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EDITORIAL NOTE

The idea for this Journal has come from the graduate students
at the Institute of Social Anthropology in Oxford. Papers given at
graduate seminars and ideas arising from work for diplomas and higher
degrees very often merit wider circulation and discussion without
necessarily being ready for formal publication in professional journals.
There obviously exists a need in social anthropology for serious
critical and theoretical discussion; JASO sees this as its main purpose.

Paul Heelas is going to teach at The Department of Religious Studies,
University of Lancaster. The editors would like to express their gratitude
to him for all his work on the journal since its inception.

The editors would also like to express their thanks to John Ryle, Tim Jenkins,
Richard Heelas, and Stephen Heelas, who have helped with the production
of this issue.

FORMAT

The journal is published three times per year. Articles are
welcome from students of anthropology and from people in other
disciplines. It is preferred that the main emphasis should be on
analytical discussion rather than on plain description. Papers
should be as short as is necessary to get the point over. As a general
rule they should not exceed 5,000 words. They should follow the conven-
tions for citations, notes and references used in the A.S.A. monographs.
Comments will also be welcome. Communications should be addressed to
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NUER RELIGION - a supplementary view

I

This essay emerged out of an undergraduate course on the study of conceptual systems. I make this pedagogic reference at the outset not only because it relates to my sub-title but also because it is as a teacher of social anthropology that I choose to express my gratitude for the works of the late Professor Evans-Pritchard.[1] I have called the essay 'a supplementary view' because it does not presume to be in any sense corrective but rather reports upon a method which I have found convenient for introducing students, early in their second year, to the totality of Nuer life as it emerges from the classic trilogy.

If social anthropology has emerged since the late war as one of the humanities able to offer itself as an education for undergraduates this implies a range of preoccupation which owes much to the width of Evans-Pritchard's anthropology. But this evolution raises new problems: the social anthropologists of previous generations had their formation in other disciplines and when they wrote it was for fellow professionals. The excellent introductions to the subject currently available reflect to a considerable extent this earlier stage: they do not have the undergraduate clearly in mind. The most fundamental problem of teaching at this level is that field-work is still represented as the essential qualification while students are required, nevertheless, to acquire a proficiency in the subject without that qualification. We can try to escape from this double-bind by tackling an associated problem. The undergraduate can scarcely be blamed if, left to himself, he tends to turn his 'required reading' into so many texts which are to be learnt, rather than as material presented by another human mind like his own to be thought about, questioned, rehandled.

One way of approaching the problem is exemplified by this essay. The attempt is to demonstrate to the student what one means when, in all seriousness, one advises him to read a book backwards as well as forwards; the implication is that he should not feel bound by the titles or chapter headings of the author which merely reflect the author's own choices, but rather attempt his own synthesis which he can then interact with that of the original. In this essay, therefore, I move freely backwards and forwards in the Nuer trilogy and attempt to show that there are certain conceptual preoccupations structuring Nuer experience. The propositions which emerge are both simple and crude. The point of the operation is to show the student that there can be alternative views and to send him back to the material in a spirit of research with the wholesome ambition of proving me wrong.

A second problem is connected with the word 'religion' which is, for the modern student, whether he has a denominational loyalty or not, a special area of experience in some way. The word 'religion' in a title is likely to set off certain defensive reflexes to the extent that 'religion' is something that other people have, something which rests upon presuppositions, faith, insight and the like which the student defiantly or wistfully, but either way disastrously, does not share in the way in which he <u>can</u> suppose himself to share, at least as a

starting point, certain suppositions about kinship or politics. The aim is therefore to demonstrate that we are dealing with simple, human thought which is the same whether people are thinking about their kinfolk, their chiefs or their gods.

A third and more general aim of this essay is to suggest how the student can grasp, as far as is possible from a literary experience, not only the specificity of Nuer life but also make some kind of meaningful and question raising comparison. Here I have limited myself to a few suggestions only of the lines along which a comparison between the Nuer and the related Dinka might run.

The discussion here presented rests upon a previous examination of the implications of the concluding three paragraphs of Lévi-Strauss' Totemism[2] in which he invites a reconsideration of the notion that 'religion constitutes an autonomous order, requiring a special kind of investigation'. The student is invited to consider the legitimacy of the grounds on which he might be disposed to distinguish between the concepts 'mother's brother' and, for example, 'ancestor' in such a manner as to subsume them under the distinction knowledge/belief. Following this discussion one turns to a consideration of the word 'religion' and, following Cantwell-Smith,[3] looks at the history and use of this term in western thought. It is useful to set against Canwell-Smith's persuasive argument the assumptions of representative exponents of traditional comparative religion, with whose dicta the student is likely to sympathise initially. Zaehner, for example, provides a good debating topic with his axiomatic: 'If we are to know what religion is we must also find something in common between the great religions of the world.'[4] The sum of these discussions leads us back to Cantwell-Smith whose welcome rejection of the term 'religion' leads him close to a sociological position from which he veers away at the last. We are, nevertheless, in a position to develop his argument and to reverse his theological proposition that it is faith which constitutes society as a community and say, rather, that society constitutes itself as faith for a community. For finally it is impossible to understand, in the sense of having something which can be communicated, in what way a man's 'belief' in his cults differs from his 'belief' in his kinship 'system', or his 'belief' in his language for that matter.

II

The terminology of Chapter I of Nuer Religion creates difficulties: one is asked to explain the force of the capital K in Kwoth as opposed to kwoth or kuth and to indicate how seriously the approximation to Hebrew monotheism is to be taken. If it is to be taken with any seriousness then a new question arises: what is it, exactly, which is 'in itself quite independent of the social structure' but 'broken up along the lines of segmentation.'? Confronted with problems of this nature one has recourse to an earlier article from the Azande period, "Azande Theology"[5] and, initially, the statement: 'In treating a religion we have only to translate primitive religious terms into our own language, and our interpretation of them is already made by the very process of translation.' The student can be invited to set the whole of the ensuing analysis of the concept mbole as a background to Chapter I of Nuer Religion and to see what he can achieve by a comparison of

the terms mbole and kwoth. This juxtaposition has the advantage that the student for whom the term 'religion' is problematic can relate that chapter, via the Azande material to his own experience.

Such a comparison should not lead to a simple equation. The most obvious difference between kwoth and mbole is that the former is both specified and unspecified whereas the latter lacks specification. To compare the two terms in this way has the immediate advantage that we are liberated from the problem posed by the presentation Kwoth and kuth, substance and fragment. Once liberated we see that we are dealing with words related as science/sciences, meaning/meanings, cause/causes and the like are related, and not with a disjunction between beings. Of kwoth unspecified we can surely say what Evans-Pritchard says of mbole: 'it[6] is the name which 'takes the place of understanding .... the horizon that rounds off knowledge and tradition ... When Azande do not understand something, it is vaguely explained by citing Mbori.'[7] But in addition kwoth is systematically specified and it is to these systematic specifications that I now turn.

The broadest specification of kwoth is, of course, kuth nhial/kuth piny - above/below. This hierarchical distinction appears to shape, or be concordant with, a set of related distinctions which are found in areas well outside the 'religious'.[8] The first associated attributes of the distinction present us with something of a puzzle. The superior has to do with the apparently fortuitous in Nuer daily life while the inferior is associated with that daily life itself. The kuth piny are largely associated with the world of lineage and descent. They are, in Evans-Pritchard's terminology, 'totemistic spirits.' The world of descent, it needs no arguing, receives a heavy emphasis in Nuer consciousness. The value (descent) is associated with what, in another context, is relatively devalued (below).

The corroborative evidence for this comes from the Nuer themselves. They, or some of them, say that originally there was only kwoth and the col wic. The kuth piny came later, they came from or with the Dinka. If one accepts that history, especially among non-literate people, has to do with now, then the factual truth of the proposition is irrelevant: for the present let us simply note that there is an association between kuth piny, second-comers, the day-to-day and the Dinka.

The idea of second comers associated with inferiority and with the Dinka puts us in mind immediately of a passage in the first part of the trilogy in which we learn that the term diel means something more funda= mental than 'aristocrat'.[9] The diel are the firstcomers, the original and authentic Nuer. It is essential to note that the term is a relative one for this reassures us that we are still in the world of idea and are not dealing with substantial identities. The members of the same clan can be diel in one area and rul in another.

People who are accepted as Nuer can also be rul but the term has its own primary association expressed by the Nuer themselves. The typical rul are Dinka. We can anticipate here and refer across to the Dinka material. Whereas the Dinka include the Nuer in an inclusive category of humanity, the Nuer draw a clear hierarchical distinction in humanity at their own cultural frontier. The Dinka are less fully human than they. The cross reference suggests some of the force in the diel/rul distinction. We are certainly dealing with ideas, but they are ideas which belong very much to the day-to-day of Nuer life, a world which, the suggestion is there, is somehow depreciated and, in some way yet to be discovered, inauthentic. What are opposed as concepts,

diel/rul, relate to what is mingled in the actuality. Nuer not only can be rul, the majority of them are.

What else do the Nuer say about the relation of diel to rul? The rul as second comers receive wives from the diel and are, therefore, sisters' sons to them. This is ideally and often actually so. The relationship expresses once again the hierarchic principle already twice noted. As affines the wife receivers remain permanently in debt for the life that they have received. The parallel with the feud is striking. Neither bride-wealth nor blood-wealth truly cancel out the life which is owed. The feud festers on to break out again. The debt incurred through alliance is registered in respect. Although the marriage is practically complete when payments are concluded and ruagh (affinity) becomes mar (kinship) the respect and avoidance owed to the mother-in-law by a man is inherited by his wife's brother's wife.[10]

We may at this point accumulate the following distinctions:

kuth nhial	diel	nath	MB
kuth piny	rul	jaang	ZS

which are associated with:

first-comers	authenticity	humanity	abnormality
second-comers	inauthenticity	subhuman	normality

What we have here is a complex of Nuer thought and it is useful to remind the student at this early stage that this is the beginning of analysis and not the analysis itself. When we are dealing with literary material it is all too tempting and indeed easy to extract a set of semi-equations of this nature. The very neatness of the extraction should alert the student to its hypothetical and provisional nature. A rich mass of material remains to be integrated; much will not be integrated by this particular formulation. As the discussion moves on one has to be on one's guard against the temptation to reduce new facts to the formula, and work, rather, towards a new formulation which might have some claim to be called analytic.

The exploratory rather than classificatory nature of the formula is usefully demonstrated by examination of the way in which relations between kuth nhial and kuth piny are represented. As we might expect the opposition is harmonious with diel/rul. Birds are distinguished in three classes - gaat kwoth, gaat niet, sisters' sons to the former and jaang. This lowest class is also described as gaat nya dila, sons of the daughters of diel. The implication is, as Evans-Pritchard points out, that they are Dinka - jaang. Similarly fetishes are said to be gaatnyadeang - children of daughters of Deng, inferior affines of Dinka therefore, 'spirits of a very inferior order.'[11] The kuth nhial are diel, the totemic spirits, jaang. All this is satisfactory and expected. What is interesting is the claim of the man who respected pythons 'that the python is the maternal uncle of the air-spirit deng.'[12]

The formula points us towards a more significant reversal: that is the relation of the so-called Leopard Skin priest to the diel of a territory. I say so-called because, following the development of Evans-Pritchard's thought in the matter, I shall henceforth refer to him as

kuaar muon- priest of the earth.[13]   This personage appears to be ideally
rul - stranger.   He is also thought of as standing in the relation of
mother's brother to the diel of the territory.   In fact many kuaar muong
lineages are of Dinka origin.

It is possible to approach this problem by consideration of another
possible complementary opposition in Nuer categorization.   This must be
tentative as it does not as clearly emerge from Evans-Pritchard's pres-
entation as do the previous ones.   I suggest a relation between the
kuaar muon and the prophet - gwan kwoth.   The kuaar muon is by his very
name associated with the below, is conceptually associated with rul, has
to do with the reparation of disunity within the tribal sections, belongs
to the world of the day-to-day and the expected.   The gwan kwoth, poss-
essed by or rather possessor of the sky-spirit is preeminently of the
above, he has to do with the political unity of the Nuer as Nuer, or
better, with the realization of the concept nath which unites all in
opposition to the external jaang.   The gwan kwoth is strikingly assoc-
iated with the abnormal and the rare.[14]   Perhaps there is something to
be made of the fact that the gwan kwoth, in the past at least, was assoc-
iated with the curing of barrenness while the kuaar muon cures incest.

The gwan kwoth belongs to the world of the above, that ideal world
which the Nuer locate in the past.   It is a world where there are only
the sky-spirits, the col wic and the pure nath, where all is diel.   This
brings the present into sharp focus.   The conceptual relationship between
diel and rul as mother's brothers to father's sisters would preclude
marriage and therefore lineal continuity for the diel males.   There is
a situation of conceptual hypogamy in a field of informal endogamy -
marriage outside the tribe is risky.   In fact the diel, who are in a
minority depend upon rul for the continuation of their lines and in real
life must be in the relation of sisters' sons to them.

More light is thrown on this by the origin myth of the Jikany tribes[15]
- in which Kir is found in a gourd, is reared by the Dinka Yul, and
becomes the founder of the Gaatgankir clan - reflects a reality upon which
the concept Nuer (nath) depends.   Seligman's account (he speaks of the
origin of the Nuer without qualification) makes Kir marry into the line-
age of Gaa, eldest son of the founding ancestor Gau, who is kuaar muon.
Kir, in this account, founds certain sections of the Jikany.[16]

The association of the kuaar muon with the mother's brother in
relation to the diel reverses the diel/rul relationship as, I suggest,
it is reversed in real life.   Positively it expresses the dependence of
the diel upon the rul for lineal continuity and for the reparation of
disunity resulting from feud.   When the kuaar muon divides, as in the
rual ceremony following incest, it is to allow lineal continuity to
develop where before incest prohibitions had precluded it.   So, accord-
ing to Seligman, the first kuaar muon was created when Gau divided his
daughters between his two sons (all children of one mother) to allow his
line to develop.   He performed the first rual ceremony which imposed
exogamy on the descendants of the two sons and made the elder of them,
kuaar muon.

The performance of the rual ceremony by the kuaar muon may be seen
as something making for lineal continuity, something upon which that
continuity depends as it depends upon the bride givers.[17]

I have said that there appears to be a contradition between the
conceptual distinction diel/rul and the facts of marriage.   Asymmetry
between affines, when combined with an endogamy whether formal, as in

the caste-system, or informal, as here, produces problems. It would seem that the greater the value placed upon descent the greater the problems must be in so far as a man's standing is affected by the marriage of a distant kinsman. The less descent is traced back the easier it is to preserve formal asymmetry combined with factual reciprocity of marriage. The Nuer concern for descent is obvious and is stressed by their habit of assimilating affines with kinsmen in the category mar. Seligman makes the point: 'Considering the wide conception of the incest barrier among the Nuer it is not surprising that the rual ceremony takes place fairly frequently'.[18] Following Evans-Pritchard we should qualify this by pointing out that there are degrees of incest from the most trivial to the most serious. Nevertheless the offence is built in as an inevitability of Nuer life. The Nuer inevitably fall short of their ideals just as, inevitably, diel stock is continued by rul women and no Nuer lineage aan be truly nath.

This, if correct, is surely the most puzzling aspect of Nuer life. The term diel seems to speak of a preoccupation wider and deeper than a purely political one. The Nuer are concerned, the literature stresses it, with lineal continuity. But the term diel associated with nath suggests a valuation of purity of descent. Leaving aside the political implications, the achieving of that purity in reality would involve incest of the direst kind. (We have already seen how the Nuer desire to widen their kinship at he expense, so to speak, of their affines, involves them in frequent, if minor, infringements of incest prohibitions.) But this condition of ideal purity is located in the mythic past. Their recorded statements relating to the proliferation of kuth piny and associated phenomena with the Dinka can be taken historically but they must also be taken as symptomatic of Nuer life at the moment when they were recorded. The contradiction is profound: the Nuer are nath not jaang, but in life they cannot be nath. They cannot maintain a strictly hierarchical organization with strict hypogamous marriage. In the Indian caste-system the dilution of purity involved in formal or informal hypergamy between castes is to a considerable extent, but not entirely, corrected by a heavy emphasis on descent. There, however, the rule of hypergamy is strict. Among the Nuer the ideal would have strict hypogamy, but an informal endogamy (or a strong tendency towards it), precludes such a solution. Nuer statements about the past have justificatory and in that sense explanatory value. We have a parallel in the hierarchy of kwoth. The movement from the above to the below is a moral decline from kwoth to jaang, even to jur. At the same time it is a movement of increasing involvement in life as it is lived. The hierarchy presents in the vertical dimension what Evans-Pritchard presents in a lateral dimension by concentric circles[19] and what the Nuer themselves present in the dimension of time: to be Nuer is best, for all that this state aan never be achieved.

The contradiction seems to be related to the Nuer tendency to at once emphasise and deny affinity. The affines of a mother's children are associated with the mother's family and simultaneously merged in the all-embracing mar. The world of mar is, again, the world of day-to-day. The world of ritual and agnation, however, is the world of agnation - buth. Those who do not have buth between them are rul.[20] The connotation of the opposition seems clear by now for those who have buth between them must marry rul. Nevertheless it is from the affines and from the children of the same mother that the lineage (literally, we remember, thok dwiel - mother's hut entrance) springs and fission results. Fusion on the other hand is between the gaatgwan - the sons of the father. Can we go so far as to suggest that this discussion points towards a re-examination of the feminine principle in Nuer society? Is it the case that the woman only achieves value by becoming male?

Certainly in rereading the trilogy I am reminded of Postumos in Cymbeline:
'Is there no way for men to be but women must be half-makers?' Evans-
Pritchard expresses the same impression: 'agnatic descent is, by a kind
of paradox, traced through the mother.'[21]

III

  I turn now to consider the material on the Dinka. If the preceding
account is both tentative and partial the remainder of the discussion
will be even more so. I shall consider only those aspects of Dinka life
which directly offer themselves as comparable with the Nuer concepts
already mentioned. An alternative, separate and necessary operation
would involve approaching the two societies from the point of view of the
Dinka. One would expect as a result to be in a position to ask questions
of the Nuer material such as might not arise without this juxtaposition.

  When we turn to the Dinka we certainly feel ourselves to be in a
familiar world. Indeed the initial impression is that simple trans-
lation will convert similarities into identities; the concepts and
manners are easily recognised. I shall touch on this question of simi-
larity in my conclusion. For the present I am more concerned with
differences. And indeed from the outset we sense a significantly diff-
erent distribution of emphasis in Dinka values. Certainly we find the
distinction of the above and the below and indeed the Dinka seem to be
more concerned with the distinction than the Nuer: but we note that
their myths concentrate on the reasons for this distinction rather than
upon the fact of it. They are myths of separation not of opposition,
if I may so put it. We can note, incidentally, that the one such myth
recorded by Evans-Pritchard 'although it accords well with Nuer con-
ceptions in general' is believed by him to be of Dinka origin.[22]

  What strikes one about the Dinka myths is that the spatial reference
is, so to speak, blurred. The separation brings loss but it also defines
man. The Dinka emphasize the positive together with the negative and
the very stress on separation suggests continuity and rejoining. This
is all succinctly expressed in the Dinka song:

    Deng brings the rope of the finch
    That we may meet on one boundary
    We and the moon and Divinity
    Give the rope of the finch
    That we may meet on one boundary with the moon ..[23]

The rope here is the rope which originally connected men and Divinity,
the possibility of its restoration is associated with the concept <u>deng</u>
in which the attributes of what Lienhardt calls free and clan divinities
are con-fused. The same tendency to merge the opposition is found in
the comparison between <u>kuth nhial/piny</u> among the Nuer, and the Dinka
<u>yath</u> (pl. <u>yeeth</u>). <u>Yath</u> is no equivalent for <u>kwoth</u>. The area of ex-
perience associated with <u>kwoth</u> among the Nuer is, among the Dinka,
divided between <u>jok</u> and <u>nhialic</u>. To what extent it would be possible
to relate one of these terms to the Azande <u>mbole</u> and then compare the
Nuer and Dinka degrees of specification, or to what extent it would be
profitable, is not yet clear. For the present what is noteworthy is

the lack of indigenous verbal distinction in yeeth which would correspond
to Lienhardt's distinction between free and clan divinities. We note in
this connection firstly that in another context Lienhardt speaks of a sky-
spirit but this is in relation to a prophet;[24] more significantly in his
list of clan-divinities the vast majority relate to earth - this includes
earth bound or low-flying birds. Those which have deng as emblem, in his
terminology, also have an earthly yath. He also reports as 'listed ....
in various parts of Dinkaland' the planet Venus, and Comets together with
Cloud emblems.[25] Evans-Pritchard has also reported that sky-spirits are
associated with small lineages, 'especially lineages of Dinka descent.'[26]
We may conclude that the spatial distinction made verbally among the Nuer
kuth is not absent in fact among the Dinka yeeth, but it is verbally
transcended and, in the actuality, mediated by earth associated birds and
objects intermediate between sky and earth.

A striking reversal of Nuer concepts is the belief among some Dinka
that their free yeeth are late-comers.[27] Again: "It is asserted by many
Dinka that long ago (watheer) they knew only Divinity and Deng who was
'Divinity itself', and the clan-divinities.#[28] Of these the most power-
ful were the divinities of masters of the fishing spear. Initially it
would seem that whereas for the Dinka the proliferation of free-divinities
'in history' is associated with an expanding universe of experience which
does not seem to challenge the lived social order, the Nuer associate the
increase of earth-spirits with the dilution of their nath quality by
jaang and jur.

We must add, as an aside, that it could also be argued that the
difference is not so great if we take into account the claims of spear-
masters, recorded by Lienhardt,[29] that their clan divinities have temporal
priority. The divinities of spearmasters may be presumed to be deng and
so sky associated. In this account clan divinities are also said to have
proliferated.

What seems sure is that the Dinka do not associate moral decline with
the presence of strangers or Nuer. Indeed, although the word jur is also
used by the Dinka it does not refer to a category within Dinka society.
The Dinka, less unified and unifiable as a people, do not appear to effect
the equation diel = nath = men (or true men) as do the Nuer. Perhaps it
is because they are less concerned with such unity that they include the
Nuer in their own humanity and sometimes speak of them "almost as though
they were one of the Dinka 'peoples'."[30] This does not mean that the
Dinka lack any equivalent for nath, for they recognize a cultural unity
in jieng.[31]

Despite this reversal the Dinka do, as we have seen, attach an im-
portance to primacy. Where in the tribal area the Nuer oppose diel/rul,
the Dinka oppose bany/kic. The similarities are obvious, the differences
more important. The kic, commoner or, Lienhardt prefers, warrior clans
are in no sense lesser men although the bany/spear-masters have more
"life", nor are the kic strangers. I cannot find any Dinka term having
quite the connotations of rul. The bany are first-comers and thought
of as standing in the MB/ZS relation to the kic; they also have the
peace-making and spiritual power which, among the Nuer is associated with
the kuaar muon. But the spear-master is much more than a superior kuaar
muon as Lienhardt makes clear. The implications of this are discussed
after a brief consideration of the MB/ZS relation among the Dinka.

Given the way in which those Dinka distinctions that parallel Nuer
ones are, at the same time, mediated, it is tempting to hypothesize that
the relations between affines among the Dinka will be both more clearly
defined and reciprocal than among the Nuer. We do not, as yet, have

the material on Dinka terminology and practice that would allow us to enter this field with such confidence. Francis Deng[32] tells us that the bany, at least among the Ngok Dinka, have more wives than the kic whom he refers to as commoners. If this is generally true they would appear to be yet more dependent (and perhaps aware of dependence) upon the kic than the diel are upon the rul. Lienhardt tends to suggest a recognition of mutual dependence in this matter.[33] Again he has somewhere pointed to the MB/ZS relation as providing the model for friendship. It is unfortunately not possible to even speculate from the terminological information recorded by the Seligmans.

It is more fruitful to consider Dinka spear-masters in relation to the aciek - prophet. The spear-master is closely associated with the river and, like the kuaar muon, with ring - flesh. But he is no less certainly associated with the sky and the above: they "are sometimes called bany nhial, 'masters of the above', and are representations of Divinity on earth."[34] Here they may be equated with Nuer prophets as opposed to kuaar muon, just as in other aspects they can be equated with the latter.

When Lienhardt speaks of the transcendance in Dinka thought of what he calls 'experiential opposites'[35] we can see, even from this rather crude comparison, how inappropriate would have been the use of this term, in its strict sense, if applied to the Nuer. The tone of Nuer categorical oppositions is, if one may so express oneself, privative and exclusive. The real, the authentic is opposed to the actual in such a way as to make the Indologist, at least, think of Sankara. The Dinka on the other hand appear to solve their problems by the use of synthetic categories which contain and transcend the opposition. Further examples of the difference would be the important Dinka words ring and wei. For the Dinka both terms synthesize spirit and matter. Among the Nuer they have spiritual associations, but whereas for them ring, apart from its association with the kuaar muon, is only flesh, it is also the divinity of the spear-masters among the Dinka. The Nuer word for chyme - wau (Dinka wei) is important in sacrifice but has material meaning only. Among the Dinka it means not only chyme but also life and breath. It is not surprising that the Dinka remark upon the Nuer habitual recourse to private prayer and compare this with their own emphasis upon the formal and the collective. Their own need is less.[36]

It would, no doubt, be surprising if the Dinka lacked all suggestion of monism. But, to follow the history of Indian philosophy, Dinka monism is 'qualified'. There is an ascending scale of life, a moral hierarchy among the Dinka but it emerges as a continuity, a series of transcendencies, not a series of cleavages. We could not draw, for the Dinka, a series of concentric circles to represent their political cosmology without overlaps to indicate the inclusion of what are simultaneously excluded. The pattern is given in the difference between the Nuer opposition nath/jaang and the lack of anything quite so clear cut among the Dinka who have a series of overlapping categories indicating degrees of humanity: jieng is a subjective reference, thai includes jieng together with other Dinka, the Nuer, Europeans and other peoples known to the Dinka. The two latter classes (not the Nuer) are also jur who are in turn distinguished by colour. There are finally "opprobrious terms for the Azande and other Sudanic-speaking peoples, whom the Dinka seem scarcely to regard as 'people'."[37]

Without speaking of causes I think that we could associate some of these differences in thought with differences in population size and environment. In Evans-Pritchard's time the Nuer numbered about 200,000 which compares with the Dinka 900,000 at the time of Lienhardt's work.

Those marked cultural differences among the Dinka which led the Seligmans
to refer to 'congeries of independent tribes' are not lacking among the
Nuer.[38]   But they are either less marked or less significant to the Nuer.
Either way 'the Dinka recognize that Nuer are able to unite on a larger
scale' than themselves.[39]   For the Dinka, life at any one time is more
settled in the sense that it is not marked by such striking ecological
changes as are found among the Nuer.   On the other hand the Dinka con-
ceive of themselves as a far ranging people over time.   Their own
geography and history contain diversity.

Much has been left out of this account of the Nuer and the Dinka.
Much will appear to have been simplified unpardonably, many exceptions
seen to be ignored.   For these faults I am quite impenitent.   The whole
Nilotic area is, I believe, ethnographically unique in our literature.
Nowhere else do we have such detailed accounts of related peoples making
possible the development of detailed comparison and the theory of com-
parison.   This potentiality is largely the achievement of Evans-Pritchard.
In 1940 he spoke of some future definition of the 'characters of Nilotic
culture and social structure.'   I have always believed that such defin-
ition was possible but clearly it could only be begun by very small-scale
and simple operations.   It is in the hope that the present venture will
provoke more informed and complex comparisons that my tribute is paid to
Evans-Pritchard's inspiration.

David Pocock.

FOOTNOTES

1.   The paper was originally composed for the festschrift to be
     presented to him.

2.   Needham translation, 1963, pp. 103-4.

3.   The Meaning and End of Religion, Mentor Books, 1964, Chapter 2.

4.   R.C. Zaehner, At Sundry Times, London, 1958, p.15.

5.   Reprinted in Essays in Social Anthropology.

6.   I have eliminated the word 'He' from the original to let the emphasis
     fall upon 'it'.   Evans-Pritchard tells us that Mbole is not given a
     personal pronoun but is distinguished from male and female persons
     and shares what he calls 'the animal pronoun' u with 'animals, ghosts,
     .... certain of the heavenly bodies, and a number of vegetables and
     tools which have an especially intimate relationship to human beings'.
     Essays in Social Anthropology, p.199.

7.   Ibid., p.201-2.

8.   Exception has been taken to the use of the word 'hierarchy' here
     and throughout.   I cannot find a better term because the kuth nhial
     are described as superior, more powerful, etc.   and part of the present
     demonstration is to show how we can understand this as an expression
     of a hierarchy of being.

9.   The Nuer, p.214.   I shall henceforth refer to the three parts of
     the trilogy in order of publication as N., KMN., NR.

10. KMN, pp.96, 101.

11. NR, pp.90, 100.

12. NR, p.78.

13. Cf. NR p.173 and NR, p.291.

14.   NR, p.305.

15.   N,231.   See also KMN, p.31.   To avoid further incest
      'ba bak ne kir - it (kinship) is split with a gourd,'

16.   C.G. and Brenda Seligman, Pagan Tribes of the Nilotic Sudan,
      1932, p.207.

17.   N.B.   'There is an opposition in thought between the two
      (kuaar muon and wut ghok)' and 'Mr. Coriat's observation
      suggests that Nuer tend to think of the man of the cattle
      in relation to the above and of the leopard-skin priest in
      relation to the below.   This may be so, but I have no clear
      evidence that the former is classed as a ran nhial, a person
      of the above'.   NR, p.302.   This opens up a line of enquiry
      in connection with the above discussion which might be followed
      up.

18.   Seligmans op. cit., p.221.

19.   In African Political Systems,

20.   KMN,   p.7.

21.   KMN, p.122.

22.   NR, p.10.

23.   R.G. Lienhardt, Divinity and Experience - the religion of the
      Dinka, p.38.   Henceforth, this work and the article "The
      Western Dinka" in Tribes without Rulers,   ed. John Middleton
      and David Tait, will be referred to as DE and TWR respectively.

24.   TWR, p.131.

25.   DE, pp.109-10.

26.   NR, p.34.

27.   DE, p.104.

28.   DE, pp.104-5.

29.   DE, p.168.

30.   TWR, p.108.

31.   TWR, p.107.

32.   Law and the Challenge of Modernization in Dinka Society, an
      unpublished typescript.   I am most grateful to R.G. Lienhardt
      and Mr. Deng for a sight of this very valuable work.

33.   DE, pp. 129, 200.

34.   DE, p.198 fn.

35.   DE, p.158.

36.   DE, p.129.

37.   TWR, p.108.

38.   C.G. and Brenda Seligman, op.cit., p.135.

39.   TWR, p.108.

Meaning and Primitive Religions

Many contributors to this Journal have adopted what might be called the 'fideist' approach to the study of social phenomena. The term 'fideism' connotes the idea that one should be faithful to one's subject matter; that one should adopt a relativist attitude, paying special attention to how participants conceptualise their activities and how they 'create' various ways of looking at their 'worlds'. Two crucial features of the fideist approach are the emphasis on the fact that cultures do not altogether live in the same 'world', and that the major interpretative task is to examine and describe social life as being informed by various types of meaningful realities. Because of this attention to meaning, some have applied the term 'semantic anthropology' to characterise the work of those who adopt the fideist perspective.

The contributors in question have expressed their dislike of those traditional approaches (including both functionalism and structuralism) which direct attention to causal or logical formulations rather than to the meaningful nature of primitive life. So far as one can gather, they have met a twofold response from exponents of older styles of anthropology: on the one hand they have been accused of failing to show what exactly is entailed by the semantic approach, and on the other they have been accused of being too philosophical, or, to use an even more damaging word, of being 'metaphysical'. To an extent, traditional-minded anthropologists have been quite entitled in adopting a negative attitude to the (often young) upstarts who have dared to say that the study of primitive society has not resulted in a proper appreciation of meaningful realities. Some contributors - myself included - have certainly been rather too inclined to engage in polemics. We have perhaps turned too easily to philosophy and have not always done enough to justify the fideist approach by detailed example.

However, our excuse must be that our elders have let us down. The study of how primitive peoples conceptualise their world, realities, states of mind, moral and aesthetic values, the study, in other words, of how phenomena exist in the primitive universe, is impossible without detailed field reports, especially of a dialogue or conversational form. Yet despite the absolute logical primacy of such facts in the study of even the most 'sociological' aspects of primitive life, the great majority of monographs contain only the most piecemeal descriptions of conceptual arrangements. We learn what the tools of ritual are, but we hear very little about what the ritual specialists think of their activities.

In this paper I shall examine one of the few monographs - Godfrey Lienhardt's Divinity and Experience (1961) - which actually portrays what is involved in the fideist approach to religion. My conclusion will be that Lienhardt's work conclusively demonstrates all the advantages of escaping from one type of canon of 'scientific' clarity, rigour, determinability and respectability. To understand this conclusion, however, we must first introduce a distinction between the general 'positivist' and the fideist approaches. For this distinction will allow us to grasp what is entailed by Lienhardt's concentration on 'meaning' rather than on 'function' or 'structure': it will enable us to see why a truly semantic study of primitive religion is imcompatible with a scientific or positivist study.

Most British anthropologists of religion have denied their subject matter a proper reality of its own. Adopting some variety of the positivist scheme (this being the view, in Talcott Parsons' words, that

'positive science constitutes man's sole possible significant cognitive relation to external... reality' (1937:61), they have had to treat religious phenomena as though they refer to scientifically acceptable domains. Consider the work of those belonging to the Durkheimian tradition who argue that the social scientist cannot accept the existence of specifically religious realities (such as God) and who therefore feel they have to relocate the substance of religion. By their reading, ritual and 'odd' beliefs do not really refer to the states of affairs maintained by participants; instead, they refer to social institutions, processes and values.

Whatever the plausibility of the positivist argument, it results in semantic impoverishment. Religious phenomena are accorded meaning by illuminating them in terms of what are essentially alien realities, and it takes no great stretch of the imagination to realise that this course has distracted anthropologists from understanding religion itself. The consequences of the 'theory-dependent' course of reducing the 'religious meaning' of religious beliefs to something other than the significances attributed by social participants is clearly visible, for example, in Richards' remark that 'They [field workers] have studied religious belief and ritual mainly through the behaviour of the people in these [small scale] communities....[they] have restricted their study of ritual to those aspects which bear on social structure....' (1967:293). A strange restriction, one would have thought, to be imposed on the anthropology of religion. Indeed, to the extent that field workers have interpreted religion in terms of the theory-dependent relationships with social structure, they have run the very grave risk of talking about something other than primitive religion: religion is very largely a participant construct; participants do not simply reduce their religious life to social structure; therefore when anthropologists make the reductionist step, they radically distort the participant's universe of discourse and their meaningful realities.

Characteristically, when positivists attempt to justify their procedure they claim that social scientists should not engage in 'theology'. Thus Leach suggests that the 'answer' given by Catholics when asked to explain the birth of Jesus is not 'the sort of answer which should be offered by professional anthropologists in the course of their professional duties'. Replies of the type, 'We know that virgins do not conceive; but we also know that the Holy Mother of God was and ever shall be an immaculate Virgin' are unsatisfactory because 'We are social analysts not theologians'. 'From an anthropological point of view', continues Leach, 'non-rational theological propositions can only serve as data not as explanation' (1969:103).

Leach's rejection of theology is surely correct when it entails the rejection of the view that one should examine religious phenomena in terms of the 'meanings' infused by what he calls the 'supernatural sender' (ibid:9). However, Leach (and other Durkheimians) are so keen to reject the fideistic approach that they do not appear to realise that there are many types of theology. Ramsey, for instance, does not deny that the full significance of the religious way of life is consequential upon what he calls the 'penny dropping', but he still insists on the value of conceptual or philosophical analysis, tracing the logical nature of religious language to show how this logic facilitates the distinctiveness of religious styles of meaning. The positivists, in other words, are so persuaded by the argument that the reality of religion must be relocated if it is to be put under scientific scrutiny that they polemically equate theology with the 'meaning

lies with the Word of God or act of 'faith' argument, and then reject
theology in toto. Accordingly, they do not realise the benefits of
fideistic (or theological in the sense of theology as conceptual analysis)
examination.

With these considerations in mind, it comes as something of a shock
to find Lienhardt claiming that for analytic purposes Dinka 'Powers' must
be regarded as representations of realities more accessible to a uni-
versal rational knowledge than they need to be in the Dinka view of them..
I have described them for the most part as the Dinka themselves understand
them; but in this chapter I try to give a different account of them, not
now as ultra-human "beings" which might form the subject-matter of a
Dinka theology, but as representations (or as I here prefer to call them,
"images") evoked by certain configurations of experience contingent upon
the Dinkas' reaction to their particular physical and social environment,
of which a foreigner can also have direct knowledge' (1961:147).
Lienhardt, in other words, appears to be following the positivists,
arguing that Dinka religion should be understood in terms of social and
physical experiences which we can share, rather than in terms of the
Dinkas' own religious entities or realities, namely the 'Powers'. Yet
we are treating his work as a classic example of anthropology as the
study of meaning.

Perhaps the first thing to notice is that Lienhardt formulates his
rejection of interpretation in terms of 'ultra-human beings' in a very
narrow and precise fashion. His formulation has two main aspects: on
the one hand we, as Westerners, cannot understand Dinka beliefs from
within (or theologically) because 'To the Dinka the Powers are known by
personal encounter, as living agents influencing their lives for good
or evil....but no European actually encounters DENG, GARANG, or the
other Powers as the Dinka claim to do'. And on the other hand, the
Powers 'cannot be understood by us if they are regarded as referring
to theoretical "beings" whose existence is posited, as it were, before
the human experience to which they correspond....I have suggested that
the Powers may be understood as images corresponding to complex and
various combinations of Dinka experience which are contingent upon their
particular social and physical environment. For the Dinka they are the
grounds of those experiences; in our analysis we have shown them to be
grounded in them, for to a European the experiences are more readily
understood than the Powers, and the existence of the latter cannot be
posited as a condition of the former' (ibid:147,169-70; my emphasis).

We can now locate Lienhardt's work with reference to our distinc-
tion between positivism and fideism. One cannot say that Divinity and
Experience is entirely free of the positivist spirit: he tends to
relocate the reality of Dinka religion by shifting the emphasis from
ontologically sound 'ultra-human beings' to those experiences to which
Westerners can respond. At the same time, however, his rejection of
a theological appreciation is limited to a rejection of the 'meaning
is dependent upon the acceptance of irreducible religious experiences
or messages or Powers' position. The scope of semantics is assured
because he specifically refuses to be drawn into the extremes of the
Durkheimian approach (see ibid:10,131,165-6) and because he does not
reject theology as conceptual analysis. Concerning the second of these
points, we have already indicated that Lienhardt is perfectly prepared
to investigate the Powers in terms of how 'the Dinka themselves under-
stand them', and concerning the first point, we might conclude that his
semantic approach is greatly encouraged by his insistence that the
experiences which offer meaning to the beliefs and activities under

consideration are only of a 'weakly' positivist order.

Lienhardt's work lies between the extremes of theology and positivism. His rejection of one type of theological interpretation does not result in the collapsing of the significance of Dinka religion according to the fashion of strict Durkheimians for instead of being content with the simple theory that religious phenomena are merely a way of 'talking about' social relationships (and functioning to maintain the social order), he is concerned to show the cultural depth of religious life.

To clarify this point, and to suggest what we mean by the term 'weakly positivist' we might reflect on the following passage from his work:

> 'What is represented...in the oral rites, is what the Dinka see as the truth of a situation - an existential truth, if one may so call it, and not the truth of specific facts in space or time....Like prophecies, the ceremony eventually represents as already accomplished what the community, and those who traditionally can speak for them, collectively intend. Thus the masters of the fishing-spear eventually state that they have freed the man from the agent which is troubling him; ideally, he should get up at once and return to normal health and vigour, and this is what sometimes happens in accounts of idealized sacrifices. The "patient" becomes "convalescent" in the full etymological sense of these terms. In fact, some delay is expected, and the delay shakes no faith. For the sacrifice is its own end. It has already created a moral reality, to which physical facts are hoped eventually to conform.

> We have seen that the main oral rites, those at sacrifices, assert by a combination of assertions of control and admissions of weakness a relationship between freedom and contingency in human life, in which freedom appears eventually as the stronger. Human beings explicitly assert their ability to act upon the conditions which they constantly passively experience. It is of particular importance, in this regard, to recognise that the sacrificial rite is first and foremost an act of victimization. A strong and active beast is rendered weak and passive so that the burden of human passiones may be transferred to it (ibid: 250-251).

The analysis is far removed from such reductionistic arguments as, 'sacrifice functions to restore social equilibrium when people are threatened by illness'. Our attention is not directed to a theory-dependent (and thus strongly positivistic) view of religion within the mechanistic and determinable (if not measurable) social process. Instead, our attention is drawn to states of affairs which, to an extent at least, lie beyond the positivist frame of reference. To make this claim is to raise awkward philosophical difficulties: for instance, are we (and Lienhardt) entitled to argue in terms of the naturalistic fallacy, to conclude that there exists 'existential truth' which is not 'the truth of specific facts in space or time'? Fortunately for us we can rest our argument on the fact that strict sociological symbolists quite clearly do not feel at ease with such realities

or 'truths'.   As positivists, they feel obliged to introduce, via their
relocatory procedure, truths of a publicly verifiable (space-time) variety.

By rejecting this position, we have seen how Lienhardt has greatly
facilitated a much broader appreciation of religious phenomena than is
to be found in the works of those who belong to the Durkheimian tradition.
He does not altogether disregard the Durkheimian idea of projection -
his emphasis on social experience is quite evident in such remarks as
'clan-divinities represent....the ideal and permanent values of agnation
for the Dinka', and, 'when ancestors more recent than the founding an-
cestor of a whole clan have been for a long time separated in different
parts of the country, their descendants, as groups, are differentiated
in a way which is reflected in their different range of divinities'
(ibid:135,120 my emphasis) - but by utilising the theory as a key to the
existential and moral significance of Dinka religious beliefs rather than
as a key to Dinka social organisation, he successfully escapes from the
confines of strict positivism.

Not surprisingly, the most interesting aspects of Divinity and
Experience are those where Lienhardt entirely transcends the general
positivist framework.   I am thinking especially of those passages where
he attempts to lead us into the conceptual framework revolving around
Dinka ideas of man-world relations.   His examination of Dinka notions
of personality, world and reality are of central importance for at
least three reasons.   Firstly, the analysis conclusively demonstrates
the extent to which semantic anthropology has very little to do with
scientific reductionism.   Secondly, it provides the key to many
features of Dinka religion, this key being relatively distinct from
the one provided by Lienhardt's use of 'experience'.   And finally, his
analysis is of great value because it can serve as a paradigm case of
the study of meaning: it suggests what is involved in tracing the
rationale of the 'deep' beliefs which inform social life;   it suggests
how difficult it is to engage in what surely must be the primary task
of anthropology, namely the exegesis of 'alien' ways of conceptualising,
in fundamental fashion, the various types of entities and realities
which might be said to exist in the world.

Discussing the 'difficult question of differences between Dinka and
European self-knowledge', Lienhardt argues that,

> 'The 'Dinka have no conception which at all closely corresponds
> to our popular conception of the "mind", as mediating and, as
> it were, storing up the experiences of the self.   There is for
> them no such interior entity to appear, on reflection, to stand
> between the experiencing of self at any given moment and what
> is or has been an exterior influence upon the self....It is
> perhaps significant that in ordinary English usage we have
> no word to indicate an opposite of "actions" in relation to
> the human self.   If the word "passions", passiones, were
> still normally current as the opposite of "actions", it would
> be possible to say that the Dinka Powers were the images of
> human passiones seen as the active sources of those passiones'
> (ibid:149,151).

When most anthropologists have been faced by ethnographic situations
where central Western concepts are either absent or differently located
by reference to one another, they have tended to ignore the implications

of thei. indings. Some indeed have even failed to report any findings at all ( ), for example, have given accounts of what happens to the concepts or states of affairs) 'love' and 'jealousy' in the context of polyandric or polygynous marriage systems?). Horton, to mention just one example of someone who has at least recognised the fact that other cultures often have distinctive conceptual configurations, does not appear to know how to handle his finding that many African societies do not possess the modern distinction between 'mind' and 'matter' (1970:157). This rather depressing situation can surely be attributed to the difficulties of such exegesis, difficulties whi•h can often be attributed to the fact that what is at stake is the relationship between language and reality. In other words, what is at stake is the problem of determining the extent to which language can create its own reality: to cite a now classic question, 'Is belief an experience?'.[1] Again, is the existence of love or jealousy dependent on the existence (in any particular culture) of these notions,or are they extra-linguistic entities? Moving somewhat closer to Divinity and Experience, what are we to make of those modern theologians who appear to treat the reality of God in terms of the language game of God-talk? And finally, this time taking an example which bears directly on Lienhardt's work, exactly what perceptual, experiential, existential, conceptual, moral and even ontological issues are dependent upon the absence of 'our popular modern conception of the "mind" as mediating and, as it were, storing up the experiences of the self'? What is it to maintain, as Lienhardt does, that 'Without these Powers or images or an alternative to them there would be for the Dinka no differentiation between experience of the self and of the world which acts upon it' (1961:170)?

Since this paper is only designed to emphasise the possible scope of a semantic anthropology, I willingly excuse myself from a general discussion of these most complex matters. Let us instead outline some of the ways in which Lienhardt gives substance and meaning to Dinka ideas of self-knowledge:

MEMORY: For the Dinka, past experiences are not mediated by what we call 'mind'. It follows that 'what we should call in some cases the "memories " of experiences, and regard therefore as in some way intrinsic and interior to the remembering person and modified in their effect upon him by that interiority, appear to the Dinka as exteriorly acting upon him, as were the sources from which they were derived' (ibid:149). To use a word developed by certain Wittgensteinian philosophers, this state of affairs affects the 'grammar' of several Dinka notions associated with the act of 'remembering past experiences'. Dreams are not 'only' dreams; the strong impressions Dinka might receive on visiting, to use Lienhardt's example, Khartoum, are not simply thought of as the 'influence' of the place; what for us is only the 'prompting of a guilty conscience' is not so regarded by the Dinka; and, perhaps most fundamentally of all, what we might call the 'immanence' of spiritual activity cannot be conceptualized in quite the same way by the Dinka (see ibid:149-150). In all these examples, what are presumably in some sense distinctive states of affairs (such as are denoted in English by the terms 'memory', 'dream', 'guilt' and so on) are conceptualised by the Dinka in a different way because they lack our notion of 'mind'. With their religious entities functioning, according to Lienhardt's analysis, as 'the images of human passiones seen as the active source of those passiones', the Dinka seem to conceptualise memories of past experiences in terms of religious phenomena.

In other words, granted the basic premise that the Dinka treat their 'memories' as affecting them in the fashion of passiones, the only way they can ensure a degree of control over the automatic pressures of the external world (or as Lienhardt says, effect a 'differentiation between experience of the self and of the world which acts upon it') is by 'imaging' their memories. And this they do in a religious way: dreams are associated with free divinities (ibid:57); Khartoum is said to follow the Dinka who have lived there for some time as 'divinities are said to "follow" those with whom they have formed a relationship' (ibid: 149); the fetish MATHIANG GOK 'works analogously to what, for Europeans, would be the prompting of a guilty conscience' (ibid:150),[2] and, to give one more example, illness and suffering are conceptualised in terms of something 'akin to "individual totemism" or "nagualism" (ibid:151).

CONTROL OVER EXPERIENCE: Mention of the Dinkas' attitude to suffering allows us to complete the extract we earlier gave concerning Dinka sacrifice. Lienhardt concludes with the words, 'It [the sacrificial beast] suffers vicariously for those for whom sacrifice is made, and men, thus symbolically freed from the agents which image their sufferings, and corporately associated with each other and with the agents which image their strength, proclaim themselves the creatures whose deliberate action prevailed over the first master of the fishing-spear and received his gift of "life"' (ibid:251). Imaging their experiences, which is another way of saying that the Dinka 'extrapolate', 'transfer', 'reflect', or 'represent' them in terms of religious entities (ibid:150-1, 165-6), ensures that 'there arises for them....the possibility of creating a form of experience they desire, and of freeing themselves symbolically from what they must otherwise passively endure' (ibid:170; See also p.291). To offer a somewhat crude generalisation, we of the West have great freedom and control: our 'minds' allow us to act on the world, often in a scientific manner. The Dinka, on the other hand, neither have 'minds' nor have a scientific response to illness. Refusing to entirely bow to the passiones, they so to speak create a 'secondary' mind: much of the interest of Divinity and Experience lies in the way in which Lienhardt traces the interplay between the control of religious entities over human affairs and the mediated way in which men can control their experiences through the sacrificial process. To an extent at least, religious entities function as 'mind', but the differences between the two ways in which both the Dinka and ourselves effect a distinction between 'a subject and an object in experience'(ibid) suffice to alter the 'grammar' of such notions as freedom and control. The consequences for political anthropology are obvious, this suggesting the primacy of semantic anthropology over more 'sociological' endeavours.

BELIEF: There are many other implications of Dinka conceptualisations of self-knowledge, but I want to conclude by mentioning just one more. Our discussion of 'belief' will then act as a convenient point of introduction to the conclusion of this paper: the problems raised by the relationship between Dinka notions of self-knowledge and Lienhardt's emphasis on 'experience' as a way of interpreting their religious phenomena.

According to Lienhardt, it is 'not a simple matter to divide the Dinka believer, for analytic purposes, from what he believes in, and to describe the latter then in isolation from him as the "object" of his belief' (ibid:155). As we have seen, the Dinka attach more

importance to the role of the 'world' in acting on them than do we of
the West (hence the fact that 'in ordinary English usage we have no word
to indicate an opposite of "actions" in relation to the human self').[3]
We have also realised that in so far as the Dinka distinguish between "a
subject and an object in experience', they do so via religious means
(or the imaging process) which allow much greater interplay between
human action and religious passiones than is the case with our predom-
inantly verificatory and manipulative relationship with reality. Taking
these two considerations together, we realise that the Dinka do not, at
least to the same extent as us, live in a world where 'belief' would be
important. As Lienhardt puts it, 'Their world is not for them an object
of study, but an active subject; hence the world (piny) as a whole is
often invoked for aid along with other Powers' (ibid:156).

The world acts on the Dinka: hence Lienhardt's emphasis on the
notion passiones. And hence also his claim that the notion 'belief'
is of dubious value when applied to their universe. But there is more
to this question than simply pointing to the interplay between actions
and passiones, and it is at this point that we can return to some of
the considerations with which we began. What we can now do, in other
words, is suggest how Lienhardt's analysis of Dinka ideas of self-
knowledge has encouraged him to use 'experience' as a key to their
religion. In conclusion, therefore, I hope to show that whilst there
is undoubtedly some connexion between his two keys to Dinka religion
(namely 'experience' and ideas of self), his appeal to the former key
is not quite so successful as his appeal to the latter. I should point
out that the semantic issue here at stake is the absolutely crucial one
of how the Dinka conceptualise their various 'realities'.

First, what exactly is the connexion between these two keys? It
is to be sought in Lienhardt's claim that Dinka Powers are 'the images
of human passiones seen as the active sources of those passiones'.
'Experience' is important because it provides the initial grounds of
the passiones; and Dinka theories of self-knowledge enter into the
picture because, as should now be obvious, the Dinka articulate their
distinction between the self and the world in such a way as not to
encourage our own clear-cut idea of believing in something.

Concerning these points, Lienhardt continually emphasises the
fact that 'Statements about the divinities, as represented in hymns,
are imaginative and creative, not dogmatic or doctrinal. There is no
formal orthodoxy, and any imaginative association which does not contra-
dict the general configuration of associations for particular divinities
in the mind of any Dinka can be accepted as an insight into the nature
of the divine' (ibid:91). Again, discussing whether or not the Dinka
have to face the problem of evil, he concludes that Divinity and
MACARDIT 'are not conceived as "beings" actively pitted against each
other, as experiences in themselves cannot actively oppose each other.
The difference between them is not intrinsically in them but in the
human experiences they image' (ibid:159). It follows that by treating
Dinka religious entities as 'experiences' (or, perhaps more accurately,
as being about experiences), Lienhardt adds plausibility to his
theoretical assumption that Dinka religion is best interpreted 'as
representations of realities [i.e. experiences of the natural or
social world] more accessible to a universal rational knowledge than
they need to be in the Dinka view of them'. What is entailed in this
is made quite obvious in the following quotations:

. it is in the representation of extremely complex
configurations of moral and physical experience, the elements
in which are not distinct from each other but are embedded,
as it were, in extensive metaphors, that the Powers have
their force' (ibid:161).

'But to attempt to produce an account, however lucid and
ingenious, of a kind of Dinka "creed" and pantheon, would
be to start concealing what, as I see it, is the clue to
our understanding the facts - that is, that Dinka religion
begins with natural and social experience of particular kinds'
(ibid:96).

Now it might well be the case that Lienhardt is perfectly correct
to emphasise that Dinka religious phenomena are pervaded by 'experience'
rather than resting on 'logical or mystical elaboration of a revealed
truth as are our own theological considerations....' (ibid:156), this
suiting his rejection of a 'theological' understanding. But one cannot
help suspect that however much his desire to apply the key of 'experience'
is facilitated by the evidence provided, amongst other things, by Dinka
notions of self-knowledge, it results in a semantic distortion of how
the Dinka themselves regard their religious phenomena. For according to
his analysis, the Powers seem often to become experiences or extensive
metaphors, a fact which does not fit easily with his assertion that 'To
the Dinka the Powers are known by personal encounter, as living agents
influencing their lives for good or evil'.

The point I am making is this: a semantic anthropology cannot
afford to make a simple minded distinction between how participants re-
gard their religious phenomena and how the outside observer might be
prompted to construe them in theory-dependent (or positivistic) terms.
This might appear to be a large claim, but it rests on the simple consid-
eration that to say 'x· people's religious entities are merely symbolic
expressions or metaphors of social or physical experiences' is not to say
anything much about what must be the crucial concern of a semantic anth-
ropology (namely 'participant meaning') if the participants themselves
assert, for example, that their religious entities are 'living agents'.
Lienhardt, I should hasten to add, cannot easily be criticised on this
score, if only because he is surely correct in using 'experience' as a
key to Dinka religion (one can hardly deny that we as Westerners must
find some way of interpreting phenomena which are alien to us, even though
such an interpretation might run contrary to certain participant assertions).
· Nevertheless, even if it be admitted that it is justifiable for Western
anthropologists to 'add' certain things to participants beliefs in order
to satisfy their own canons of intelligibility, we should still not lose
sight of the limitations of such an approach.

It seems to me that what we require is a form of 'two-way' intell-
igibility. On the one hand, Lienhardt gives much evidence to suggest
that many aspects or features of Dinka religion can quite justifiably
be interpreted in terms of the 'experience' model: 'Divinity is thus
comprehended in and through natural experience, and not merely as a theor-
etical force producing the order of the world from without' (ibid:158).
Appropriately applied, this model seems to bridge quite satisfactorily
the gap between what understanding must be for us, and what understanding
religious phenomena is for the Dinka. It can also sometimes be applied
to illuminate for us certain features of Dinka religion which the Dinka
themselves do not regard in quite the same way (the Dinka regard their

Powers as living agents, and, for all we know, do not possess the term 'extensive metaphor'. But this is not to say that we cannot acquire understanding by treating the Powers as gaining some of their 'force' from their metaphorical relationship with 'experience'). On the other hand, however, there comes a point when we ask different questions of the Powers, and it is at this point where we might need another way to intelligibility.

To develop this, we can take Lienhardt's claim that 'It is not suggested, of course, that the Dinka apprehend their beliefs in this way [in terms of the imaging process, including the process of "separation"]'. It is true that Lienhardt continues by giving an example, pertaining to the notion atyep, which suggests how close they are to our notion of 'image', but the fact remains that the 'experience-imaging' model is not especially appropriate if we ask the question, for instance, what type of reality do the Dinka themselves attribute to their Powers, and what exactly do they have in mind when they call them living agents? It is surely significant that Lienhardt has little to say on these matters, and that what he does say is not entirely consistent (compare, in this respect, his claims that the Dinka live in a 'single world', that the Powers 'operate beyond the categories of space and time which limit human actions', and that the Powers are living agents (ibid:28,147).

There is no single way of interpreting Dinka (or any other) religion. From a semantic point of view, the 'experience-imaging' model can be regarded both as an heuristic device and as a substantial replication of certain features of Dinka religion. It affords one perspective and answers one set of questions. Other features are perhaps best treated in other terms: in the example just raised, understanding the type of reality of Powers would surely entail establishing what the Dinka regard by 'space' and 'time', what the notion of 'living agents' has to do with these notions, and how it is possible for the Dinka to live in a 'single world' when this world is so disrupted by space/time considerations. It is perhaps paradoxical that Powers are, from one point of view, intelligible in terms of 'experience' and 'metaphorical extension' of the imaging process, whilst from another perspective they become real living agents and all that that entails, but it should be born in mind that if we desired a full understanding of, for instance, our notion 'mind' we would be faced with a situation where: a) scientists, Christians and others would all give different accounts, and b) where different questions would so to speak articulate different usages (and therefore meanings) of the notion.

Thus my only criticism of Divinity and Experience is that Lienhardt does not seem to fully appreciate the advantages of what I have called 'two-way' intelligibility. This is to say that he does not fully free himself from the 'one-way' intelligibility provided by the general positivist approach: he lets his emphasis on the 'experience-imaging' model take precedence over asking, in a non-positivist fashion, questions about how the Dinka conceptualise the reality of their religious entities. I cannot push this criticism very far because Lienhardt frequently engages in remarkably sensitive analyses of features of Dinka religion, such analyses not always being couched in terms of the key provided by 'experience' (see for instance, his discussions of such Dinka notions as 'truth', 'justice', and 'respect' (46-7, 139-40)). Nevertheless, we have argued that his handling of the nature and reality of Powers is hampered by his interpretation of them solely as images. The Christian God has been interpreted by some theologians as a symbolic expression of existential depth or of the Unknowable,

but in an analagous vein to our criticism of Lienhardt's work, they also have been criticised for neglecting the propositional or dogmatic nature of God-talk. Can we realistically suppose that Dinka religion lacks dogma to the extent suggested by Lienhardt? Or are we rather to infer that he has treated it like this in order to facilitate his key of 'experience'?

Whatever criticisms one might make of <u>Divinity and Experience</u>, they all take a constructive form. To question Radcliffe-Brown's interpretation of primitive religion is to engage in a futile exercise, there being no worthwhile returns to one's endeavours. To question Lienhardt's work, on the other hand, is to raise the type of issue which most anthropologists of religion have consistently ignored. How do alien concepts cohere together? How do the Dinka express, organise and control their experiences? What are the consequences of their lacking our popular concept of 'mind'? How do conceptual shifts work (is there an entity called 'guilt' which we conceptualise in one way but which the Dinka conceptualise differently)?. And, just to mention some issues which we have barely remarked on, how do the Dinka solve the problem of evil, what has their religion to do with moral life, and how exactly do their religious entities relate to and manifest themselves in the affairs of man? Instead of drearily trotting out the sociological symbolist,intellectualist or structuralist interpretations of primitive religion, should we not instead be attending to these essentially semantic and essentially profound questions, especially those which are raised by the ways in which man's various relationships with the world (characterised by terms like 'verification', 'expressive', 'experience', and 'events')[4] give rise to equally various styles of meaningful relations?

To summarise the approach I am advocating, the fideistic study of primitive cultures does not simply entail grasping that notions of the 'witchcraft', 'guilt' and 'intention' level of description are quite variously articulated in different cultures. More fundamentally, we have to do with those categorisations of the world which lie behind, and inform, such institutions as witchcraft, sacrifice or courts of law. Imagine a culture which supposes that phenomena ranging from gods to material objects are thought of in terms of the idea of 'force'. Clearly, this notion will affect, amongst other things, how we interpret 'magic', the idea of being free to act in certain ways, and the relationship between spiritual and everyday affairs. But it is also important to realise that the way in which the key notion 'force' generates distinctive relationships and patterns is very largely a consequence of its 'reality' status. Thus if according to participant criteria 'force' is opposed to some notion of everyday mechanical causality, we would be inclined to seek its conceptual implications at, say, the moral level. If, on the other hand, the notion has a reality status which obscures our distinction between 'causing someone to do something' and 'causing a car to go', we would somehow have to trace a different set of conceptual implications (we might find, for example, that the notion has an existential status, having to do with life force, this explaining why it obscures our distinction between what can losely be called physical and moral causality).

Although I do not find it easy to make my point, examples such as these suggest that the way in which fundamental notions organise conceptual arrangements and social activities has much to do with their reality status; whether or not they concern attitudes (such as worship), moral propositions, 'dream times', the publically observable and verifiable world, poetic insights, transcendental cosmologies, immanent powers of a supposedly automatic nature, and so on, By treating Powers as experiences, Lienhardt has been able to trace a set of relationships between many other Dinka concepts. It is sad that British social anthropologists have been so obsessed by positivist reductionism, for if this had not been the case we might today be able to contrast the Dinka situation with the conceptual patterns associated with such basic reality constructs as 'life is an illusion', 'men do not have souls', 'all is alive', 'all is force', 'religion is love', 'only God knows the truth', and, to give a final example, don Juan's state of 'seeing'.

Notes

1. Needham's <u>Belief, Language and Experience</u> is rapidly acquiring the status of a classic in the field of semantic anthropology; all the more so because of Needham's prior allegiance to the reductionistic and crudely logical style of analysis known as structuralism.

2. Lienhardt has several interesting things to say about the relationship between, for example, witchcraft, morality and states of mind such as envy. Thus: 'An envious man....not recognising the envy in himself, transfers to another his experience of it, and sees its image in him, "the witch"' (ibid). Such analysis of witchcraft reminds one of Crick's outlined reconceptualisation in terms of 'moral spaces' (see J.A.S.O. Vol. 4, no.1).

3. To emphasise this point, we can remember Lienhardt's remark, 'We see the difference between the underlying passivity of the Dinka in their relation to events, and the active construction which we tend to place upon our role in shaping them' (ibid:235).

4. In an earlier article in J.A.S.O. (Vol.3 no.3) I have attempted to trace how don Juan's utterances can be interpreted in terms of a distinctive relationship with 'events'; I suggested that his discourse somehow belongs to the level of 'events', this explaining why so many of Castaneda's questions were inappropriately addressed.

<div align="right">Paul Heelas.</div>

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Power and the Big Man in New Guinea

As anthropologists we tend to be confused at times as to whether we ought to be ethno-sociologists or empirical philosophers. Falling uneasily somewhere between two opposed camps, we continue to look for 'data', but are often unclear what to do with it. We must make a stand. In this paper I argue for the "primacy of semantic anthropology over more 'sociological' endeavours" (Heelas: "Meaning and Primitive Religions" J.A.S.O: this issue). A sociological approach to political realities tends to begin with a search for 'leaders, power, authority, anarchy' and so on, fitting the existential situation into the categories formed by political science. Semantic anthropology first goes to the indigenous interpretations and then decides how best to translate them, often leading to a proliferation of categories, some of which we may immediately recognise, others of which will stretch our powers of imagination. Given the proliferation of cultures and their particular ways of seeing the world we must be prepared to accept such a proliferation of categories. Being prepared to look at other peoples' ways of seeing the world is a healthy step away from the mechanistic model of man implicit in much of the sociological type of anthropology. Behaviour is rule-governed, but we must first of all find the rules which govern a particular people's behaviour; and these are unlikely to be rules couched in terms derived within a highly stratified, industrial society if the culture in question consists of a few thousand people living close to the soil.

In this paper I wish to illustrate some of the problems surrounding the analysis of power structures in New Guinea. My argument generally is that works on concepts such as the 'New Guinea big man' have been largely methodologically misguided, given our present state of awareness about certain facets of New Guinea thought. We must know more before we can generalise. "...social anthropology is comparative or it is not a discipline at all" (Needham 1967:447n.), but we must make sure that what is being compared is susceptible to comparison, and if so, on what level. In situations of social change, such as those which prompted this debate, the 'before' and 'after' may be so very different that a completely different type of analysis is appropriate to the 'before' from that which seems adequate for analyzing the 'after'. Any meaningful comparison is not a simple matter of a question of the "were the old leaders more powerful than the new?" type, but a matter for detailed and painstaking conceptual analysis. The 'before' and 'after' debate I am referring to in this paper centres upon two papers (Brown 1963 and Salisbury 1964) which I present here in such a simplified form to deem it necessary to direct the reader to the sources to avoid misrepresentation. For the present purposes, however, the main threads are drawn out briefly.

The introductory remarks of Brown's paper will set the scene nicely: "It is a commonplace in the study of changing political systems that the imposition of alien rule restricts the power of traditional authorities. Yet I am going to claim what may seem the reverse - that alien

rule gives new powers to the native authorities it establishes....(Brown 1963: 1). Whereas in the typical New Guinea society leaders are subject to constant control by the group within which they operate, never being sure that "his opinions will be respected, that his orders will be obeyed" (op. cit.: 6), the new 'leaders', the government appointed luluais, were directly responsible only to the local officials, such as the District Officer, and often wielded significant power.   From a situation of "anarchy", new leaders were in a position to become "arrogant satraps" (op. cit.: 2).   Salisbury, in his supplementary interpretation", argues that, in New Guinea, "Although the indigenous ideology was one of democratic equality and competition, the empirical situation at this time was one of serial despotism by powerful leaders" (Salisbury 1964:225). The "ideology" of New Guinea politics was, he agrees, one of equality, but there were powerful leaders, characterized by him as "directors", who were despotic and whose emergence "can be regularized within the political structure" (op. cit.:237). Until the government intervened, appointing lulais whose status he describes as "a bureaucrat rather than a satrap" (op. cit.: 232), indigenous leaders were often to be seen as despots and to prove his point he cites the careers of three such men who held sway over their fellows for some time;   and stories collected from Tolai of indigenous leaders which commented on "their 'badness' and unpleasant natures" (op. cit.: 226).   The advent of not only government control but also of economic and social innovation restricted the power of the despots and, although the luluai could become powerful, there was adequate machinery for controlling his activity in the form of the District Officer who, "aware of the dangers of satrapy" will "lean over backwards to be legalistically correct" (op. cit.: 229).

One of the central criticisms which Salisbury makes of Brown's approach centres upon what he calls a confusion of "political reality" and "ideology" which leads to mistaking "functional anthropological reconstruction for fact" (op. cit.: 225).   Of course, there is a danger in any anthropological investigation that one's view of the realities of a situation will be over-influenced by the indigenous comments on that situation.   Categories may not be hard and fast, but mere descriptive devices open to a considerably wider interpretation than is given by the people.   And at the level of a search for political 'facts', we may wish to avoid a too literal reliance upon the statements of our informants who are not, it is clear, social scientists, and who may lack the desired precision in their own political philosophy.   What Salisbury is saying is that if we wish to ascertain whether the advent of the administration had a certain effect upon the political realities in New Guinea societies, we must take care to compare not ideologies, but personalities, histories, facts, and data.   Only in this way can we satisfactorily begin to make an objective assessment.

Unfortunately, there are severe limitations to this approach, not the least of which is that it is an almost unattainable ideal in itself. Salisbury himself tends to drift into an account of the 'ideology' of government policy in the way he deals with the powers and limitations of lulais.   Throughout his account of the position of the luluai Salisbury

uses phrases which lack the sort of detailed factual backing one would expect
from one whose approach is to establish "reality" over "ideology". We
are told of the "official position" of the luluai; we hear about what is
"prescribed by government ordinance"; that the luluai has "no statutory
authority to adjudicate " certain disputes; "as far as the administration
is concerned" the luluai acts as a mediator; and officially "the luluai
is not the judge in own case". As for the administration, the District
Officer "is aware of the dangers of satrapy", "must weigh the dangers
against the advantages" and will "lean over backwards to be legalistically
correct" (op. cit.: 228-9). It would, perhaps, be tedious to demand
that Salisbury should provide concrete evidence that the official position
reflects the "political realities", and I do not think statistical
evidence would be of any real value in deciding one way or the other.
Yet where Brown has been accused of failing to differentiate between what
Salisbury must see as a model of political relationships and the actual
careers of individuals within the political arena exemplified in that
model, one is entitled to demand that in a reply, a critic should himself
make a clear distinction between his own model and the 'factual' situ-
ation which he is offering as evidence for a "supplementary interpretation".

What has gone wrong, however, is not that Salisbury has offered us a
more precise method of gauging the respective powers of pre-contact
leaders and administration appointed luluais and then failed to match the
ideal with his own rendering of the situation, but, rather, that the
question itself is not so susceptible of scientific analysis as one may
be led to believe from Salisbury's second paragraph. One is increasingly
led to the question of what sort of evidence will establish one view over
the other. How can one be sure that the pre-contact leader who was
prominent for many years was really popular or unpopular, and whether his
power was really great or 'merely' legendary? Salisbury's attempts to
establish the 'facts' on this score rely upon the only data a frustrated
historian has access to in an essentially non-literate society, namely
oral tradition, and he points to the hopeful fact that the local Tolai
informants kept "a lively tradition of the history of the 1880's."
However, "stories" have to be dealt with in a far more subtle way than
they are treated by Salisbury in this paper.

In an attempt to illustrate the government appointed Luluai's limi-
tations Salisbury writes that a luluai operates as a mediator in disputes
within native custom "of which no official cognizance is taken" whose job
it is to suggest a "compromise which is enforceable only to the extent
that a party which does not accept the compromise then becomes guilty of
an offence against Native Regulations such as disturbing the peace"
(Salisbury 1964: 228). Salisbury's use of the word "only" seems to
ignore the significant point that, even if we are here concerned only
with policies, the new 'leader' has a significant sanction which his
predecessor lacked. Whereas in the indigenous "ideology","no leader can
be sure that his opinions will be respected, that his orders will be
obeyed...." (Brown 1963: 6) the luluai who fails to convince disputants
has a sanction against those who do not "respect" his "opinions".
Granted that the District Officer will "lean over backwards to be

legalistically correct", the disputant will go before the kiap as "guilty of an offence against Native Regulations" (Salisbury 1964: 228). which, even in a situation of common respect, trust and understanding between Kanaka and administrator, is an unnerving experience for any defendant. And where there is a fundamental lack of respect, trust and understanding in the relations between the black man and his 'superiors', the threat of being brought before an examining District Officer is alone suffic-ient force to grant the luluai a great deal of power in situations of dispute.

The activities of Bumbu, a luluai at Lae (Hogbin 1946: 45-6) could be seen as a balance to the activities of the "despots" cited by Salis-bury (1964: 228), and we could spend time deciding whether he was an "exception" or whether his "emergence can be regularized within" the administrative situation. It is true that it was not government policy to give power to the luluai for it to be misused in the way Bumbu misused it, but neither is it the policy of the indigenous political ideology in New Guinea for people to be subjected to the arbitrary whims of "despots". But the real answers to the problem will not be found by counting heads - piling up examples of luluai corruption, by adding up how many years a big man held sway against the average term of office of a luluai, by giving too much credence to traditional tales of "bad" leaders in the pre-administration days, nor even by establishing, presuming that we could, that the administration is generally very fair in its treatment of complaints against the luluai who misuses his position. The prob-lem lies much deeper than any amount of this sort of data gathering will be able to penetrate, and we will go much further towards a solution if we look at what the indigenous leaders meant to the people they led, whether any powerful men are best described as "despots", and if so, whether their "emergence can be regularized within the political structure".

There are cases of legendary strong men in many New Guinea societies. Indeed, in a political situation in which no formal rules obtained which could regulate the behaviour of a man with strength or charisma, it would be curious if there were not "exceptions" to the general pattern of acting within the implicitly defined system. (Cf. Hogbin 1951; Popsipil 1958; Burnett 1959; Finney 1968). Yet we cannot always be sure what impact these men have. It is not to be unquestioningly assumed that strength in a New Guinea society will automatically lead to political success. One is easily led to believe that in a free-floating political system a man with strength will be the man with authority. Amongst the Gahuku-Gama, however, the strong man "may be admired for his abilities. He will earn a name, even attract adherents, but he is unlikely to achieve generalized authority or lasting influence" (Read 1959: 433). Amongst the Gahuku-Gama there are strong men but the real leaders are not those men. The real leaders are, rather, those men who have strength and the qualities associated with it, but who have learned to temper their strength with an awareness of the other values of the society, the most significant of which is the maintenance of equivalence.

Equivalence, or reciprocity is a key concept throughout New Guinea
societies, and where there are equivalences to be maintained, it is very
easy for the insensitive strong man to overstep the mark, and to lose
favour within the society. In Gahuku-Gama, "successful leadership seems
to require a considerable degree of self-control. It needs judgement if
not calculation and sensitivity to the nuances of opinion and feeling in
the gathering" (op. cit.: 431). To be a successful leader, to gain
lasting support from the people a man must have strength: must have
skills in oratory, in manipulation, in management, in production; but he
must also have an insight into the problem of the antithetical natures of
unbridled strength and the subtle maintenance of equivalence: "It is men
who possess this insight - and whose self-control enables them to profit
from the knowledge - who are 'selected' as leaders in the traditional
sociocultural system" (op. cit.: 434). The existence of strong men in
Gahuku-Gama society is not to be questioned. What is to be questioned
is whether, in this case at least, their position "can be regularized
within the political structure". Any strong man with power would here
be better described as a 'bully' or a 'swashbuckler' than a 'despot' or
'director', for his activities are to be seen outside the political system,
not within it.

Big men, the normal leaders in traditional New Guinea society, are
not merely political figures. Their roles in society are far more uni-
versal, and they form the pivot round which many definitions and activit-
ies circulate. In Hagen society, for example, the rise of the big man
is not only linked to his leadership of a segmentary group, which of
itself would easily lead to despotism, but he also has a central role in
the exchange system referred to as moka. Moka systems are fairly rigid
based upon equivalence and although the competitive aspects of the system
will tend to create situations of tension, the leader of the moka group
will be well advised to take care not to try to push his exchange partners
too hard, for it is success in moka which "is perhaps the most important
single criterion and index of influence and prestige." Although a man
may achieve some status through strength in other fields such as "prowess
or former prowess in war, and by their forcefulness and shrewdness in
debate of public affairs", the relevant title of "'numi', or 'ranking
leader' is explicitly stated by informants to be achieved when a man first
becomes a principal in the Moka and to be held only as long as he maintains
this role." (Bulmer 1960:5.) This is a situation which holds through-
out the area of moka activity, not just for the Kyaka of whom Bulmer is
writing.

It is the big man's role in the delicate area of equivalence which
acts as a brake on his personal ambitions. Drawing support from his clan
or sub-clan for the means of exchange, he has their interests to consider
as well as his own, and stands to lose a great deal by mismanagement.
Even when he attempts to create a personal prestige by individual action,
scope for gaining authority is severely limited (Cf. Strathern 1966: 364-5).

The typical big man is, then, a central figure within a group. His
position is defined by the group, rather than the reverse. Whereas

within a business or a centralized state one refers to "X and Co." or to the "kingdom" and so on, the big man in New Guinea, even though he makes many more suggestions than others as to what should be done, how, when, and by whom, is dependent on the group for his role, status, and position. That there will be a leader or leaders is subject to an unwritten rule; who that leader shall be is not so much the decision of one man, but of the group. "'The name of the apumtau (leader),' they say 'was heard first on the lips of the people. It was they who decided whether they wanted a certain man to direct them. The village picked an apumtau; he did not make the claim himself.'" (Hogbin 1946:42). Thus it is in most New Guinea societies, that "authority is achieved rather than ascribed" (Read 1959:425), for it is rarely the case that the selection of a big man will precede the establishment by that individual of his powers in all the relevant fields of endeavour.

Having had his position defined by the group for whom he will act as a guide and mentor a situation arises in which the definitions begin to flow back to the people. Aims and objectives are defined within a situation of "consensus democracy" in which the 'elected' leader acts as a filter through which the activity is decided and executed. In the Hagen area, for example, "The people themselves, with and through their wua nuim have decided their goals and how these were to be achieved. The big man, in his turn, had to adopt a personalistic philosophy and deal individually and on a person-to-person basis with those whom he represented." (Brandewie 1971:209.) It is this definitional role of the big man which anchored him to the people. Those who did become despots, though their strength may have led to a position of real power, and though, in rare cases they may have held sway for a considerable time through coercion and intrigue, were essentially outside the political norms of New Guinea society and flourished not upon the vagaries of the political philosophy of a fluid society, but upon fear, greed and charisma, factors which can lead to the temporary breakdown of even the most carefully monitored political system. Given that these "exceptions" were supplementary to, and not derived from the political system, we may be tempted to lend credence to the assertion that "Primitive Melanesian Society ... was living proof that anarchy can work." (Hogbin 1951:141.)

We have so far been largely unaffected by indigenous terminology. The problem has been set out and examined almost entirely in terms defined within our own political theories. "Anarchy" and "satrapy", "despots" and "directors" are terms which have great value in our own political philosophy. But "English language patterns of thought are not a necessary model for the whole of human society" (Leach 1961:27). What we ought to be far more concerned with is the indigenous reaction to the big man, and to the luluai. To assess the thesis that "alien rule gives new powers to the native authorities it establishes" (Brown 1963:1) we not only have to judge whether we think the luluai is more powerful than the big man, for this is only half the answer. It is, surely, the people who experience that power who are to be the best judges of the respective powers of the old leader and the new leader. And just as we should take notice of the indigenous thoughts on the individuals and categories in question, we must not forget that the

concept of "power" itself may well have nuances of meaning for the Kanaka
which we, with our preconceptions formed within one particular political
system, may tend to overlook as irrelevant to what we define as a
question of politics. It is to these latter considerations that I will
now turn.

There seems to be a general attitude amongst many writers on indigenous
political systems that a big man has an "office". That the "status" of
a big man is a fixed entity, at least so long as an individual has that
status, and that, for a particular group, one man will be "the" big man
for a time, to be succeeded by his successor when his powers wane. The
feeling one gets is that there is a "big manship", like a Presidency, or
a throne, which is filled now by one man, now by another. But to take
this as an accurate picture of all New Guinea societies would be to mis-
represent the meaning of the indigenous term which we translate as "big
man". Let us look at the evidence from the Hagen area. "The big man's
role in society is a very pervasive one, yet difficult to characterize
in clear, unambiguous terms. In some cases informants are definite:
'So-and-so is a big man'. At other times they are doubtful, or they
state a man's position relative to that of another person. The response
may also depend on the lineage affiliation of the informant. Miap of
lineage X is a big man for an informant from lineage X, but someone from
another lineage may call him a 'rubbish man', the precise opposite.
Indeed, at times of competition, even a commonly accepted big man may be
called by this epithet." (Brandewie 1971:195). So for Hagen, at
least, the ascription of "big man" is more of a description of someone's
abilities than a label or title to be attached to the appropriate man.
It is a relative term which does not have an excluse indigenous word to
describe it. "The expression 'big man' is a translation of various
phrases, the most common of which is wua nuim, meaning 'great-important-
wealthy man'." (Brandewie 1971:196.) The separate terms have other
uses and it is significant that, "A woman may be called an amp nuim;
she is one who knows how to raise many pigs, who is strong and has many
children" (ibid). Thus it is that a strong man can aspire to wua nuim
and strong women can have similar aims, even though her sphere of acti-
vities will be considerably less within the community as a whole than
than of a big man. There is also the dubious term wua korupa or
'rubbish man' to describe a man "who is always asking for things. In
any case he is a man who does not engage in many exchanges, or when he
does he is primarily concerned with his own benefit" (op. cit.: 196-7).
It would appear that a strong man who used his influence to his own ends
could even be referred to as a 'rubbish-man' - certainly not the name a
leader would aspire to.

There is evidence that this sort of relativism in the ascription of
the indigenous term which we render as 'big man' is fairly widespread.
In a general survey of the status of the big man in Melanesia,Sahlins
writes that "Big men do not come to office.... It is not accurate to
speak of "big man" as a political title, for it is but an acknowledged
standing in interpersonal relations - a 'prince among men' so to speak
as opposed to 'The Prince of Danes'." (Sahlins 1963:289.) It is not
surprising that we do not find in many New Guinea languages, a term

which would adequately translate into "director", for even the "lesser" term, "executive" is not an accurate reflection of the big man in most New Guinea societies. The fact that the big man does, in fact, execute certain plans is hardly the point, for those plans have been created in a situation of "consensus". The big man is simply he who is considered to be fittest to execute certain tasks at a particular time; for a finite period he is 'the best man for the job', and there is only a distinction of degree between him and an ordinary man, and even between him and a 'rubbish man'.

It is clear, then, that it is misguided to try to match the big man against the luluai in an attempt to balance their respective powers. There is a logical distinction between the two terms. Whereas the term 'luluai' refers directly to an official status, irrespective of the individual who may at any time be fulfilling the role, and so is a name or reference, the term for 'big man' is entirely descriptive and cannot have meaning without reference to a particular big man. To say of a certain man that he is a big man is, in part at least, to define what the term means; he is an example of the sort of thing we mean by 'big' or 'nuim'; and it would make no sense to talk of a big man who had no wives, took no part in exchange ceremonies, was unaware of his reciprocal relations with people, had no gift for oratory, had no supporters in gift exchange or warfare; generally who lacked support from his fellows, for it is precisely these people who define the big man. A man who had none of these qualities would logically not be a big man, and had he once been a big man, the recognition that he no longer possessed the appropriate qualities would in itself entail the removal of the description 'big man'. A man cannot be a 'big man' if he is not at one and the same time a 'big' man, cannot be wa nuim if he is not nuim. The big man is essentially a man.

On the other hand, by being given an office, a recognizable status by the administration, the luluai has no need to justify his title. Of course, it may be the case that he was given the post as a result of certain skills being displayed by the man, although this is not necessarily the case. Again, the execution of certain duties, the liasing with the District Officer, the mediating between the nature and the administration are marks of a luluai. But they do not themselves make a man a luluai. Even if it is the case that a luluai who failed to do what luluais are meant to do would be removed from office, there is an office from which he can be removed, and that office is a vacuum waiting to be filled by the next luluai. There is no logical, immediate link between a luluai's activities and his being a luluai, and it would be of little value in defining the term luluai to point to one and say that is the sort of man who is a luluai, because he may well be a very unusual type. Logically a man can be a luluai whatever his qualities. The luluai is essentially an office, and whereas a group can say of a man "You are no longer a big man" thereby making it true merely by the statement of the group, it would be of no avail for a village to go to a luluai and say "you are no longer a luluai" for even if they were to steal his cap and baton, they cannot steal his title; this privilege rests with the administration.

The problem does not hinge merely on a fine logical point, however, It would hardly be of importance if it were not for the corollary that the big man is defined within the community and the luluai defined from outside the community; by the white man. Of itself the situation in which "..... sometimes only nonentities can be persuaded to accept the office" (Salisbury 1964:229) would tend only to a disregard for the whole idea of the luluai. But when these 'nonentities' begin to demonstrate that they can have power over men who have established themselves through the more recognizable channels of exchanges, speeches and so on, it becomes clear that the question hinges not so much upon the individual big man and the individual luluai but upon the very validity of the traditional definitions. And this, again, does not restrict itself to the traditional definitions of big man alone, but rapidly extends to other fields. Salisbury cites a case which is apposite to the argument: a luluai from Siane "was taken to court accused of intercourse with a non-nubile girl, an offence which is a capital crime in native theory and much more serious than adultery. The Native regulations do not recognize this offence, however, and the case was dismissed despite the evidence." (Salisbury 1964:229.) Not only do we have a case here of a luluai getting away with what would not have been tolerated from even a big man in the pre-administration days, but we find that the natives have lost the means to operate sanctions against a man who commits what is to them a capital crime. In the practical sense, intercourse with a non-nubile girl has ceased to be a crime, though for no accountable reason. Alien ways of thinking, new ways of operating, often implemented through what the indigenous people see as a "nonentity" are challenging the Kanakas right to define their own terms. No longer are they able to define their leaders; no longer to define their laws. A luluai who can capitalize upon this situation, "the progressive fellow" will quickly find, "that alien rule gives new powers to the native authorities it establishes" (Brown 1963:1).

To a certain degree, then, Brown's thesis seems to hold. But whereas the alien rule in New Guinea does give "new powers to the native authorities it establishes", this does not necessarily refute the "commonplace in the study of changing political systems that the imposition of alien rule restricts the power of traditional authorities" (ibid). So long as we retain our ehtnocentric stance in the study of changing political systems, the two assertions seem at odds. Closer examination of the indigenous situations in New Guinea show that, far from being mutually untenable in this area, both may be correct. For having begun to establish that the "traditional authorities" were not generally individuals, or when they were, they were acting in concert with a group within which they received their definition, we will not need to find evidence for or against the restriction of power in the traditional authorities in the range of behaviour of the big men over their 'subjects'. Traditional authority, rather, lies in the "consensus democracy" of the group as a whole and a restriction of the power of the group as a whole, if it is to be established, will not be found in any re-allocation of responsibility or authority within the group, from individual to individual, or from group to individual, but from the powers of the group itself, acting

in co-operation with, and through the big man. What we need to discover
is whether or not "the imposition of alien rule" restricts the power of
the typical New Guinea political unit as a whole, remembering all the time
that the concept of 'power' is subject to a variety of interpretations,
depending upon the culture which is under scrutiny at the time.

What we should be looking at is the relationships which have sprung
up between Kanakas and white men, attempting to ascertain whether there
is a feeling by the native peoples that the arrival of the European has
led to a restriction of their own powers. There is a prima facie case
for saying without further ado that this must have been the case since in
all areas the administration imposed rules and laws, many of which were
received grudgingly by the natives. We have already seen how the luluai
could easily be resented in the general ambience of a traditional society
for the simple reason that he wielded power without first having achieved
the status of a big man. On the other hand, the magistrate was not
drawn from the ranks of the nonentities. There are those who would
argue that the typical New Guinea native would happily follow a magistrate
so long as he was strong and commanded respect. Of course in many areas
the magistrates filled this strong-man role quite well. Amongst the
Elema, for example, "Magistrates were physically and mentally strong and
self reliant; in many cases they had been recruited from the ranks of
those who knew how to 'manage' the natives - the expatriate planter class"
(Cochrane 1970:40). These men would command respect, just as the strong
man amongst the Gahuku-Gama would command respect. But he could not
fulfil the role of leader, of big man, because he had no idea of, or if
he did, he took no notice of the necessities of equivalence, and all the
other values of the society. He was the sort of man who would get
things done, but there would be an over-all feeling of unease, even on
the part of those who followed him, probably because they had not the
desire to maintain the equivalence required of a successful big man. The
short term impact of such a man would be fruitful, "But the magistrate's
status was not the same kind of status that was possessed by the 'big man'.
Traditional 'big men' has presented a synthesized cultural image of their
society. The magistrate ignored Elema culture and his status was of a
personal nature - the Elema could not think of him as their 'big man'."
(op. cit.: 42). The imposition of alien rule, biting as it does at the
traditional definitions, will need to do more to compensate for their loss
than supply strong magistrates and transistor radios - indeed these items
may well be even more destructive than constructive.

The definitions to which I refer are not merely political. Minor
irritation could only be the result of a redefinition of political real-
ities if these were not already as closely interwoven with the other areas
of thought and action in New Guinea societies. For the removal of power
from the "consensus democracy" into the hands of, immediately, the luluai,
but more significantly, into the European sphere of activity requires a
political reaction only in so far as the concept of "consensus democracy"
was political. The evidence is that the reactions were marginally pol-
itical, but were more strikingly "millenarian", "messianic", embracing a
far wider range of realities of political power between individuals. So
long as we are not lulled into believing that the reactions to European
contacts generally referred to collectively as "cargo cults" were scarcely
disguised political movements, or political reactions framed in an essent-
ially magico-religious mode as a result of the fact that this "is the
characteristic type of explanation which is current in that society"

"Jarvie 1946:67) we shall be able to begin to see that "consensus democracy" was not a merely contingent element in New Guinea societies, but part of a conceptual framework to which no element of thought or action was unrelated. A functionalist would be able to tell us this, but would be unable to explain why the new system, which was functionally sound, caused such difficulties. A structuralist would also be able to show the interrelations between the seemingly disparate areas of thought and action, but the typical structural model would lack the fluid dynamism required to reflect the nuances of indigenous modes of thought. What is required is not a general model to 'explain away' the phenomena, but a careful examination of the details of at least one situation of culture contact and its ramifications in the changing modes of thought and action amongst the people contacted. A big man in one society may have strong similarities with his counterpart in other New Guinea societies: and European administration has followed an essentially similar pattern throughout the territories: Native Regulations applied wherever the appropriate machinery was available and labour legislation under which natives were liable to imprisonment for breaking their contract of employment by running away, for refusing to work, and even for failing to show ordinary diligence, applied universally between 1893 and 1946 irrespective of the reactions of the respective groups of natives. But we will not understand reactions to these situations on the part of the indigenous peoples by attempting to draw out patterns without in each instance discovering how big men were operative and fit into the cognitive map, and exactly what the Europeans respresented to the peoples, not just in a political sense, but in the wider concepts of the respective patterns of thought. Where political questions are, for the Kanaka, inseparable from other questions, reactions to the new situation will depend upon the whole conceptual framework of each society. For the advent of a new class of beings such as Europeans must have represented, and, moreover, a class of beings whose behaviour showed both ignorance and disregard for the moral, political, and philosophical realities recognized by the contacted peoples, hand-in-hand with ostensible power over the physical environment, the taxonomic systems will be severely strained. Thus it is that what is called into question in the typical New Guinea society with the advent of the European, both Missionary and Kiap, is not just the validity of political structures, but the validity of the whole conceptual scheme within which the political structure gained meaning. Europeans are not just another set of phenomena to be slotted conveniently away into a pre-existing category or class, but a means of severely testing the whole categorical and classificatory system itself. In some cases the problem was easily overcome, if we can believe the ethnographers' reports; in others, the impact has been to "make the world turn over".

Keith Patching.

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A Glimpse of Malinowski in Retrospect

After a seminar on Malinowski which I gave sometime ago at the
Oxford Institute, the then editor of JASO asked me to give the gist of my
talk in a short article and include some of the extracts which I had
quoted from Malinowski's letters to me. The letters which I had
received from Malinowski were all too few: mostly notes written when I
was at the LSE just before the outbreak of war.   What I include here are
extracts from those written in connection with my book Aboriginal Woman:
Sacred and Profane.   They are the only ones I possess and this must
excuse what might pass for my egoism and vanity of which, of course, I
have my due share.   They give a glimpse of the man himself, his warmth,
his sense of humour, and his capacity to take criticism.   His detractors
have frequently said that he would not accept criticism, and it is true
that on occasion he did indulge in abuse and vituperation, particularly
when as much as anything it was a matter of clash of temperament and
personality.   But there are all too few anthropologists of eminence who
have accepted criticism with grace, however justified it may have been.
Fundamentally, he was deeply diffident, as those who have read his Diary
with detachment and his monographs with care will appreciate.   However,
my talk about him was not yet another evaluation of his work, though there
is need for continual reappraisal.   My essay in Man and Culture (ed.
Raymond Firth, 1957) gave some assessment of his enduring contribution to
fieldwork methods, to the wealth of the material he collected, the
exacting standard it set for others, and the stimulus of his generalisa-
tions and theories at the time and indeed now.   He was a great anthro-
pologist, and if one adds "but", let us remember that that adjective is
almost invariably followed by "but".   So, butt me no butts !

This short piece endeavours to give some impression of what it was
like to be one of his postgraduates at the LSE from 1936 to 1938, the
climate of time and place, and the intoxication of it all.   It was not
undiluted euphoria by any means; that would have been monotonous and
unproductive in terms of human interaction.   And here, in true Malinowskian
tradition, I must put myself into the context of situation.   My first
degree in anthropology was taken at Sydney University when Raymond Firth
had taken over from Radcliffe-Brown, and Ian Hogbin had just returned
from the field.   Both had been students of Malinowski; both were friends
of Radcliffe-Brown and versed in his methods and doctrines.   We had
Australian subsection systems (to my consternation and confusion), and
also Tikopia and Ontong Java, and much besides.   When, later, under
Professor Elkin, I completed a library thesis on "Culture Change in
Melanesia", Camilla Wedgwood who was my external examiner said:"Professor,
she must do fieldwork and she must have an island." This was precisely
what I wanted; but Elkin, who was an authority on Australian Aborigines,
replied: "We know nothing about the secret life of Aboriginal women: I
want her to study that." As the main thing was to get off the ground and
to any field, albeit a subsection one, I agreed and went off to North-West
Australia for eight months.   The time was short, but money was scarce,
I was an untried fledgling of 23, and one problem was whether I would
sink or swim in the field.   I swam ! My next fieldwork was to have been
an intensive study of a tribe in Western Australia, 80 miles by camel
from the nearest town, Laverton.   Unfortunately, when I arrived in
Laverton I found that the missionary and his wife on whom I would have
been dependent for supplies (by camel) had just arrived ill and would not
be returning to their base for some time.   The head of the mission in
Laverton would have no truck with anthropologists whom he regarded as
encouragers of devil worship (this included specifically Radcliffe-
Brown and Elkin); so the only thing to be done at a moment's notice was
to go back to the north-west.

In October 1936 I duly arrived to do my Ph.D. at the LSE - that Mecca then for all young anthropologists. Raymond Firth was my supervisor; and I was research assistant to Audrey Richards, who characteristically read and gave me advice about chapters of my thesis, and attended the seminar when, for Malinowski, I put some of her Bemba material on chieftainship into three columns for the analysis of social change. She commented, perhaps wryly, that she did not know there was so much in her material! 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 agonising because, inexorably, would come the question: "What do you think of that Miss K.?" Paralysed, 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. Like all students and indeed fieldworkers present, I kept a record of notes on papers and discussions at the seminars. One, dated October 1937 to March 1938, included comments and points made by Leach, Fei, Hsu, Kenyatta, Fadipe. Wilson, Stanner, Piddington and Margaret Read; in the previous session there were Nadel, Wagner, Stevenson and others. Not surprisingly all these students were considerably influenced by Malinowski's theories! Anthropologists, historians and writers passing through London and interested in anthropology and Malinowski dropped into seminars. There was a cross-fire of European languages, argument and laughter. In the first session, Malinowski was billed to give a series of lectures for postgraduates at 5 pm., one hour after the conclusion of his seminar. He gave only three lectures; thereafter and to our mutual enjoyment the hour was devoted to a continuation of the seminar after a break for tea at 4 pm. Along with anthropological seminars, some of us had the stimulus of attending lectures on sociology by Mannheim, and on history by Laski. All this was fed back into the 'seminar proper'. Nor was conviviality neglected. Raymond, Audrey and Bronio entertained frequently and lavishly at their homes; and there was one wealthy amateur who from time to time placed her car and chauffeur at Malinowski's disposal, and always had two or three bottles of vintage claret for him at her parties.

However, I fell from grace just before the beginning of my second session at the LSE, when Malinowski paid me the honour of inviting me to become his research assistant. Work with him would have been enormously stimulating and worthwhile, but time-consuming so I regretfully refused, since money was short and I had to finish my thesis and return to Australia by the end of 1938. For me the ice age set in, and giving papers at his seminar became once more an ordeal though my fellow Australians always came to my rescue, particularly on one occasion when I had to give a paper on subsections in north-west Australia. Malinowski glacially dismissed it as 'kinship algebra'. It was not: I am not at all mathematical; more importantly, the Aborigines had allocated me to a subsection and I had had to live the system in my relations with them.

When I returned to Sydney, my revised thesis was accepted early in 1939 by Routledge for publication as Aboriginal Woman: Sacred and Profane. I wrote to Malinowski to ask him if I might dedicate the book to him; but, in view of the contretemps which had occurred in my last year at the LSE, and, more importantly, the fact that I disagreed with some of his theory, I thought he might not want the dedication. So I explained that I could not accept some of his generalisations in his armchair book, The Family among the Australian Aborigines (1913), and that I had reservations about his theory of culture: it did not explain how derived needs arose; it did not account for the diversity of institutions, and so on. I received the following letter, dated 7th April 1939 from Tuscon, Arizona, shortly before I was due to leave for New Guinea to do fieldwork (I had got "my island" at last).

"My dear Phyllis,

Your letter of 5.iii.39 was a lovely birthday present on my sad 55th anniversary of the most calamitous event which befell in my life - except perhaps the one which preceded it by 9 months; or else my decision to become an anthropologist.

You know, my dear Child, that in dedicating your book to me you bestow a great honour on me. I can tell you that you are giving me real pleasure. I was really moved not only by the fact that you want to inscribe it to me but also by what you write in your letter. All about genius etc. is tripe no doubt, but then as you know tripe is our Polish national dish, and prepared and served as yours was (and is) it was very readily, greedily and gratefully assimilated. To push this metaphor as far as it can go (no ribald giggles please), I imbibed or ate or lapped it up, and the way to a man's heart is his stomach.

When a young, capable and attractive girl offers her First-born as to a God-Parent, it is a pleasant gift indeed (another metaphor). And seriously I know that the First-born, the Daughter (for surely the book is of feminine gender) will also be bright, attractive and withal solid of brawn and brain.

I am very much looking forward to see Her ('Aboriginal Woman') in evening dress or full dress (or isn't it negligee?) and shall look forward to getting an inscribed copy. Is She going to be more 'sacred' or 'profane' I wonder !

As you can see my second childhood (metaphor) is coming on rapidly. I am trying to work and if I produce anything you'll get a copy. I sincerely hope you'll get over to New Guinea and do some work on Papuans or Melanesians. The latter are more pleasant to work with, the former more dramatic and certainly fuller of mysterious elements. I have been amusing myself in doing a bit of work on a detribalised group from Sonora (Mexico) which may be quite profitable.

<div style="text-align:center">

With parental affection,
Yours,
B.M."

</div>

I had then a brief note of July 20 1939 to say he had written to Routledge to say the dedication was "OK" by him. And then:

"I am very keen to see the book as soon as it is ready. I shall be equally keen to hear more about your fieldwork, so please write to me as soon as things begin to crystallise. You know my passion for yams and other vegetables, together with magic and ceremony mixed into an Irish stew."

The next letter was dated May 10 1940 from Yale:

"My dear Phyllis,

I have just received ABORIGINAL WOMAN and your accompanying letter of February 20th. Please let me thank you very affectionately for dedicating this excellent volume to me. Having books inscribed openly and publicly is perhaps the most pleasant type of distinction, and in many ways I appreciate your dedication more than some of the others...

"I am writing this straight away after having spent a couple of hours with ABORIGINAL WOMAN in chaste though affectionate converse, but I hope to read the book more carefully and with a mind balanced by criticism as well as enthusiasm. I shall then report to you my disagreements, as well as bring out more concretely the points where my enthusiasm becomes effervescent.

I shall not write more fully, since from your short note I gather that you are leaving Sydney where I am addressing this letter, but I am not certain whether it will reach you there. Please let me know.

<div align="center">
With affectionate thoughts,
Yours always,
B.M.
B. Malinowski"
</div>

And then comes a postscript in his handwriting:

"P.S. This morning's news from Europe – just heard over the radio – invasion of Belgium and Holland is so shattering that nothing else seems to matter.

<div align="center">B.M."</div>

And here is the last letter I have from him, written May 18th 1940:

"My dear Phyllis,

I wrote to you a few days ago, but now your letter (probably February 8) arrives and I must send you a few words. I say 'probably' because your handwriting is a fit matter for an Egyptologist or some other specialist in deciphering difficult and complex texts.

As regards 'for the moment'[1], I have in truth not noticed it in a way which would touch my sensibilities. Now, as you know, 'Malinowski is as touchy as he is conceited' to quote the majority of my colleagues, pupils and friends. So the phrase cannot be in any way offensive. At the time I probably reacted to it as an indication that you plan and propose to develop Malinowski's theory and to replace it by something bigger and better. Indeed, I hope you will remember the first two pages of your letter to me. I am keeping it in my files and on request shall return it to you. The reason is that what you say is very much to the point and I hope you will write it out in full as a constructive criticism of functionalism and get in printed in Oceania. I would then be able to reply to it if I found I disagreed with some points and it would certainly stimulate me as well as others to solve some unsolved problems of functionalism. You have stated them very well. There is no doubt that the weakest point in my theory was the insufficient analysis of how 'derived needs' arise. There is also no question that your second point, that is, the development of the concept of institutions or hypertrophied institutions is something which functionalism sooner or later will have to deal. 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. From your point of view, a theoretical contribution would be extremely useful to you for your reputation at the present state of your career. So sit down and write out this article. I am also sending you a reprint of my latest article in which one or two points are perhaps more adequately and fully dealt with, although it is too short to be satisfactory.

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1. I had in the Foreward to the book said: "This book offers no new theory of culture; for the moment I am substantially in agreement with that formulated by Professor Malinowski and others of his school."

"Lots of affectionate thoughts,
Yours always,
B.M."

In October 1941 I went to Yale where Malinowski was Visiting Professor.
There were seminars attended by postgraduate anthropology students and
many others, but the gatherings lacked the sparkle and thrust of those at
the LSE. Moreover, the War overshadowed everything, and he was working
on what was to be published posthumously as Freedom and Civilisation. He
was also increasingly interested in Mexico, and I was to go in mid-1942 to
do fieldwork there and eventually collaborate with him on a book on culture
change. He died in May 1942.

Malinowski could be inconsistent, maddeningly so; he had his
prejudices - but that goes for most. He was a great teacher. As Firth
has said of him: "his constant question was: 'Where does the real problem
lie?' And he saw it always not in terms of fine-spun academic theories,
but arising out of behaviour of ordinary human beings." And, as I myself
said in the same volume,[2] "In passing from one dimension to another, from
the technological to the structural or the ideological, Malinowski has
his own criteria of relevance and these are determined by the scientific
rigour which he considers necessary for the documentation of his more
abstract generalisations... He is never guilty of concocting what Postan...
has called 'a soufflé of whipped postulates'... He provides us with a
wealth of information on native incentives, values and attitudes, on the
tensions and conflicts which underlie the operation of structural
principles, and on 'the amplitude of deviation' from the norm. In so
doing he has 'drawn attention to a range of problems which increasingly
are demanding the attention of anthropologists."

Phyllis M. Kaberry

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2.   Raymond Firth, ed., Man and Culture, 1957, p.8; pp.85 and 86

CRIMINOLOGY AND SOCIAL ANTHROPOLOGY

As an anthropologist now engaged in research in the criminological field I have inevitably been interested in methodoligical similarities and differences between the two disciplines. They came closest together in the late nineteenth century, when, feeding on the primitive/civilised dichotomy evinced by evolutionist theory, Cesare Lombroso developed pseudo-scientific techniques, akin to those of physical anthropologists, for classifying the 'criminal type'. Indeed, Lombroso specifically compared 'criminals, savages and apes': the typical criminal was seen as an atavistic being, sharing with the other two groups features such as 'enormous jaws, prominent superciliary arches, solitary lines in the palms, extreme size of the orbits, handle shaped or sensile ears...' and so on.(1) Conversely, the anthropologist F. Galton turned his attention to criminology, writing in JAI in 1879 'On Composite Portraits', an attempt to get at the essence of the criminal face.

However, once it became clear to both disciplines that crude evolutionary theories were untenable, and that 'innate' character could not be equated with physical or racial features, the paths diverged significantly. Whereas ideas such as those of Lévy-Bruhl on 'primitive mentality' found no support in the new schools of social anthropology, being criticised by Malinowski, Durkheim and others for over-stressing individual psychology (and thus, by inference, merely new ways of confirming the otherness and inferiority of primitives), in criminology the traces of the evolutionist period were not so easily shaken off. Instead of rejecting the idea of intrinsic difference, Lombroso and his followers simply created new 'types': to the atavistic criminal were added the 'epileptic criminal', the 'insane criminal' and almost in the same breath the 'poorly educated criminal'.(2) No British criminologists 'stepped off the verandah'. The subject developed as the blinkered study of individuals in captivity and the quantification of suspect official statistics, separating itself from any wider-scale sociological analysis. Terence Morris complained in 1957:

> 'The founding of a school of "criminal anthropology" seems to have resulted in the total or near total eclipse of the work of sociologists in the criminal field. The genetic theories of crime which have subsequently been replaced by psychological theories of crime seem to have excited so much interest that sociological theories, especially in Europe, have been of secondary importance.'(3)

One explanation put forward for this state of affairs is the occupation of the field for most of this century by 'medical men', who originally moved into it attracted by Lombroso's widely publicised biological theories. Particularly in England and the Scandinavian countries, psychologists and psychiatrists have subsequently outlined the history of criminology as though it were a branch of medicine, tracing it through the works of Gall, Lavater, Pinel, Morel, Esquinol, Maudsley, etc., and ignoring the sociological theories of Guerry, Quetelet, Bonger, Marx, and others writing before the 'Lombrosian myth' took hold.(4) Only within the last few years have sociologists made any real headway against the prevalence of pathological models of crime - and this has been largely due to the influence of American criminology, where Merton, Sutherland, Cressy and others have at least kept the sociological tradition alive.

However, a more fundamental reason for the continuing emphasis on flaws in the individual criminal rather than on social structures and definitions may be found in a comparison with the privileged position of social anthropology. Because criminal activity takes place as it were 'on our own doorstep', it has always been regarded as more of a threat to the accepted values of life in Western society than has the behaviour of 'natives' on the far side of the world. British anthropologists could afford the luxury of an ethically 'neutral' stance with regard to practices they studied in the colonies and elsewhere, although many of these (e.g. infanticide, mutilation in rites of passage, burial alive of Divine Kings) would have been treated as serious crimes at home. Most social anthropologists would endorse Pocock's statement:

'It is evident at the outset that the anthropologist working in another society (or in his own society regarded as "other") must take a certain stance quite different from that of, say, a governmentofficial or missionary, who is concerned to bring about changes in accordance with certain beliefs which he holds.'
(1971:86)

The word 'criminologist' could not automatically be substituted for 'anthropologist' in the above extract. Criminologists have generally had to justify their research to funding authorities as a series of attempts geared directly or indirectly to finding ways of reducing, or ideally eliminating, the incidence of crime in their own society, and have often worked closely with 'government officials and missionaries' (probation officers were originally known as 'court missionaries', interestingly). From the beginning, the dice were loaded against their chances of portraying criminals as ordinary healthy individuals acting in a specific socio-cultural space. The criminal act became stripped of meaning except as a futile response to weaknesses within the individual or, latterly, within his environment.

Thus positivist criminology developed and took hold. It grew out of an uneasy blend of sociological, psychiatric/psychological and juris-prudential thought (criminologists, like social anthropologists, coming to their subject from a variety of academic and professional backgrounds), and gradually took on a character of its own. The traditional method - which is by no means defunct - relied heavily on statistical analyses of official data about the background, character and offences of convicted individuals. 'Law-like generalities' were sought inductively through measurement and quantification, a procedure commonly justified by refer-ence to a stereo-typed model of the natural sciences (criminologists have been far slower than social anthropologists to see the implications of the philosophy of science debate between Kuhn, Popper, etc.).

There is no need to repeat here familiar arguments about the posit-ivist method, but two features and their consequences must be mentioned:
a) the prevalence of <u>deterministic</u> explanations, and
b) the obsession with <u>finding</u> 'real' facts.

a) <u>DETERMINISM</u>

The procedure of comparing a sample of convicted offenders with a control sample of 'normal' people, which has been a common method in criminology, not only creates a false dichotomy, but leads to the position that criminals are regarded as theproduct of various physical, psychological and/or environmental determinants. Recent examples are the 'discovery'

that more convicted criminals than non-criminals have an extra Y chromosome,
measurements of 'psycho-pathic tendencies', 'social deprivation', 'broken
homes', etc.   The clear inference is that if experts were allowed a freer
hand to alter such determining influences, the volume of crime could be
reduced.   Yet at the same time, the legal process in England still rests
heavily on the 'free-will' model of human action, where a man is held
responsible for his actions.   There has been a fundamental ideological
clash between the judiciary (and sections of the police and prison service)
on one hand and criminologists and welfare workers on the other, for most
of this century.   In almost every criminal case the contradiction mani-
fests itself: should the punishment fit the crime or the criminal?   In
practice, the conflict is mediated by a variety of devices, from the ex-
tremes of declaring a person 'unfit to plead' (thus relieving him entirely
of free will) to detailed consideration of 'mitigating circumstances' (often
based on reports by doctors and welfare workers).   In effect the judge
weighs up 'evil intent' against 'circumstances beyond the offender's
control'.   The general trend has been increasingly towards the hegemony
of the deterministic model, with rapid growth of welfare services and
acceptance of more non-custodial sentences, but occasionally heavy 'exem-
plary' sentences are handed out to defendants (e.g. the Train Robbers, the
Krays, even 'vandals' or 'hooligans') who have been singled out as delib-
erately 'evil' criminals 'with no excuse':   thus the free-will/punishment
model reasserts itself.

   Despite the humanist advantages of the policies which have followed
from the positivist-determinist approach of criminologists the fact remains
that by concentrating on behaviour to the exclusion of thoughts and beliefs
of the actors it has not greatly improved our understanding of the pheno-
menon of crime.   In the course of attempts to break down exotic myths
about the nature of the 'criminal type', it has moved the concept of the
criminal from that of 'other' to 'like us essentially, but ...'This is
reflected in the concept of 'rehabilitation' - whereby an offender can be
'made fit again' for social life.   Apart from the veiled insult to, for
example, many drug-takers and 'political criminals' who would argue with
the idea that they cannot help what they do, it has continued to support
a consensus-view of western society similar to that which functionalist
anthropologists held of primitive societies.   'Conduct norms' have been
seen as given by society, and obedience to them the natural response of
its members.   Deviation from them is dysfunctional.   Thus while social
anthropologists were elevating the behaviour of of one nineteenth century
'other' - primitives - to the status of 'normal' and 'healthy', criminolo-
gists were relegating the behaviour of the second 'other' - criminals -
to that of 'abnormal' and 'pathological'.

b)   THE OBSESSION WITH FINDING 'REAL' FACTS.

   The more sophisticated positivist criminologists have recognised that
official statistics on crime are extremely problematic.   First, they are
categorised in legal terms which regularly undergo minor changes, thereby
making comparison over time difficult;   second, they are based on 'crimes
known to the police' and on individuals who pass through the complicated
legal machinery, so that a large number of 'real' crimes and criminals
appear to escape inclusion (and, conversely, through miscarriage of justice,
some people who are 'really' non-criminals are included).   How can the
'scientist' work with such shoddy material, it is asked.   Another apparent
problem is that ignorance or prejudice on the part of law-makers may
produce definitions of crime at variance with the majority: many positi-
vists would question the bland assumption made by Paul Tappan:

'The behaviour prohibited has been considered significantly in
derogation of group welfare by deliberative and representative
assembly, formally constituted for the purpose of establishing
such norms: nowhere else in the field of social control is there
directed a comparable rational effort to elaborate standards
conforming to the predominant needs, desires and interests of
the community.... Adjudicated offenders represent the closest
possible approximation to those who have in fact violated the
law, carefully selected by the sieving of the due process of
the law.'

(in Wolfgang, etc., 1962:38-34).

Those who cannot accept this idealistic view have managed to guard
their positivist position by making adjustments to the official figures.
Sellin and Wolfgang, in an article entitled 'Measuring Delinquency',
suggested ways of constructing an 'index of delinquency, that would, in
contrast with traditional and entrenched methods in use, provide a more
sensitive and meaningful measurement of the significance and the ebb and
flow of the infractions of the law attributable to juveniles, taking into
account both the number of these violations and their seriousness.' This
included the establishment of a 'community jury' (composed of students,
policemen, juvenile court judges and social workers!) who 'rated' offences
according to their seriousness, awarding points for 'injury inflicted on
a victim, intimidation and violence , value of property lost or damaged,
etc.' (5);measurement of the rates of commission was also limited to
those offences which were calculated to be most consistently reported to
the police. Thus, the authors thought, official definitions could be
side-stepped and a picture of 'true' delinquency and the 'real' extent of
'deviation from the norm' among juveniles could be calculated. Suitable
action could then be taken to correct the situation.

This 'answer' of using conduct norms rather than legal criteria as a
base for measurement reveals clearly the gap between anthropological and
criminological thinking. Social anthropologists have for some time
been looking behind empirically observed 'behaviour' and stated norms at
the mechanisms (linguistic, social, political, ecological) producing the categories
within which such 'facts' are framed. The 'correctional' perspective
adopted by so many criminologists has put them into blinkers, allowing
them to see only one reality.

LABELLING THEORY

Over the last decade there has been some headway against the prevailing
tradition, inspired largely by American sociologists of crime. An approach
which at first sight appears to be more palatable to a modern social
anthropologist has grown up from the initial recognition that 'crime' and
'criminals' can be created or defined away by acts of legislation and
decisions of policemen, juries, magistrates, etc. This is known as
'labelling' or 'social reaction' theory. The two names most notably
associated with it are Howard Becker and Edwin Lemert. Becker's well-
known statement of the position they start from reads as follows:

'The deviant is one to whom that label has successfully been
applied; deviant behaviour is behaviour that people so label.'
(1963:9).

Another 'labellist', Kai Erikson, insisting that 'social process' is of more interest than individual peculiarity writes:

'the critical variable in the study of deviance is the social audience rather than individual persons, since it is the audience which eventually decides whether or not any given action or actions will become a viable case of deviation.'(1962:308).

Unfortunately, much of the work of these theorists does not live up to the promise of these interesting general statements. Instead of examining the generation and operation of the social categories ('thief', 'drug addict', 'psychopath', 'delinquent', etc.), they concentrate mainly upon the effects of the labelling upon individuals so labelled, thus reverting to a form of social psychology. One factor leading them in this direction is the questionable distinction made by Lemert between 'primary' and 'secondary' deviation: the first meaning simply rule- or law-breaking, the second the social and psychological responses of the people 'processed' by the legal and penal system. Under his influence, labelling theorists have come to regard one of their main tasks as to trace the development from primary to secondary deviation, i.e. to document changes in self-identity from 'being normal' to 'being deviant'. The argument is that society confuses the act with the actor, so that a person arrested for primary deviation, e.g. a theft or a sexual offence, becomes regarded as a deviant personality, and consequently experiences rejection, contempt and suspicion which may not be merited. Eventually he may come to accept the labels thrust upon him. In Becker's words:

'Treating a person as though he were generally rather than specially deviant produces a self-fulfilling prophecy. It sets in motion several mechanisms which conspire to shape the person in the image people have of him. When the deviant is caught, he is treated in accordance with popular diagnosis of why he is that way, and the treatment itself may likewise produce increasing deviance.'
(1963:34).

Thus what Lemert means when he puts forward the provoking thought 'social control leads to deviance' is simply that the way society reacts to an offender may cause him to counter-react to its image of him, and as a part of this reaction, to offend again. But as Ronald Akers says:

'   'From reading this literature one sometimes gets the impression that people go about minding their own business, and then - "Wham" - society comes along and slaps them with a stigmatised label. Forced into a role of deviant the individual has little choice but to be deviant.'
(1967:46).

One of the fundamental confusions in the work of 'labellists' is of the same order that Ardener has discussed with reference to work on divorce.(6) They swing between two quite separate ideas of what 'deviants' or 'criminals' are: those labelled by society as such (irrespective of actual behaviour, true guilt or innocence, etc.) and those who really, 'out there', break rules or laws. At the beginning of the process they describe, it seems that the first idea holds - nobody is deviant until

caught and labelled. However, once social reaction has taken place, the second notion is brought in, and the person becomes a 'real' deviant (actually breaks the rules or laws) as a reaction to his label. One might ask how Lemert would regard an unconvicted bank robber setting out on his fifth 'job'. He has as yet experienced no official reaction, but, a) knows very well that his actions are against the law, b) if not arrested he will likely do it again, and c) he has a self-identity as a professional criminal... Is he 'really' a criminal or not?

Ironically, although labellists strongly criticise positivist criminologists for accepting official, legalistic definitions of criminals without reflection, they have not fully escaped the trap themselves. They set out on the road to an analysis of the labelling process, but quickly turn back to explanations of why officially-defined criminals actually commit crimes. Like the positivists, they have largely excluded meaning and intention from criminal acts. Our unconvicted bank-robber does not just happen to be pointing a shot gun at a cashier. This is part of a planned, rational action, in cooperation with others (the 'finger', getaway driver, etc.) and it has a definite meaning to him and to those he is robbing. This meaning is obviously dependent on the social arrangements of the time and the country in which he is acting - the existence of banks, cashiers, shot guns and the significance accorded to them by society.

Anthropologists have spent a great deal of time discussing 'rationality' and 'translation' of social meanings, but criminologists have lagged seriously behind. A bank robbery is relatively simple for most observers to understand, but where phenomena such as 'vandalism','hooliganism', 'drug-taking', 'silly'minor thefts, etc. are concerned, many observers cannot see any rationality at all in the actions. Certainly, 'social reaction' is an essential part of the analysis required, but only a part. The social reaction must be explained, not just given; and the intentions and projects of the deviants must be given social meaning. Labellists have dodged the first of these requirements by vague references to 'moral entrepreneurs' forcing their categories on the rest of society. As two modern deviancy theorists put it (L. Taylor and I. Taylor, 1968):

'The definers are (regarded as) a group of free-floating baddies.'

The second issue they have obfuscated by over-emphasising the individual's self-image as a rejected citizen.

AN ANTHROPOLOGY OF CRIMINALS?

Charlotte Hardman asked in an earlier JASO (IV.2:83): can there be an anthropology of children? If we substitute 'criminals' for 'children' in her question, how can an anthropological approach help in understanding crime? It may be fruitful to take note of M. Crick's stance in his discussion of witchcraft (JASO.IV.1:19):

'A sign of conceptual advance in this field will perhaps be our ceasing to write on witchcraft. So I disagree with Standefer, who saw the first problem as that of defining witchcraft: I shall endeavour to deny the phenomenon; to define it away.'

The concepts of 'crime' and 'criminals' have given so much trouble they too might initially be 'defined away'. Criminologists would do well to start at the other end from institutional definitions. Ardener writes (JASO.IV.3): 'It is always the major task in anthropology to find the actor's classification.'

I would say 'first task' rather than 'major task.' Anyhow, it is surely a good idea to look at specific cultural activities such as 'pilfering', 'safe-blowing', 'tax-fiddling', 'pot-smoking', 'house-breaking', 'picking pockets', or 'joy-riding', i.e. using the categories ordinary people accept rather than legal definitions. We can find people who consciously have taken part in such activities, convicted or unconvicted, and without calling them criminals, find out how they go about them, the terms they use to talk about them, and how their relationships with other people are affected. Some will be highly developed criminal 'trades' (e.g. picking pockets), others recognised as part of a definite 'criminal culture' (e.g. housebreaking), others virtually accepted as part of 'what everybody does' (pilfering at work, tax-fiddling). We can then go on to see how the actors' understandings fit with those of the agents of social control - police, magistrates, pro- bation officers, etc. - and how both sides act out the cultural programmes produced by society as a whole.

There are many interesting categories used in casual conversation which merit 'unpacking'. For example, police tend to divide persistent criminals into two general categories of 'villains' and 'mugs'; 'ordinary' prisoners classify some people who have committed certain sex offences as 'nonces'; some people become known as 'grasses' while others who have informed in a similar way are seen as having legitimately saved their own skin under pressure. Official terminology is also a rich field: 'clients' (probationers), 'psychopath', 'treatment', 'delinquency', are all filled with social meaning and a discussion of any one leads into insights about general social divisions and assumptions.

With this sort of approach, we are likely to come up with better explanations of why some people and some offences are pursued with greater vigour by the police than others (cf. the 'alcoholic petty thief' with the 'expense-account fiddler'); why some attract public or press outcries and others sneaking admiration (cf. 'masked bandits in payroll snatch' with the Great Train Robbery); why certain phenomena suddenly cause 'moral panics' - 'mods and rockers', 'Hell's Angels', 'skinheads', 'telephone vandalism', 'mugging' - when they have continued for years before (and after) under a different name. (7)

Criminology has lagged behind anthropology since the development of fieldwork. Although some criminologists are now becoming aware of debates about the philosophy of science, rationality, meaning, etc., the discipline suffers the disadvantage of not having undergone a lengthy fieldwork period. The 'deviancy theorists', a group of mainly young academics who meet regularly at the National Deviancy Conference, are attempting to make revolutionary changes in the subject, and have pro- duced some excellent studies of subjects like industrial sabotage, football hooliganism and drug-taking using essentially anthropological techniques linked with a Marxist perspective. (8) However, the danger is now apparent that lacking a substantial tradition of fieldwork, they will fall back into abstract social theory and 'lose the phenomenon'.

In any event, there is a pressing need for a readable and convincing alternative approach to be developed to combat the alarming implications of psychological positivist thinking as it has been developed by Hans Eysenck:

'The problem to be discussed is: how can we engineer a social consent which will make people behave in a socially adapted, law-abiding fashion, which will not lead to a breakdown of the intricately interwoven fabric of social life? Clearly we are failing to do this: the ever-increasing number of unofficial strikes, the ever-increasing statistics of crime of all sorts, the general alienation on which so many writers have commented are voluble witnesses to this statement. The psycholo-gist would answer that what was clearly required was a technology of consent - that is, a generally applicable method of inculcating suitable habits of socialised conduct into the citizens (and particularly the future citizens) of the country in question - or preferably the whole world.'

(1969:688).

Michael Maguire.

NOTES

1. Cesare Lombroso, Introduction to Ferrara 1911: xiv

2. This change is observable even between different editions of Lombroso's famous work 'L'Uomo Delinquente'. Between 1876 and 1897 he modified his views considerably.

3. T. Morris (1957: 41).

4. This phenomenon was well described by Lindesmith and Levin as early as 1937, and their criticism is developed by Taylor, Walton and Young (1973) chapter 2.

5. In Sellin and Wolfgang (eds) 1969 pp. 1-6.

6. E. Ardener (1962).

7. The term 'moral panic' was coined by Stanley Cohen. (1971).

8. Cf. Cohen (ed) 1971.

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                           definitions and behaviour.' Social Forces, 46:
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An Apparent Paradox in Mental Evolution.

One aspect of the current interest in feminist studies is the allegation that history has tended to ignore the contributions of women, irrespective of their worth, even when they did manage to penetrate the male-dominated professions. If one takes this premise seriously, then there is a case for reviewing the early anthropological journals to see whether there were any examples of 'sexism' at work. The term 'sexism' was deliberately coined to suggest a similar discrimination to that of 'racism'. It is interesting to find, therefore, that both sexism and racism were instrumental in the formation of the Anthropological Society.

According to J.W. Burrow, 'The immediate cause of the secession' of Sir James Hunt and a number of other leading members from the Ethnological Society who then founded the Anthropological Society in 1863, was 'the decision of the ethnologists to follow the example of the Royal Geographical Society and admit ladies to its meetings'. (1966:121.) Even if this was little more than a pretext, it served as a focus for controversy between the two societies, and the anthropologists believed that science and history were on their side; 'Sooner or later it will be learnt that the glory of scientific men will consist in the patient record of observed facts rather than in the fatal facility of being able to attract a crowd of both sexes to listen to equivocal science and still more equivocal pleasantries' (ibid: 125n5).

Burrow also put the record straight with regard to racism. J.L. Myres' paper 'The Influence of Anthropology on the course of Political Science' (1909) put forward the reason for the foundation of the Anthropological Society as a revolt of those who upheld the unity of mankind against the pro-slavery propaganda of the polygenist Ethnological Society. 'This is quite untrue. James Hunt, the president and most active member of the new society, was an ardent racialist, and so favourable to slavery as to be suspected of some sinister American or West Indian interest' (op.cit: 121). Moreover, the Ethnological Society was an offshoot of the Aborigines Protection Society, and its president, Crawfurd, condemned slavery.

In view of this beginning, it must have been with some trepidation that women ventured to give papers to meetings of the Anthropological Society, even more so, if the paper was intended to criticise the accepted view of the mental inferiority of 'savages'.

Anthropologists and psychologists conceived of mental evolution and the growth of intelligence as being closely allied .to the evident evolution of altruism and the development of ethical behaviour. Thus, primitives, children, and women were envisaged as illustrating a continuum from instinctive to intelligent, rational behaviour. Since the maternal instinct was thought to account for any altruistic sentiments in women, there was some confusion as to where they should be placed on this scale. There were those who wished to credit the development of all humane behaviour to the initial example of maternal care; but the majority classed the maternal instict as yet another example of instinctive action, and placed women as closer to animals because of this. Further confusion arose where there appeared to be a malfunctioning of the so called 'maternal instict' in ethnographic examples of the practice of infanticide, and mothers eating their own children (JAI 1872:78).

The superstitious practices recounted in the early ethnographies were accepted as complete justification for the 'scientific' view of their mental inferiority. Missionaries and travellers who had spent long periods exposed to these customs were more inclined to look for parallels in civilised societies, and their accounts stimulated the vogue for research into spiritualist practices.

Many of the more scientifically oriented members of the Anthropological Society criticised what they considered to be a useless digression of 'psychology' into the spiritual beliefs and practices of the savage. Mr. Dendy, after hearing the Rev. H. Calloway's paper on 'Divination etc. among the Natives of Natal' (JAI 1872:185) stated that he found it a most boring paper, that it was neither true nor new, and that such clairvoyance should be ridiculed as a pseudo-philosophy. Some years later, on the occasion of Herbert Spencer's paper 'The Comparative Psychology of Man' (JAI 1876: 301-315) Mr. Hyde Clark praised this approach to the study of psychology, which he felt had been held back by people wanting to talk about spiritualism and mesmerism (ibid: 316). Still later Ferrier's paper 'The Functional Topography of the Brain (JAI 1888:26-28) produced a similar response from Dr. Lauder Brunton, Sir James Chrichton Browne, and especially Mr. Hyde Clark, who had been elected chairman of the section for Comparative Psychology some years previously and had been unable to act, as the members had taken to spiritualist practices (ibid:31).

Tylor continually made detailed investigations into the prevalent spiritualistic practices in Britain and America as a part of his major interest in 'animism'. But for early anthropologists with a strong religious background, and experience abroad, like the Rev. Calloway, the interest in dreams, sympathy, and what he called 'presentiment' or premonitions, the phenomena were considered worthy of study for the light they threw on the Christian religion. Self-mesmerism for the purpose of clairvoyance, and the different methods of divination, especially through contacts with spirits affected by drugs and fasting were explained in his paper: 'As it is necessary in order that one   a mind should act on another that the two minds should be in a certain relation to each other, so a mind can only be influenced by good or evil spirits when it is in a state of sympathetic relation with them' (JAI 1872:180). He found that these practices interfered with his evangelism. His ideas prompted Mr. Jackson to hope that, 'Soon the dreams, divinations and ghosts of those nearer home as well as Kaffirs, will be considered subject for enquiry. Psychology of the savage does not differ from that of civilised man nearly so much as one might have supposed (ibid:185).

However, this was not the generally accepted view. The problem of the relationship between instinct and intelligence, irrational and rational behaviour remained until well into the next century. Yet there was one paper in these early journals which did attempt to come to terms with the issues. It was read by Galton in 1891, and written with fastidious scholarship by Lady Welby. This paper, 'An Apparent Paradox in Mental Evolution' (JAI 1891:304-325) attempted to put the irrational beliefs of savages into a new perspective. She questioned the established view that the savage was closer to nature, and more governed by his instincts than rational man, and most important, she challenged the understanding and the methods of investigating savage beliefs and ceremonies, offering an alternative which none of the members of the British Association, or the Anthropological Society, seem to have understood. The view she put forward challenged the evolutionary method of understanding primitive beliefs, and questioned their function as useful

adaptations. She wrote of their beliefs as exact parallels to those of modern men of science, who were also believers, and 'the authoress could not with propriety assume, off hand, that such men's religious belief was absurd' (ibid:326). The originality of her approach lay in her synthesis of ideas concerning language, symbols, and metaphor, 'emotional experience', and her decision to look at primitive cosmologies in their own right.

Lady Welby's contribution was almost obscured by her humble and tentative style and the exhaustive documentation of the current authorities. Their theories presented her with a paradox. According to evolutionary doctrines, the development and purpose of instincts was to enable animals and man, through these drives, to react correctly to environmental stimuli. But in the case of the animal 'man', who uses his brains to supplement his instincts, he gets the wrong answers to his questions, which result in such phenomena as animism and wasted efforts to placate the spirits and the dead. She drew attention to Spencer's comparison of the mind of the savage to that of a child who invested the natural world with spirits and animates objects for drama. (This idea of the 'natural' anthropomorphism of children had great tenacity despite the much later efforts of those such as Margaret Mead, who were motivated to disprove the false psychologism (JAI 1932:173-189)). Lady Welby did not find the analogy between children and primitives satisfactory, since education enabled children to discard the products of their imagination, whereas the savage stereotyped his fantasies, and they became like 'other habitual tendencies organised and perpetuated' (op.cit:306).

In order to understand how the over-developed imagination worked, she made use of the medical books explaining the location and function of the different powers of the mind: M. Foster's Central Nervous System; Maudsley's Cerebral Cortex and its work; Spencer's Principles of Psychology; William James' Principles of Psychology; Chrichton-Browne's Hygienic Uses of Imagination; and she was especially impressed with Bastian's use of symbolism in Brain, Organ of the Mind. If it was the imagination of primitives which led to their erroneous beliefs, Lady Welby thought it necessary to find a model of the way it worked. Since the senses linked the individual brain to the environmental stimuli through the nerves and the ganglion, and returned along other lines to the appropriate muscles, she found this process a convenient analogy to explain the act of imagination. But a touch of 'emotional experience' generally appeared to go to some emotional centre apparently at random, and thus set the wrong mental muscles in motion.

According to Mercier, 'conduct is the adjustment of the organism to its environment' (ibid:318). However, she could find nothing in his books, The Nervous System and the Mind, and Sanity and Insanity, to account for the highly developed fantasy of the savage which prevented him from adapting and learning from the environment like the ordinary rational man. 'When he (the lunatic) attempts to think out an elaborate course of conduct he falls into a state of confusion... he fails.... to estimate the comparative value of circumstances' (ibid:318). Lady Welby thought that the analogy between the mind of a lunatic and a savage was as misleading as that between the child and the savage. The confusion between rational understanding and imagination and emotion remained. She thought that savages would have died out if they did not possess logical powers which would enable them to adapt to their environment, and this was not the case. She based her understanding of primitive practices on Tylor's Primitive Culture (Vol.I), Frazer's Golden Bough (Vol.I), Max Müller's Physical Religion, and Dorman's Origin of Primitive Superstitions. She concluded that man also possessed, unfortunately, a 'middle centre' for emotion and imagination, which would explain wild beliefs and practices contrary to logical reasoning.

Man was accustomed to the discrepancy between reality and his sensations, and made allowances for these. Lady Welby quoted William James: 'From the day of our birth we have sought every hour of our lives to correct the apparent form of things, and translate it into the real form by keeping note of the way they are placed or held.' However, 'In no other class of sensation (than visual) does this incessant correction occur' (Principles of Psychology, Vol.11: 259-60).

Lady Welby noted, 'The average man is a slave to "habit" which has roots in physiological process' (op. cit:327). This was the vital point in her argument. The difference between the emotional and imaginative thinking of the savage, on the one hand, and the logical thinking of the civilised man, on the other, implied that there was a change in the way the mind developed; otherwise, she thought it would be necessary to investigate the primitive cosmology underlying those views and judge its relationship to reality as they understood it. She postulated two hypotheses:

(i) 'Either we are to suppose an absolute break and reversal in the evolution of mind; a stage of gratuitous incoherence in which the developing imagination has let go all the organised reactive power which up to that stage had made its owner what he was, and proceeds to create a burlesque of the universe.....', or she thought, one should at least ask whether -

(ii) 'we have, if not to assume that there be in primitive cosmology and natural history, an underlying element of true "mental shadow" of outward fact; an unbroken continuity of response in consciousness answering to the unbroken series of structure, function and organic reactions; a mine of valid suggestion, carried on within us and prompting more and more definite expression' (ibid:321). If there was an inherited 'responsive control' she thought this faculty should not have been lost: 'We have less instinctive power now....after.....weakening our ties with outer nature' (ibid:322). Lady Welby thought we might gain a better understanding of emotion, imagination and instinct by understanding what prompted the beliefs and ceremonies of savages who might not have lost this instinctive power.

Thus, for Lady Welby, the study of savage ceremonies and beliefs was not as the missionaries sometimes held, to throw light on Christian faith, nor to understand good, evil, prophecy, spiritualism and dreams, but in order better to understand the human mind itself, and how it works. For her the 'grotesque parodies' and 'ludicrous ceremonies' were 'failures of "translation"; failures to express worthily things which lie deep down at the centres of human experience, were true then and are true now, form part of natural order, and may soon for the first time be able to find scientific expression. If so, what is first needed, here as elsewhere, is an accession of power rightly to interpret "myth, ritual, religion", and mysticism in general. And this, not according to any dogmatic ghost-theory, dream-theory, sun-myth theories, or any other preconceived assumption....and in relation to its own merits....and the new school of psychology' (ibid:322-3).

Her notion of the 'new psychology' included not only the work of James, Spencer, Bastian and Shand, but also the study of language and symbols, and especially the use of metaphor, both by civilized and primitive man. She quoted from the Prefatory note to Part III of the Oxford New English Dictionary (known to the scholars of the period as Murray's), to explain the importance of the continual innovation of creative language as a psychological process:

'The creative period of language, the epoch of "roots" has never come to an end. The "Origin of Language" is not to be sought in a far off Indo-European antiquity, or in a still earlier pre-Aryan yore-time, it is the perennial process around us' (ibid:n.323). She thought that the clue to understanding reality lay in our use of language and metaphor especially: 'Cultured man connects "dreams" as he does "reflection" with an "inner" which he has acquired metaphysically - in an advanced mental stage. But to early man if not "outer" reality the dream would only be "inner" in the mucous membrane or the digestive cavity sense. And this sense of "outer" and "inner" may well be launched with us into the world of mind at its earliest stage' (ibid:314).

Lady Welby realised that metaphor was open to abuse as a method of understanding, but she thought greater attention should be paid to our own and the savage's use of symbols, as the choice of these symbols was of fundamental importance.

'Of course the tendency to right reasoning is quite different from a tendency to right organic response to a stimulus ... but the real question seems here to be where does the literal use of the phrase end and the metaphorical begin ... As to "breath" taken to represent and express the "dead" or the "double" it seems, on the usual assumptions, absurd. But question these, and of course there may be good reason for its symbolic selection, as there may be important realities which it symbolises better than anything else within reach could do... Are we quite sure that our tacit assumptions are invulnerable? Have we begun far enough up in the stream of experience "or penetrated far enough into the secret springs of mind" to justify them?' (ibid:328-9).

Her interest in studying savage myths and practices however 'grotesque' they might be as 'translations' and 'expressions' of a symbolic nature with a 'logical consistency' of their own, can be seen in this context as both proto-Freudian, and a forerunner of modern approaches to their study. However, it is evident from the discussion which followed her paper, and her reply to this in writing, that her paper was misunderstood (ibid:323-329). Pollock did not understand her use of the word 'translation'. She tried to reformulate her explanation that savages did not theorise in the 'modern way' but strove hard to use the function of expression to convey primordial impulses 'exploding' into fundamental organic energies. Thus the 'generic resemblance of belief' became part of the point proposed - 'and intimates links with the starting points of life' (ibid:328). She saw myth, religion, and rituals as a form of 'expression' used by savages 'conveying to each other certain primordial impulses within them as strongly as the nerve or blood currents, and as insistent in demanding outlet or prompting "explosion" as the most fundamental of organic energies' (ibid:328).

The points raised in the discussion were along familiar evolutionist lines. Pollock thought that archaic man reasoned incorrectly because he did not have the superior facts 'as we do'. Lewis commented that we did not know enough about the imagination of animals to know if there was a 'break' in evolution. Galton thought that superstition and illusion had proved useful in creating bellicose 'fanaticism', and Pollock thought savages' delusions would prove fatal when there was effective competition, and as the theory of the survival of the fittest was put to the test. Galton politely noted the novelty of her ideas to psychology and sociology, and that there appeared to be a break in evolution between instinct and reasoning, exemplified in the perverse imagination of savage minds. Mrs. Stopes, who was hearing the paper for the second time,

tried to be constructive. She suggested that the questions put forward by Lady Welby should be presented one at a time. Was there a break in mental evolution, and if not, was the evolution of an individual like the evolution of a race? The fact that she took this attitude suggests that she had a preconceived picture of the mental evolution of a child, and was specifically referring to Lady Welby's remarks about the education of civilised children, and the fact that savages stereotyped their fancies which became like 'other habitual tendencies organised and perpetuated'.

This attitude to primitive cosmologies can surely be seen as an early formulation of Lévy-Bruhl's idea of collective representations. One need not go further in pressing the point that she wished to look at the 'consistencies' in primitive beliefs and practices, which were evidently irrational and illogical to the scientific mind. The symbolic 'translation of things which lie deep at the centres of human experience', and 'the recent developments of the study of language, its growth and development on the figurative and psychological' (ibid:323) were just two of her contributions to the new approach that she urged. Since the development of anthropological theory allows for the retrospective adoption of founding fathers, perhaps one could nominate Lady Welby as a founding mother, and go on to try to find reasons why she thought as she did, and why she was misunderstood.

This allows me to attempt to put forward a line of argument which derives from a feminist approach. Firstly, it is interesting to see whether there were many other women represented in the first two decades of the JAI, and whether their interests were in any way similar. Miss A.W. Buckland presented two papers on drugs, surgery, and the superstitions of savages (JAI 1879:239-253; 1881:7-20), and Mrs. Sophia Bryant gave an account of intelligence tests which she had devised, which are typical of those in use today (JAI 1886:3-50). This common interest in the intelligence of children and primitives, and their practices, could be fortuitous. But if the early work on 'Ethnic Psychology' by Dunn (JAI 1875:255-265) and 'The Comparative Psychology of Man' by Spencer (JAI 1876:301-315) are admitted to provide the most general view accepted at that time by the Anthropological Society, then the spectrum of mental evolution ranged from lunatics, primitives, children, women to rational man. The women discussed the three other inferior groups, but did not mention their own vested interest in verifying or discrediting these views. Spencer's section on the relative mental nature of the sexes went into minute detail about the biological and social reasons for the mental differences, which he took for granted. The views on illogicality, emotionalism, lack of mental plasticity, incuriosity, laziness, lack of coherent or abstract thought, and so on, were applied to women and primitives alike. W.L. Distant's 'On the Mental Differences between the Sexes' (JAI 1875:78-85) reasoned that civilised women's brains were comparatively smaller than their menfolk as they had become playthings and ornaments. He compared this with the reduced brain of the domestic rabbit.

If they were to fight their own case the women had to put themselves forward as ethnographic examples, which was neither modest nor good tactics. In trying to direct interest and research towards a reassessment of the mentality of children and primitives in contradistinction to lunatics, they were moving in the right direction. They had to 'ape' the methods of their superiors in presenting a scholarly, logical and erudite treatment, and in the case of Lady Welby, it almost camouflaged her novel ideas. There are several interrelated factors which may have something to do with why and how she developed these ideas,

apart from the overt reasoning of her argument.

Firstly, she was a member of a group characterised as illogical and emotional, who were not expected to be familiar with the learned conventions of scientific and rational debate. It is perhaps legitimate to speculate that once she had mastered the latter, she would not necessarily feel that her previous life had been irrational and emotional, nor her psychology governed by bodily afflictions. The language of the educated male may or may not have provided her with a good lexicon for translating the apparently random, intuitive and vague statements of her own sex, and analysing them to make logical sense. Presumably she had to think of her previous understanding and interpretation of women's behaviour, or infants, or children's, as inferior. This understanding may have been tacit, instinctual and intuitive, and other such words that we use to describe 'things which lie deep at the centres of human experience, not yet scientifically understood, were true then and are true now, form part of natural order, and may soon for the first time be able to find scientific expression'.

Secondly, it might just be possible that the education received by the erudite men of the Anthropological Society had begun early enough to equip them with permanent blinkers that would prevent them from straying from the rational, logical, scientific way. Such a view of life was sufficiently distorted to prevent George Eliot's Mr Casaubon from ever glimpsing it at all. The educated women, on the other hand, moved in circles where they did not always converse with those of similar education, and could not dismiss their illogicality and irrationality on grounds of class, as men could. A betrayal of this code was dramatic, as in the example of Jane Austen's Emma who used the weapon of logicality to ridicule Miss Bates. In this case she was contravening the accepted code of mutual sympathy which permitted the real meaning of the conversation to be extracted from the random sentences.

Thus, as a member of an inferior human group, with the experience of understanding and conveying significant communications which were not usually amenable to scientific analysis, Lady Welby was, perhaps, herself, aware of the problems of trying to 'translate' and also to justify an unrespected cognitive code. It was to be expected that the trained minds of the Anthropological Society would be more resistant to accepting such a line of argument, at least in a pre-Freudian era. It is also, perhaps, both significant and arguable that a large number of women anthropologists have shown a definite preference for psychological studies and cognitive anthropology.

Juliet Blair.

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BOOK REVIEWS

Tahitians: Mind and Experience in the Society Islands.

Robert I. Levy. Illustrated by Pierre Heyman.

xxvii + 547pp. Chicago & London: University of Chicago

Press 1973.

Leenhardt once reported from Tahiti that the people had placed over the entrance to one of their primary schools, "as a sign of supreme beauty," the solitary inscription:

$$2 + 2 = 4$$

Whatever could have been in their minds to do something so austere and arresting?

The ethnographer writes of their enthusiasm for knowledge, and proposes that the Polynesians were seeking a religion that would revivify ancient mythical forms of experience and give these a new content. And doubtless they did find a beauty in this limpid paradigm of the new organisation of thought. But these suggested answers, as they stand, do not seem to meet the case. Naturally, we need to know far more about the circumstances, and we can readily conjecture the kinds of ethnographic detail that we should require in order to think deeper about the affair. Yet these too would probably not carry us far enough, through the avowed motives, into the premisses from which the Tahitians in question even began to think about the symbolic stand they were about to make. For we are starting from the wrong end, as it were; that is, from an everyday familarity with arithmetical formulas and the entire apparatus of numerical calculation into which we have been drilled since we first learned the tables of addition and multiplication. What we need to understand, then, is what it can be like to be without this knowledge, and to see $2 + 2 = 4$ as an illumination, a new sign of the power of abstraction. More precisely, we need to know what it is in fact like for certain Tahitians in certain circumstances to frame their thinking in non-traditional categories- and we cannot hope to understand such particulars a priori, for we are ignorant of the terms even in which our questions should properly be couched. What we should seek, therefore, is what Leenhardt has termed the "structural elements of their mentality".

There has now been published an ethnographic monograph on Tahitians, the subtitle to which refers precisely, and excitingly, to mind and experiences in the Society Islands. The author, Robert Levy, is a professor of anthropology at the University of California at San Diego (La Jolla), and was formerly a practising psychiatrist. The work is dedicated to Gregory Bateson. Three encomia on the back of the jacket, by American anthropologists, describe the book as a classic, praise its "sensitivity for Tahitian thought," call it a major theoretical contribution, and give readers to expect that it will enable us to comprehend "what goes on behind those handsome visages." Even if we take duly into account (as a matter, not for disparagement, but simply of different national styles in academic prose) the hyperbole that characterises American public judgements, whether in reviews or in university testimonials,

these panegyrical passages must encourage great hopes. It should be reported, too, that the University of Chicago Press has produced a distinctly handsome volume, attractively designed in a format fit indeed for an ethnographic classic, and graced with striking illustrations by Pierre Heyman.

Professor Levy worked in the Society Islands for a bit over two years, mainly in 1962-64. He evidently had a good command of Tahitian, and he was substantially aided with "copious marginalia" and otherwise by Ralph Gardener White, an expert on the language. Afterwards, and in a style that an English anthropologist can associate only with the astounding affluence and spaciousness of American academic life, he was able to reflect on his fieldwork during "some years of relative peace and quiet" as a senior fellow and then research associate at the University of Hawaii. Earlier versions of some sections of the book were read by a number of the author's colleagues, among whom the best recognisable here are Roy D'Andrade and Melford Spiro. And of course there was an immense fund of published and archival materials on the islands and their inhabitants, going back nearly two hundred years. So in practically every respect Tahitians has been as fortunately prepared as one could well look for.

In the event, there is indeed a great deal of patently sound ethnographic detail in Professor Levy's account, and it is plain that he has made a more than useful contribution to knowledge of Tahiti which will be of lasting value. He writes unpretentiously (his opening words are, disarmingly, "This is a first book ..."), and he succeeds throughout five hundred pages in sustaining an almost warm interest in those individuals whose lives he chiefly examines. The book is directed to two audiences: those who wish to learn about "the natural history of this sample of Polynesian life," and those more professionally concerned with problems of psychological anthropology and of "personality theory." A main thread of the exposition is provided by "psychodynamic" interviews with twenty individuals, recorded on tape. Centrally, the author is interested in his subjects' "experience as Tahitians" (his italics), and he says he believes his methods reveal much of this.

The monograph is divided into four parts. The first, "Orientations," sets the scene and introduces some of the actors. The second, "Shared Privacy," deals (chapter by chapter) with bodies, souls, and aspects of personal relationships. "Psychological Abstractions" treats of self and identity, thinking, feeling, and moral behaviour. The final section, "Organisation and Disorganisation," covers fantasy, adjustment and readjustment, aspects of growing up, the question of maintenance, and aspects of personal organisation. Two appendices record the check sheet used for psychodynamic interviews and a sample interview (about a dream). There is a useful glossary, followed by a bibliography and a good general index.

As a whole, and taken not too exigently, the work creates such an instructive, rewarding, and generally pleasing impression that one is rather reluctant to turn critical. And perhaps one

might not be much inclined to do so if only it were not for the
crucial words "mind and experience" in the subtitle. For in the
end it must be said somehow that <u>Tahitians</u> does not really make
the kind of contextual analysis of exotic categories which these
words encourage the reader to look for. A large proportion of the
book presents descriptions of customs in very much the fashion
of <u>Both Sides of Buka Passage</u>. (This is a compliment equally to
Miss Blackwood and to Dr. Levy.) Take the chapter on bodies in
Part II. It deals in succession with cleanliness, eating, exposure,
masturbation, supercision, sexual intercourse, homosexuality,
conception, pregnancy, childbirth, menstruation. This is all
good solid information, and interesting enough as far as it goes,
but by this point we are well over a hundred pages through the
text and we are still hardly in contact with what goes on inside
the Tahitians. Certainly there is no critical comparison of the
Tahitian psychological vocabulary with that of western discourse
and psychiatry. Actually, the author's prologue to this part
makes clear that this is an expectation that we must be prepared
to forego:

> I have sliced up behaviour, or rather abstractions
> at varying distances from behaviour (generalities
> about 'cleanliness' are less abstract than gener-
> alities about 'moral controls'), into gross cate-
> gories - 'bodies', 'souls', 'feelings', 'thinking' -
> purposely naive categories which are natural for me.
> Within these gross categories there are finer ones
> which take some account of native categories.

There we have it. The ethnographer relies on naive categories,
he says, which are "natural" for him - and he ventures to take no
more than "some account" of the categories of the Tahitians them-
selves. Now this would be entirely unexceptionable if it alone
were simply what he decided to do. Anthropological readers in a
certain intellectual tradition would still be considerably disap-
pointed, but they could not rightly complain that the author had
written the kind of book he wished and not what they would have
preferred. Yet the issue is not so clear-cut. Mind and experi-
ence, deliberately chosen as indicators of the essential subject
matter, have certain established connotations which here make it
necessary to go deeper than commonplace descriptive categories
permit; and to convey the distinctive characteristics of the
Tahitian conception of experience demands an exposition which is
itself premissed on those psychological and cognitive categories
which for the Tahitians themselves define, articulate, and in
some regards even constitute that experience.

Professor Levy, however, is admittedly on a quite different
tack. For instance, he does not state his own premisses when he
writes of mind and experience, and (as is confirmed by the paucity
of references under these words in the index) he does not attempt
to convey what, if any, are the equivalent concepts by which the
Tahitians discriminate among their apprehensions. The nearest he
comes to procuring us this interior view is in the chapters on
the self and on thinking, but although these accounts open promis-
ingly with grammatical considerations they prove to lead hardly

any distance into these fundamental notions. Instead of providing
the reader with a grasp of exotic concepts with which he can then
learn to acquire further categorical distinctions, as these are
effected in Tahitian collective thought, and eventually gain a
critical comprehension of alien modes of existence in Polynesia,
the author quickly reverts to his more usual manner of descrip-
tion. Instead of becoming more abstract, as is the explicit
intention, the exposition changes vocabulary as it proceeds but
stays at much the same level of behavioural anecdote, reminiscence
by the subjects, and more or less pertinent replies to the ethno-
grapher's questions. Taken in a repertorial sense, this style
of presentation contributes effectively enough (even if in a rather
rambling and slightly repetitious way) to a rounded picture of
Tahitian life, but in general by a process of factual accumulation
rather than by analysis.

     It will be unnecessary by this point to protest that none of
these observations is to be taken as derogatory, but only as
hinting at the respects in which Professor Levy has departed from
his own declared ambitions. In view particularly of the modesty
of his approach, moreover, it may be in place to suggest certain
comparisons and recourses by which his argument could better have
been made to reflect Tahitian ideas and apprehensions. The chapter
on the self recalls an example which for an Oxford social anthro-
pologist makes a classical beginning to such a study: Mauss's "Une
catégorie de l'esprit humain: la notion de personne, celle de 'moi'"
(1938). This essay in turn links directly to another work of the
same period: Lévy-Bruhl's perturbing Carnets, edited by Leenhardt
(1949). Then there is Leenhardt's own work Do Kamo: la personne
et le mythe dans le monde mélanésien (1947). This magnificent
but ill-recognised investigation into the meanings of two words
in New Caledonia presents itself indeed as the very pattern of
an enquiry into mind and experience in an alien tradition, and
it is genuinely a pity that Professor Levy should appear to have
been unacquainted with it. And subtending such invaluable para-
digms there is of course the fact that what Professor Levy ulti-
mately confronts are problems of comparative epistemology. On
this score the standard concepts of clinical psychiatry and
academic psychology, though doubtless apt enough to the under-
takings for which they were contrived, are not unquestionably
serciceable in the critical treatment of Tahitian categories. To
this end, given the pertinence of language and the emphasis on
inner experience, the Philosophical Investigations could have
given Professor Levy's investigation a far more probing and revel-
atory character. Also, as a final example of a kind, it would
be hard not to mention a recent enquiry, inspired largely by
Wittgenstein and by Levy-Bruhl, into the question whether belief
is an experience; for this deals precisely with what is taken
for a fundamental faculty of mind in its connexions with language,
alternative psychologies, and alien modes of experience.

     The point of these comparisons is by no means to claim that
one intellectual tradition (or, more trivially, one national style
of anthropology) is simply better than another, or to maintain
that the linguistic analysis of collective representations is
in principle more profitable than one carried out in the terms

of a western psychology. Professor Levy's dedication to Gregory
Bateson shows in 'itself that he is not so parochial or so part-
isan as to merit a blunt admonition. But he has devoted a fair
part of his life and a deal of earnest thought to the understanding
of Tahitians, and unless he becomes irrevocably distracted by his
current research in Nepal he may continue to publish about them.
It would be unprofessional at least, therefore, not to allude to
that scholarly tradition which once characterised Oxford social
anthropology and which, in the works of the late Sir Edward
Evans-Pritchard, showed its distinctive power to reveal certain
radical features of exotic forms of thought and action.

Rodney Needham.

I.A. Richards. Essays In His Honour. Edited by Reuben Brower,
Helen Vendler, and John Hollander. New York, Oxford University
Press, 1973. viii, 368pp., illus. £5.75.

Fifty years on from the first publication of The Meaning
of Meaning, Richards' colleagues, students, and friends have
seen fit to offer him this mark of their esteem. There are two
ways in which a contributor to a Festschrift may offer his respects
to the subject: either he chooses simple praise with the description--
perhaps elucidation--of the subject's thought; or he attempts to
push ahead with theories in the direction and manner of his predecessor,
Most of the authors in this volume have chosen the former approach.
There is a good deal of biographical detail, and with the notable
exception of Hartman's essay on psychoesthetics, the essays are
very much about Richards rather than inspired by him. While this
approach may seem less adventurous, the essays produced are often
more interesting or more useful.

This is certainly true in the case of this volume. Hartman's
and Cleanth Brooks' essay (on the concept of tension) may make
greater contributions, in the long run, to the theory of literary
criticism; but the essays which stay in the mind are Reuben Brower's
skilfully conducted interview with Richards, and Janet Adam Smith's
enticing and beautifully written piece on Ivor and Dorothy Richards
as mountaineers.

But apart from these more personal themes, and the largely
uninspiring dedicatory poems, the volume lacks a certain breadth
compared with Richards' own wide-ranging interests. Particularly
disappointing from an anthropological point of view is the failure
to treat in any serious way Richards' approach to problems of
translation, best exemplified in Mencius on the Mind. This is a
fault of the editors, who are all professors of English at
American universities. A survey of the list of contributors :.
reveals that three-quarters of them are or have been academic literary
critics. But one then realises that nearly all of these are
distinguished ex-students of Richards', so that one can understand
this editorial weakness.

In terms of what the book aims to be, rather than what it might
have been, however, the book is a complete success. All the essays
repay reading and pay true credit to Richards' greatness. They
are the very least that he deserves.

Martin Cantor.

Women in Between.   Marilyn Stratherne.   London. Seminar Press. 1972.

Dr. Stratherne's title is intriguing.   At the outset one wonders whether her use of the preposition and the implied spatial image would introduce the notion of women as agents of cultural transformation; a description of the role of women one might have wished for from Lévi-Strauss.   His lengthy writing on the subject of food would seem to have demanded some such treatment of the subject of women, but in Stratherne's work, like Lévi-Strauss', no such considerations were forthcoming.   The title was taken from a sub-heading of a chapter entitled 'Judicial Status', but this reviewer did not think the title represents a real theme of the book, nor an idea which was fully developed in the work.

Women in Between, to Stratherne, simply means women between two kin groups.   But, for women, there is an unevenness, an inequality in the arrangement (of mutual transactions) for although she is a 'road' for men, she has no road; she is powerless to act on her own. She has limited contacts, her prestige derives from her dependence on men.   Paradoxically, Stratherne mentions Hagenece categories which might show that Hagen women see their situation somewhat different-ly from men, as they are able, we are told, to claim and achieve a degree of autonomy.

I use the word 'paradox' because in my view, Stratherne does not develop any of her ideas on women in a clear and precise manner, because nowhere does she treat kinship, or any other aspect of Hagen society, as a linguistically oriented subject.   The result is that her views on Hagen women, explained in a totally functionalist theoretical context, are given without the benefit of detailed ex-planation of the Hagen meanings of kin relations or any other aspect of their society.

This point is crucial, for Stratherne concentrates on marital relations of the Hagenese for several reasons; inter-group and inter-sexual relations and the domestic roles of women are at the centre of her thesis.   One would have thought, therefore, that Hagen linguistic categories, classifications and cosmology would be of supreme importance as evidence for such central role definitions. However, the indigenous modes of thought only occasionally (and then partially) manage to struggle through.   Why?   Too often the heavy-handed imposition of western marital categories is apparent and it is simply frustrating, because in other sections, it is equally clear that these categories have little or nothing to do with the way the Hagenese view life.   Some insights into this are provided in a section entitled 'Husband and Wife: the supernatural dimension'. Here, we are told that ties between men and women extend beyond physical death in many ways.   We are told, for example, that women say that after death the spirits of husband and wife find each other again.   As in her lifetime, a woman's 'min' (spirit) may wander around and visit her clansmen, but it always returns to the abode of her husband's 'min'.   Claims spouses have over each other thus persist after death.   Indeed, most of the disputes, prestations, compensation payments, etc. surrounding arguments between Hagen men and women seem to have something to do with the claims of dead kin. I think it obvious that there is a cosmology indicated here, but it remains unexplained, hence un-understood.

We are told firstly, that Hagen notions of sexual relations, co-habitation, mutual domiciles, kin alliances, shells, pigs, etc.

are tied up with an intricate classificatory terminology which differs greatly from ours, mainly because there is a strong distinction made between men's usage and women's usage of the same terms (p.34); and secondly, that most of these terms are in some unexplained way intricately connected with Hagen notions of life and death. Unfortunately, Stratherne leaves these realities of Hagen thought to the reader's imagination. It appears that this is so because throughout her book, Stratherne uses what Hilary Henson (<u>British Social Anthropologists and Language</u>. Oxford University Press, 1974) has called 'associationally treacherous terms such as Father's Sister and Mother's Brother's Daughter' (p.102). Analytical categories such as 'clan', 'tribe', etc. are imposed 'carte blanche',

I find many paragraphs utterly confusing. The perplexity can be summed up in two questions: If it is true that in many cases the agnatic model is irrelevant, then why use it? If there is no comprehensive genealogical framework for the whole clan, then why bring it up in the first place?

It is clear that, to Stratherne, function — defined as her assigned categories - is obviously what the Hagen terms sprinkled throughout the book are taken to <u>mean.</u> This is made plain in the last sentence of her note on case histories at the beginning of the book:

"Cases" based on informants' accounts alone are distinguished by an asterisk. I take these as revealing about attitudes even if they are not accurate as to behaviour.

Doubts also arise as to the meaningfulness of the statistical samples given in the Appendices. The main problems are:

(i) the paradox which exists in the functionalist paradigm, i.e. that of dogged adherence to an empirical base without sophisticated quantification techniques for that base to rest on.

(ii) an obsessive preoccupation on an ideological level with 'typicality' and with 'normative' behaviour'. Out of 75,000 people, speaking at least two languages, from knowledge of how many were these 'norms' derived? How many people did Stratherne meet in eighteen months? Fifty? Two hundred? Five hundred?

(iii) a selection of inadequate samples with what seems to amount to an accompanying refusal to recognise that quantification of a people also means quantification of linguistic categories. The selection of a sample in the first place depends on non-quantifiable decisions.

In conclusion, if Stratherne's book had been published in 1932, a reviewer might be able to find ample historical justification for the defects to which the reader's attention has been drawn. But <u>Women in Between</u> was published in 1972, although theoretically and methodologically it belongs to the generations of Richards, Malinowski, Radcliffe Brown and Fortes. The saddening thing is that I now know nothing more about the Hagenese and New Guinea than I did before reading the book, but I do feel I know a lot more about social anthropology circa 1930, through the writing of an anthropologist who is obviously a bright, competent and very articulate exponent of that period.

Drid Williams.

Man and Woman among the Azande.   Edited by E.E. Evans-
Pritchard, London. Faber and Faber. 1974. £4.50

 This book avowedly aims to present what some Africans - to be precise
some Azande - are really like, how they talk and think, with only the
barest introduction and commentary.   The editor states that in their
writings anthropologists may have seemed to dehumanize Africans 'into
systems and structures and lost the flesh and blood', and he here tries
to let the Azande speak for themselves on a variety of topics concerned
with relations between men and women, and domestic life.   Most of the
texts presented were recorded by the editor or his clerk Reuben Rikita
between 1927 and 1930, but others written between 1961 and 1964 by
Richard Mambia and Angelo Beda are also made available.   Many of the
texts, all of which have been translated from Azande, have already
appeared in journals and books.   It was a happy idea to bring them
together in this way.   They make refreshing reading, and will parti-
cularly interest would-be social anthropologists who have not yet had the
opportunity to work in a field situation, illustrating as they do one
kind of data from which general statements are often drawn by social
anthropologists.   They will also be a very useful resource for analysts
for years to come.

 Although Professor Evans-Pritchard, with his usual modesty, has
attempted to avoid imposing his own views, a certain intrusion must have
been inevitable.   He notes that he 'did not elicit the texts', but it
would be idle to conclude from this statement that the Azande would have
spoken in the same manner, or indeed at all, if there had been no scribe
present.   He has also had the problem of choosing which texts to publish
here, and some editorial bias must be assumed.   Nevertheless he is
sensitive to the need to include 'what may seem irrelevancies' because,
as he says, 'they were not to the Azande who dictated them'.   It is no
doubt these 'irrelevancies' which will make this kind of contribution
especially valuable for future scholars.   Given the problem of observing
without being observed, or without influencing the observation, we are
probably in safer hands than in any others when Evans-Pritchard is
concerned.

 There are two small regrets: text follows text with only the
occasional minimal attribution (Mambia and Beda excluded).   How much more
helpful these would have been if we could have known at least at which
end of the age-range the commentator could be placed, let alone other
biographical details.   In this volume, which presents a series of distinct
though anonymous items from different sources, where the editor has
refrained from comment and left the readers to draw their own conclusions,
less anonymity and more personalisation would have been particularly
useful.

 A more serious cause for concern is that since this is intended to
be 'a presentation of an African way of reflecting on how men and women
see one another' and 'get along together', and 'how and African people'
look at these problems, more stress was not laid on the fact that this
book only provides evidence of a possible male view of the relations
between men and women.   A book of texts by Azande women might, of course,
carry exactly the same messages as this one: we have no way of knowing.
It is, however, certainly inadequate for the editor, in view of his
claims, merely to comment 'though I ought to add that all the texts in
this collection were taken down from men, who naturally had a bias in
their own favour'.

 With such reservations in mind, the volume is very welcome and may
set a precedent for others.   It in no way replaces those books of 'system
and structures' alluded to, but is a very valuable complement to them.

<div align="right">Shirley Ardener</div>

Anthropology and the Colonial Encounter. Edited by Talal Asad. London:
Ithaca Press. 1973. Library and paperback editions available (£5.50 and £1.00).

> '_[_The Anthropologist Northcote-Thomas_]_ was a recognised
> maniac in many ways, He wore sandals, even in this country,
> lived on vegetables, and was generally a rum person.
> _[_Clearly, Residents_]_ did not want to have an object
> like that going about ... partly because he was calculated
> to bring a certain amount of discredit on the white man's
> prestige.' (Colonial Office file, 1930)

It is at first sight curious how relatively long it has taken for
social anthropologists to see themselves as part of the colonial period -
less so, perhaps, when it is realized how alien to the colonial system
they always succeeded in seeming to be - even in their most respectable
phases. At least two quite unradical present-day professors had their
difficulties in those days - reports suppressed, or entry permits blocked.
Other social anthropologists were closely in touch with the colonial
independence parties, and remained persona grata in the successor states.
It is surprisingly difficult to think of an anthropologist who has been
barred from his field save by a militaristic or repressive regime. A few
reflections of this sort might lead some to feel that there may after all
be something about the subject that does help to moderate the ordinary
ethnic or class features of individual social anthropologists. We certainly
have to account for the contradiction between the marked conservatism of
ideas within social anthropology itself and the destructuring effect its
writings have on other conservatisms - an example, Talal Asad suggests, of
'bourgeois consciousness' transcending itself.

The contributors to this volume are essentially all puzzling over
this problem. Wendy James points out in some detail the highly critical
nature of some pre-war anthropology. As she reminds us, Kenyatta was
regarded as a particularly dangerous product of the Malinowski seminar (it
may be added that he changed his name from colonial 'Johnson' to 'Jomo'
during that time). Generally, however, the volume attempts a broadly
Marxist accommodation of the fact that there were possibly liberal, even
left-wing, individual social anthropologists with the undoubted fact of
their colonial context. There are useful accounts and resumés of the
nineteenth century origins (the Aborigines Protection Society and the rest),
and of the complicated relationship with Indirect Rule in the twentieth
century. Lackner uses official documents to good effect for Eastern Nigeria
on the latter subject.

The special cases of Nadel (Faris) and Godfrey Wilson (Brown) are examined.
Others (Asad & Clammer in particular) deal with the political perceptions shared
by administrators and anthropologists about exotic peoples. Feuchtwang and
Forster take us into recent Marxist analyses, including, in some detail,
the 'New Left critique'. Papers from the 'indigenous' side come from Willis
and Ahmed. A bibliographical digest is supplied by Marfleet. Asad's
introduction takes a middle view of the central problem, but perhaps all
the writers feel somewhat uncomfortable with it. The stamina required for
a treatment of knowledge as ideology, and their relationship to action,
must lead to some kind of questioning of the very structure in which studies
occur.

Since the writers hope for a Marxist solution, it is worth noting how
recent any awareness of the relevant, mainly French, literature has been
in social anthropology. This Journal itself pioneered such discussions.

It is not easy to recollect much serious mention of Althusser,
even of Godelier, Terray or Meillassoux, in other British anthropological
journals before 1971 - least of all from the one or two then acknowledged
senior Marxists in the subject. The story of the New Left critique,
which is referred to so often in this volume, should be mentioned, first
of all, to clear away the odd charge sometimes sporadically made (from
surprisingly conservative quarters) that the newer movements in social
anthropology are in some way 'elitist' and non-Marxist by definition.
The truth is rather that the newer Marxism was itself in part introduced
to favour among British social anthropologists by the same intellectual
currents that made vulgar functionalism untenable in other ways.

        Thus it was our student Jairus Banaji who, in his second term of
the Diploma, created the so-called 'New Left' Critique. Until then there
had been no 'critique', merely an article by Goddard, defective in
coverage, and clearly ignorant of many developments in social anthropology
since 1960. Banaji's response, based on the now fashionable authors,
was composed extremely rapidly - for this was the period when the
underdeveloped nature of much British anthropology made many contributions
from students more interesting than those available in the standard
literature. None will be more amused than Banaji that a definitive
milestone in anthropological Marxist criticism should have been so quickly
and so easily established, and should be cited so soberly for so many
years afterwards. It is an irony that the 'New Left' Critique should
stem from the world of this Journal to which he was a founder contributor,
as part of that 'new anthropology' to which his critique is now sometimes
cited as an alternative. It was not a traditional Marxist approach that
gave this early critique its edge, but rather its hints at the grinding
effect of structuralism and Marxism upon each other. To understand
French anthropological Marxism a knowledge of the strengths and weaknesses
of the Structuralist period is required.

        In taking the matter so far I move beyond the volume under review
to remark that there are high levels of ordinary functionalism in much of
the supposedly Marxist British work, as some of the discussion at the
1973 Decennial Conference session on Marxism showed. This is not
surprising, as it was through economic anthropology that many have come
to the writings of French Marxists. Yet it is difficult to believe that
the implications of some of the latter are understood. In purely
theoretical terms Godelier and his followers have outstripped British
economic anthropologists. They have modernised a field which had remained
unreconstructed since the sterile substantivist/formalist discussions of
the '60s. The modernization closely resembles that effected elsewhere in
the subject by the rise of structuralist and post-structuralist approaches.
But Godelier himself characteristically exceeds the traditional materialist
brief when he says 'we must learn to see reality as phantasma', or again, that
'mode of production will be located in different ways: we must learn to
see it even in religion' (oral discussion).

        The Godelier of the ASA, Decennial Conference in 1973, cannot be
easily accommodated within the sort of Marxism that British ex-functionalists
are likely to feel at home in. Indeed there was a little embarrassment
at the Conference when Professor Salisbury asked 'what distinguishes a
Marxist analysis from an ordinary anthropological analysis?' Maurice
Bloch replied - with intended humour - that 'all good economic anthropologists
had been doing Marxist studies'. The ecumenical and hardly radical note
of British anthropological Marxism is revealed again in the weight given
to Sir Raymond Firth's essay on the subject. Marxism is more serious an
enterprise than this. The switch from functionalism to Marxism as an

inexpensive way of building some intellect into the functionalist machine, runs the risk of holding up, and confusing, that anthropological restructuring of Marxism itself which is the main contribution of the French theorists. The latter are much too kind to the British: once more they are in too much awe of the famed British empiricism.

The present volume does not stem from the economic anthropology tradition and is not open to the full force of these criticisms. But the final lack of power in the papers, a kind of mesmerization which leaves the contributors and their subject more or less as they were, lies in an ultimate unwillingness to live mentally in the arduous kind of world their attempt at heightened awareness requires. Perhaps the relative juniority of some of the contributors makes them unprepared to face the erosion of the very structure of academic hierarchy by which they live. The story of anthropology shows how too many ideas are 'laundered' according to the prevailing ideas of the middle-class circles of each period. Yet it is surprising how few are prepared to risk the obloquy of choosing their own path, if necessary to their own detriment. Like Northcote-Thomas, with whom we began, they merely accept transfer to another (mental) colony. Edwin Ardener

SHORTER NOTICES

Structuralism: an Introduction. Edited by D. Robey. Oxford. Clarendon Press. 1973. £2.75 (paperback £1.25).

Yet another introduction to what is rapidly becoming yesterday's subject. It is already 75 years since Saussure's early statements, 35 years since Troubetskoy's, nearly 30 years since Lévi-Strauss's, 20 years since Leach's, 10 years more or less since the main British work – without considering all the other highly relevant theoretical streams. Although surprisingly weakest in the chapters on linguistic and mathematical structuralism, this is still a better set of essays (once Wolfson Lectures) than some on the topic. It is interesting, however, to see how semiotics, 'boundarism', and transformational generative grammar, as well as the views of Foucault, Lacan and the rest are simply collapsed together with structuralism. Too elementary for experts (not Hjelmslev's examples, and Berlin and Kay again!), and too outdated for students, it is literate and may interest the readership outside social anthropology that it aims for – without, perhaps, allaying its doubts.

The Mafia of a Sicilian Village, 1860-1960: A Study of Violent Peasant Entrepreneurs. Anton Blok. Oxford. Basil Blackwell. 1974. £5.25. xxxiii, 293 pp. illus.
Blackwell's Pavilion Series continues in its uninspiring but competent tradition. Most of the familiar themes go to make up the framework: social networks, entrepreneurs and the rest. An impressive body of facts on a subject fast attaining great general popularity, the book is perhaps most remarkable for its excellent photographs.

Friends of Friends: Networks, Manipulators and Coalitions. Jeremy Boissevain. Oxford. Basil Blackwell. 1974. Cloth £4.75, paper £2.25. xv, 285 pp.
The book attends to "the way interpersonal relations are structured and influenced". The approach predictably involves the general framework provided by the idea of taking an actor's view of his society, and analysing how he manipulates the other people and resources in his environment. The tone of the book is captured by the following quotation: "The most important structural criterion of a person's network, whether total or partial, is its size."