor and do not detract from the book. It is a welcome contribution to the anthropological consideration of problems of present-day Western society.

PAT HOLDEN

DONALD CORDRY, *Mexican Masks*, Austin: University of Texas Press 1980. xxiv, 259 pp., Bibliography, Coloured Ills., Index, Maps. £24.00.

In what is to date the most comprehensive record of Mexican masks, the late Donald Cordry has combined into one volume some forty years of experience as an ethnographer and artist.

Mexican Masks is by any standard a beautiful book, containing over three hundred illustrations most of which are in colour. In many ways it is a testament to Cordry's own life-long fascination with Mexico, for this remarkable collection provides us not

only with an insight into the material culture of Mexico; it also says a great deal about Cordry's eye as a collector. The vast majority of masks pictured come either from his own collection or from collections which he helped build for institutions such as the Museum of the American Indian in New York, the National Museum of Anthropology and History in Mexico City, and the American Museum of Natural History.

Cordry's own selectivity, however, exposes him to certain anthropological criticisms, especially since his perspective is primarily an aesthetic, if not a somewhat curatorial one. Masks, thus considered, comprise a category of objects responsible for a bewildering plethora of meanings since the method of categorization frequently places the social significance (or insignificance) of the phenomenon of masking on a level secondary to the aesthetic contemplation of the objects themselves. While, therefore, most of the masks are beautifully photographed, a much smaller number of plates actually shows them in use. Still, this format is not altogether Cordry's own choice. As the author points out time and again, mask customs die daily in Mexico and all too often a mask surfaces from storage or in a market without anyone recollecting the details of its meaning or function. In these cases we are left only with relics produced by mask-makers for whom carving was often a sacred activity performed only after fasting and with the aid of hallucinogens. That many of these masks are evidence of the limits of the human imagination cannot be doubted; but nor can it be doubted that in general we are yet very far from a full understanding of Mexican masked phenomena. What we are left with in Cordry's work is probably the best document on Mexican masks for some time to come as well as a distinct feeling that a great tradition has collapsed in the lifetime of one man.

DAVID NAPIER

CLAUDIO MARTA, *A Group of Lovara Gypsies Settle down in Sweden: An Analysis of their Acculturation*, Stockholm: University of Stockholm, IMFO-gruppen 1979. 128 pp., Bibliography. No price given.

The Acculturation of the Lovara, Stockholm: University of Stockholm, IMFO-gruppen 1979. 31 pp. No price given.

These two books were written by an Italian anthropologist for a Swedish doctorate in the social sciences. They constitute no small problem for the social anthropologist. On the one hand Marta has an excellent case: he addresses himself to one of the most pertinent problems of Western society, that of the treatment of minorities or immigrants; and he has shown both the energy and the courage to continue his work in a sensitive area

of social ethics and practical policies. On the other hand, Marta's analytical skills are less than convincing, and his results are almost devoid of anthropological insight. Nor can he be redeemed merely by his declaration of solidarity with the Lovara Gypsies.

In 1969 Marta, then a student of philosophy in Rome, came into contact with a group of Lovara Gypsies who were settling down in a Roman shanty-town, where he had been working as an unpaid social worker. In this capacity Marta soon found himself committed 'side by side with the Gypsies, to the achievement of their most basic rights: vaccination and schooling for the children, [and] improvement of the hygienic ... conditions of the camp'.

In spring 1971 the Lovara accepted the offer made by the Swedish government to settle in Sweden. Marta eventually accompanied the Lovara to Sweden later that year, and he stayed for two months in the first temporary camp. In 1973 he re-established contact with the Lovara. They consisted of approximately 40 people and it was among these new immigrants to Swedish society that he carried out fieldwork for 15 months in 1974-75, sponsored by the Swedish authorities.

Marta's studies of the Lovara Gypsies leave one with the impression of an extremely favourable anthropological case which could be analyzed with a view not only to their clinical or scientific aims but also to political and pragmatic issues. Potentially, then, Marta's work is of extreme interest because it is placed squarely in the field of tension between theory and practice - or between speculative and 'applied' anthropology. For a Scandinavian like myself Marta's work was of further interest because it could provide us with a much-needed anthropological commentary on what seems to be an increasingly inefficient and even deplorable policy towards immigrants in Scandinavian countries.

These positive expectations unfortunately did not survive the first few pages. The works are totally lacking in anthropological awareness, and leave the impression of theoretical and conceptual incoherence; Marta shows astonishingly little sensitivity even to the Lovara culture which he purports to study. The failings of Marta's work emphasise the urgent need to weed out reports which purport to be anthropology but seem to be quite unaware of what anthropology has become. It would be unfortunate if anthropology were to continue to be represented simply as the study of exotic peoples; it is, rather, a specific field of comprehension based on a particular epistemology, and with a coherent set of concepts springing out from a long tradition of scholarship.

The good points in Marta's work emerge directly from the material - that is to say from the quoted statements of the Lovara, the Swedish welfare workers, and the schoolteachers - rather than from Marta's analysis itself. Marta continually poses the wrong questions. For instance he refers to 'double socialization', and quotes a remark made by a schoolteacher about a Lovara girl, H, that

"It's as if H lived simultaneously in two different worlds. One is the 'world' of her group, the other that of school. In each of the two worlds she has to speak a different language."

Marta reduces this to a problem in personality development. Even his acknowledgement of two different sets of ideologies - based on the 'clan' and on 'Magic' on the one hand and on 'individualism and rational thought' on the other - never takes Marta to the point of analyzing the nature of the problem of acculturation as one of a particular relationship between cultures as different systems of meaning. We do not even get a clear impression of the Lovara culture, and Gadjé society (in this case Sweden) is presented as nothing but welfare policy, suppression and prejudice directed against the Lovara, with the aim of effacing Rom culture. Even though Marta's analysis is carried out very much at the surface level of the encounter, he chooses to neglect the explicit wish of the Lovara to settle down in Sweden, to go to school, to get treated as equals of the Swedes, to get permanent jobs so that they could buy their own houses, etc. Would it have been less suppressive, had the Swedish authorities denied the Lovara access to all of these things? Marta leaves us wondering.

The failure to see the majority society as in any way a culture with its own rationality is a problem all the way through Marta's book. It possesses a structure which is impermeable to the reality of 'muted' groups (to borrow the Ardeners' term). It is as a mediator between the dominant and the muted structures, that the role of the anthropologist could have been at once theoretically interesting and practically useful.

Let us see how Marta takes up a particular problem in the encounter between the two 'societies', viz. the Lovara's exposure to Swedish television. Apparently the Lovara's interest in the 'lighter programmes' offered by (the wicked) Swedish television could have 'disastrous consequences not only on their physical but also their mental health' - but we are not told what is meant by 'mental health', or whether it is a psychological or a cultural entity. What we are told is how television 'thus became an instrument of oppression of their awareness', suggesting that the Swedes consciously use this instrument. Yet we are told in the very next sentence that

... they [the Lovara] deliberately chose not to watch the more educational broadcasts which might have helped them form some critical eye to their surroundings.

Thus, useful programmes exist, but the Lovara refuse to watch them. Who is the villain, then, if it is no longer the television programme makers; and what is the relationship between the alleged 'oppression' and the fact that the Lovara 'deliberately' choose the lighter programmes? It would have been interesting to know whether Marta would have thought it less oppressive had the Swedish authorities denied the Lovara access to television

altogether, or had they supplied them with televisions with only one channel, viz. that of the more 'interesting' programmes.

Marta takes the laudable stand that an anthropologist should take the *inside* viewpoint when studying a particular minority as against the host society, that is he should declare himself at the side of the minority group in case of conflict. However, from an anthropological point of view this stand is not honoured. Not only does Marta fail to give the reader any coherent impression of the semantics of the Lovara culture, but more seriously he seems to have misread anthropology and the method of participant observation.

For example, the Swedish family therapist (and the school-teacher quoted above) engaged in finding a job for a young woman among the immigrants showed more awareness of the no-man's-land between two cultures than the anthropologist tape-recording their words. When a job was found for this particular woman, her husband suddenly intervened, forbade her to take it, and told her to stay at home to look after their child. Facing this the family therapist remarked:

"Up against situations like that, I just don't know what to do; should I respect the group's tradition and/or husband's will or should I support the cause of the Gypsy woman's liberation?"

On this honest remark Marta comments:

Thus, the ... therapists' function ended up by having no sense at all as far as the Gypsies were concerned, and there was a serious risk that it would jeopardize the successful outcome of the acculturation process.

We are nowhere told what the 'successful outcome of the acculturation process' would be. It is this incoherence at the conceptual level which is perhaps the most disturbing aspect of Marta's work. It seems incoherent even within its own frame of reference, or phrased more accurately, it lacks a conceptual framework, which could have lent Marta's arguments some unity. Yet Marta seems confident that there is such a thing as a 'correct' approach to cultural transformations:

Only through the widening of their own traditional cultural horizons and the critical integration into the host society, minorities such as the Rom will be able to resist the continuous attacks inflicted upon them by all types - overt or hidden - of imperialism, and to find their place, side by side with other groups of dispossessed, in the struggle for the edification of a truly democratic society. In this sense, anthropological research requires a civil and political commitment.

Not only does he talk about assimilation, integration, and acculturation on one and the same page without revealing what he thinks is the difference between them, but what is worse is

his brief hint as to what a 'widening of their own traditional cultural horizons' might imply:

... it is necessary that he [the anthropologist] contributes to the awakening of the class/ethnos consciousness of the Rom and - I may add - not as an outsider, but as an INSIDER (emphasis in original).

One can very well ask whether we are still dealing with 'their own traditional cultural horizons'. It is likewise very doubtful if Marta could claim to be an 'insider' on that score. Of course, if by 'insider' we refer to a person of good-will with some understanding of the particular culture who is prepared to fight for the people as against the authorities of the host society, it is acceptable. But if we think of the dichotomy inside/outside in a more anthropological way, Marta's statement makes no sense, neither in his own terms (class consciousness, imperialism, and so forth are not part of the inside vocabulary), nor in relation to anthropological discourse in general (where the anthropologist is a mediator between the inside and the outside world).

It is very important that anthropologists acknowledge their responsibility towards the people they study, both at a theoretical and at a practical level. Certainly Marta has had the courage to commit himself to the problems of the Lovara. My misgivings about his work are based on the fact that it is merely glossed over with anthropological terminology. Social work does not become social anthropology because it deals with Gypsies. Marta has not taken the leap from social worker to social anthropologist.

KIRSTEN HASTRUP

JAMES J. FOX (ed.), *The Flow of Life: Essays on Eastern Indonesia*, Cambridge, Mass. & London: Harvard University Press 1980 [Harvard Studies in Cultural Anthropology, general editors David Maybury-Lewis *et al.*]. viii, 355 pp., Index, Illustrations. £18.00.

In his doctoral thesis, published in 1935,¹ the Dutch ethnologist F.A.E. van Wouden identified several prominent features of Eastern Indonesian societies as the core of an ancient Indonesian civilisation, traces of which were still to be found among the cultures of the area as described by nineteenth- and twentieth-century

¹ *Sociale Structuurtypen in de Grootte Oost*, published in English translation by R. Needham as *Types of Social Structure in Eastern Indonesia*, The Hague: Martinus Nijhoff 1968.

ethnographers, missionaries, and administrators. While the objectives of comparative research in this region have changed since van Wouden's time, the essays included in the present volume - which as the editor notes is the first comparative study of Eastern Indonesia to appear since van Wouden's thesis - manifest as a whole the same concerns as those van Wouden himself focussed upon, namely, asymmetric alliance (or 'exclusive cross-cousin marriage' as he called it), symbolic classification, dyadic and triadic modes of representation, and diarchy or dual sovereignty. This continuity of interests is to a large extent attributable to the nature of Eastern Indonesian societies themselves; and as Fox mentions in the Introduction, the international group of contributors to this volume, most of whom carried out fieldwork in Indonesia during the 'sixties or early 'seventies, do not otherwise share any strict unanimity of views or belong to a single school of thought. Nevertheless the far-reaching influence of van Wouden's many insights is apparent throughout this book, which Fox thus describes as both a tribute to and assessment of van Wouden's work. It should therefore be of interest not only to Indonesianists but to anyone concerned with alliance, exchange, the relation between myth, ritual, and social structure - and related topics of current anthropological enquiry.

The first part of this volume is entitled 'Marriage, Alliance, and Exchange', and the second part 'Systems of Social and Symbolic Classification', thereby reflecting the two major concerns of van Wouden's study. As van Wouden himself showed, however, there can be no absolute separation between the social and the symbolic. The division is thus more a matter of convenience than of method; and indeed, certain themes, most noticeably that of exchange, are explored throughout the volume. One problem raised directly or indirectly by several contributors is that of variation within basic patterns of social and conceptual order. In the opening essay of Part One, Needham confronts this question in the realm of social organisation by isolating five features - drawn from relationship terminology, marriage rules, and institutions - that appear to vary more or less continuously (roughly as one moves from east to west) between the domains of western Sumba. The variations found in this region, he suggests, represent different 'instants in a general process of structural change', which might be viewed as a linear, evolutionary series of transformations, within a general pattern founded on six principles that are basic to Sumbanese social life. The only shortcoming I can find in Needham's exemplary analysis is that what he means by 'asymmetric prescriptive alliance' (with the emphasis on 'prescriptive') is not always made clear. Thus, while he has elsewhere maintained that prescription is to be defined with reference to terminology, Needham states that asymmetric prescriptive alliance is practised in several western Sumbanese domains, whereas the terminology employed in these regions, according to the sources he cites, does not support this.

The essay by Gordon, which follows, also touches upon the notion of prescription, as it has been applied to Manggarai society. Specifically, Gordon contends that the contrast between a symmetric prescriptive terminology and asymmetric marriage by which Needham (in *Sociologus* XVI (1966), pp. 141-157) has characterized the Manggarai system is spurious, since the terminology, though symmetric in certain particulars, is not prescriptive. Gordon, however, bases his argument on little more than the fact that, while MB=FZH=WF=HF and FZ=MBW=WM=HM, cross-cousins in Manggarai are not distinguished from parallel-cousins and siblings. The rest of the paper is then taken up with a description of institutional and ceremonial accompaniments of marriage and an unnecessarily elaborate demonstration of the evident fact that in the Manggarai marriage system any group partakes simultaneously in a number of different sorts of affinal relations, neither of which clearly relates to what is apparently Gordon's main point. This is unfortunate, particularly as the Manggarai case could have been used to raise what seems to me an important analytical question, namely, how many and what kinds of terminological equations and distinctions need be present before a terminology as a whole can usefully be characterized as prescriptive.

Barnes's paper on alliance in Kédang is largely an expansion of certain conclusions of his 1974 monograph. Among the most important of these, from a comparative point of view, is that while Kédang has both a social order of asymmetric prescriptive alliance and corporate descent groups (in this case patrilineal clans), the latter do not operate as alliance groups, so that 'the system does not function as a whole'. One possible reason for this, Barnes suggests, is that Kédang clans are not political corporations and therefore marriages between their members do not serve political ends. In this regard, Kédang contrasts in an interesting way with the Rotinese domain of Thie, described in the following paper by Fox. In Thie, as elsewhere on Roti, marriage is not governed by a categorical prescription nor is bridewealth the collective responsibility of clans as in Kédang. Yet Thie clans are titled political units, grouped into moieties which are further divided, and marriage in Thie is supposed to accord with these divisions. By applying the notion of alliance to a system in which the marriage rule - moiety exogamy from which one part of one moiety is exempted - amounts to a purely negative injunction, Fox extends the analytical use of this term to non-prescriptive systems in an interesting way. His essay, which has a great deal besides to recommend it, also provides an illuminating instance of the relation between dyadic and tripartite aspects of Eastern Indonesian social structure. Clamagirand's sketch of the social organisation of the Ema of Timor illustrates yet another variation on the themes of prescriptive alliance and the political implications of the alliance relation. She distinguishes between a 'private domain' in which 'core houses', the senior segments of patrilineal clans, are involved in a network of asymmetric marriage relations, and a 'communal domain' involving a hierarchy of core houses, grouped into an

eastern and western division, and a group of three chiefly houses, which partake in a system of marriage alliances at a higher, more inclusive level of organisation.

Both in Kédang and among the Ema, the exchange of women in marriage is viewed as a 'flow of life', the phrase taken as the title of this volume. The last two papers in Part One deal more specifically with exchange, as it is expressed in ritual practice and ideology as well as in social arrangements. Forman examines marriage and mortuary practices among the Makassae of East Timor and shows that these are founded on a common ideology that incorporates exchange as a prominent principle. One point of significance of his essay, taken in connexion with the others, is that it reveals a good deal of similarity with regard to categories and ideology between the Makassae, who do not speak an Austronesian language or practise prescriptive alliance, and other Eastern Indonesian peoples who do. This is also true of Friedberg's paper on the non-Austronesian Bunaq (though the Bunaq do practise prescriptive alliance). Valeri, writing on the Huaulu of Seram, focusses upon marriage prestations. Since gifts made by wife-givers and wife-takers, while distinct in kind, exactly balance one another, he characterizes the exchange of these valuables as reciprocal and symmetric, in contrast to the unilateral transfer of women in marriage, which is non-reciprocal and asymmetric. Valeri then goes on to discuss the social implications of the relation between what he sees as the symmetrical and asymmetrical aspects of Huaulu marriage.

This is an appropriate place to mention that, as Valeri's essay is the only one which deals with a society in the Moluccas, an area to which van Wouden paid a good deal of attention, in this respect the volume suffers from a slight imbalance. Moreover, of the 13 other ethnographic essays six concern Timorese societies while five deal with speakers of languages belonging to the Bima-Sumba group, namely, Sumbanese, Savunese, and Manggarai.

By the end of Part One the reader will have become aware of a number of recurring themes that connect different Eastern Indonesian societies, in particular the analogous conception of the human body, society, and the cosmos; and the extensive use of the same paired categories, such as male and female. The essays in Part Two further explore these themes and thus bear further witness to van Wouden's seminal observation that in Eastern Indonesia the cosmos and human society are organised in the same way.

In his description of ideas and usages connected with livestock mainly in the western part of Sumba, Onvlee demonstrates once again the intimate connexion between the exchange of goods and social structure; his paper thus serves as a useful transition between the first two parts of the volume. Also writing on Sumba, Adams attempts to show how three major ordering principles manifest in the design of decorated textiles are also present in other areas of Sumbanese culture. Perhaps in part because, through fieldwork, I am personally familiar with Sumba, this is

for me one of the most disappointing contributions to the volume, and I feel that rather more could have been done with the topic Adams has chosen. Many of the parallels she adduces, moreover, seem trivial and factitious. Thus, to cite one example, while there is an undeniable isomorphism between the tripartite division of major villages and the three-part composition of decorated textiles, each of which, as Adams correctly notes, also incorporates a dyadic aspect, the analogy she suggests between these forms and other instances of what she identifies as a 'dyadic-triadic set' - the triad of wife-giver, ego-group, and wife-taker; the practice of two regional groups meeting at a spatially intermediate spot to engage in trade; and the use of ritual speakers to mediate between two groups - appears to be at best tenuous. In addition, the relation between the dyadic and (supposedly) triadic aspects of these arrangements is not clearly shown to be comparable between different instances.

Nico Kana's essay on the Savunese house provides a valuable addition to earlier analyses of the house as a social and symbolic structure in Eastern Indonesian societies (see for example essays by Cunningham and Fox in R. Needham (ed.), *Right and Left*, Chicago 1973). The papers by Schulte Nordholt and Francillon, which follow, discuss the same sorts of categories as those uncovered by Kana and other contributors, as these occur in the classifications of western Timorese societies. Schulte Nordholt's essay, which begins with a useful, and in part autobiographical, assessment of the influence of van Wouden's work, deals with transformations of the relationship between paired categories - in particular male and female - as these are applied in different contexts and in ways that emphasize different associations or aspects of the categories and relationships in question. Francillon, who considers the recent political history of Wehali in the South Belu area of Timor, concentrates on binary symbolism in connexion with diarchical rule, and the inconsistency between traditional and colonial leadership resulting from the failure of the Dutch to grasp the true nature of Timorese governance. Of particular interest is Francillon's suggestion that the matriliney of Wehali society can be attributed to the position of this domain within a larger political and symbolic whole, by which Wehali is opposed to peripheral princedoms that display a complementary masculine principle, as expressed *inter alia* in patrilineal institutions.

The importance of masculinity and femininity and related categories on Timor is further illustrated in Friedberg's essay on myths and rites connected with agriculture among the Bunaq and Traube's paper on the Mambai. Traube explores the contextually varying significations and associations of 'white' and 'black' as categories of ritual action, in order to demonstrate an elaborate language of opposition which orders and unifies diverse realms of Mambai experience. Traube's paper, in particular, also attests to the widespread tradition of semantic parallelism in ritual language in Eastern Indonesia.

The two essays that make up Part Three are both methodological, focussing in particular on the 'field of ethnological study'

approach advocated by van Wouden's teacher, J.P.B. de Josselin de Jong, and implicitly followed in his 1935 thesis by van Wouden himself. In the first paper P.E. de Josselin de Jong reviews the advances and limitations of this method, relating it to the wider question of comparison in social anthropology and the problem of history. Fox, in the concluding essay, discusses the development of comparative research in Eastern Indonesia. His main point - and one that is borne out by the essays in this volume - is that a common structural core, a unifying pattern of Eastern Indonesian culture that can accommodate variations encountered in individual societies, is to be located in the realm of categories rather than in the field of institutional arrangements. Being principally concerned with the latter, van Wouden, on the other hand, based his formal model of an ancient Indonesian civilisation - in essence a system of exclusive cross-cousin marriage involving phratries, marriage classes, and double unilineal descent - on certain predefined elements of social organisation. Yet, as Fox notes, more recent ethnography (including van Wouden's own research in Kodi (in *Bijdragen* CXII (1956), pp. 204-246), which showed asymmetric alliance - or exclusive cross-cousin marriage - and double unilineal descent to be inversely related) has placed his model more and more in doubt.

In this regard, it is arguable that the volume as a whole could have brought out more the major shortcomings of van Wouden's theory. (The need for such criticism has recently been shown by the attempts of the linguist Robert Blust to reconstruct mainly from linguistic evidence a Proto-Austronesian social order (in *Current Anthropology* XXI (1980), pp. 205-247). In this endeavour Blust has tended to treat van Wouden's thesis as established fact, valid not only for Eastern Indonesia but for the Austronesian-speaking world as a whole.) This, however, does not detract from van Wouden's many insights into the nature of Eastern Indonesian culture, nor from the merits of the individual essays included in the present volume.

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