CONTENTS

EDITORIAL NOTE

The idea for this Journal has come from the graduate students at the Subfaculty of Anthropology at Oxford: in particular from those at the Institute of Social Anthropology. 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, and JASO sees this as its main purpose. The Oxford University Anthropological Society established a Journal Sub-Committee to organise the venture.

This autumn saw the departure from the Institute of Dr. John Beattie who has held a teaching post here since 1953. He has taken up the position of Professor of the Cultural and Social Anthropology of Africa in the University of Leiden. We wish him every happiness in his new home.

The article in this issue by Professor Evans-Pritchard is the first of a series which he has kindly promised to give us. They are based upon lectures on the history of social anthropology that he has given in Oxford over the years.

We should like to express our thanks to Nigel Barley for valuable assistance in the production of this issue of the Journal.

FORMAT

We shall produce one issue per term (three per year). Articles are welcome from students in all branches of anthropology and from people in other disciplines interested in social anthropology. Reviews and comments will also be welcome. For the present, it is preferred that the main emphasis should be on analytical discussion rather than on description or ethnography. Papers should be as short as is necessary to get the point over. As a general rule, they should not exceed 5,000 words. For future issues, papers should be submitted following the conventions for citations, notes and references used in the A.S.A. monographs. Communications should be addressed to the Editors, Institute of Social Anthropology, 51, Banbury Road, Oxford.

BACK ISSUES

We have a stock of back issues still unsold. Individual copies are available at 30p. in the U.K. and $1 abroad. Volume I complete (1970) is available at the following rates: U.K. - 75p. to individuals, £1 to institutions; abroad - $2.50 to individuals, $3 to institutions. The subscription for Vols. II (1971) and III (1972) are the same. (All prices cover postage). Cheques should be made out to the Editors.

HAS SOCIAL ANTHROPOLOGY A FUTURE ?

Much has been written and said recently about our subject's present unhappy condition and future prospects, if indeed it has any.[1]

If several recent pronouncements are to be believed, the outlook is gloomy. Needham thinks that social anthropology, 'which has in any case only a nebulous and unconvincing definition', 'is falling apart' (p.39). Its only hope, he argues, is to 'disappear' by way of a 'progressive dissolution', its disjecta membra being absorbed by philosophy, sociology, history, art, political science, psychology, and so on. Needham sees this cannibalistic orgy as an 'iridescent metamorphosis'. Jairus Banaji, too, sees social anthropology as 'dissolving': for him 'British social anthropology has been slowly and steadily disintegrating, its future distracted between disparate sectors of the "human" sciences' (pp. 71-72). Others have expressed similar pessimism. An anonymous reviewer in the Times Literary Supplement wrote in 1964 (4 June) that 'by the 1970s, the discipline (of social anthropology) will have to join forces with sociology or become an anachronism'. According to Levi-Strauss, 'Anthropology will survive in a changing world by allowing itself to perish in order to be born again under a new guise' (1966, p. 126). And if it should dissolve, he writes, 'this would not be for the benefit of any so-called social sciences (in his opinion 'there is no such thing as sociology'), but rather of the humanities; linguistics, philology, archaeology, history, philosophy' (1967, p.359).

All these authorities, and others, consider that the discipline of social anthropology is in a state of grave crisis. But is it ?

It is worth noting, to begin with, that social anthropology is not alone among the 'so-called social sciences' in undergoing at the moment such a crise de conscience: sociology (which malgré Lévi-Strauss unregenerately continues to exist) seems to be going through a comparably agonizing self-appraisal, to judge from some recent pronouncements by its exponents. Thus in a new academic weekly called Faculty (which appeared - and as quickly disappeared - towards the end of 1970) D.G. MacRae, the distinguished L.S.E. sociologist, published an article with the intriguing title (for which of course he may not have been responsible!) "How sociology found itself and lost itself in a lifetime". Although he concludes on a modestly hopeful note, he remarks that sociology has failed to satisfy all the hopes, in fact more 'practical' than theoretical, held out for it in the 1950s. The crises of the 1960s, he writes, 'found sociology apparently lacking in prescience, competence and conscience'. And in a recent review in the New York Review of Books (11 March, 1971) entitled 'Has Sociology a future ?', the sociologist Tom Bottomore refers to 'yet another diversion [in modern sociology] in the shape of Alvin Gouldner's "reflexive sociology", or, as Bottomore puts it, 'the sociologist contemplating his own navel'. He goes on to refer to W.G. Runiciman's view of 'the present confused state of sociology in which he (Runciman) can find neither a distinctive method nor a distinctive interest'.

So social anthropology's self-concern is not unique. None of us can afford to be complacent about the state of our discipline, but I cannot persuade myself that its present state is as bad as Needham, Banaji and company say it is. On the contrary, it seems to me to be alive and reasonably well in Oxford (as indeed the existence and quality of this journal might suggest) and in a number of other places. The dialogue between what Edwin Ardener in his Malinowski Lecture calls 'the new anthropology' (concerned with categories and concepts rather than with consequences and systems of action, with cognitive structures and 'programmes' rather than with functions, and with paradigms rather than

'syntagms') and the older 'structural-functionalism', not to mention other perhaps hardly less interesting dialogues, are, one might think, symptoms of vitality and growth, not of morbidity. And the fact, disquieting to some minds which seek some single, 'proper' way of doing anthropology, that different social anthropologists do, more or less interestingly and illuminatingly, a variety of different things, does not seem to me to be a weakness in what we are constantly and no doubt correctly told is an eclectic discipline. In The Concept of Mind Ryle described modern psychology as 'a partly fortuitous federation of inquiries and techniques', which neither has nor needs a locally trim statement of programme (quoted in Zangwill 1956). This state of affairs does not seem to bother psychologists, perhaps because they see more clearly than social anthropologists do that there is no reason why workers in what is nominally "the same" field should not study quite different, perhaps mutually incommensurable, kinds of problems. As Paul Heelas justly remarks (p.55), "for all scholarly ends it hardly matters what we label ourselves".[2]

If I am wrong, and the subject is on the way out, experience over the past few years suggests that, at least as an academic discipline, it is more likely to be taken over by sociology, than to be dispersed among a variety of established humanistic specialisms, as Lévi-Strauss and Needham suggest. One might of course hope that it would continue to interact with these, as it does now. And even this fate is, I believe, very much less likely now than it was even a few years ago. This is partly because of sociology's own dwindling assurance, mentioned above. But it is due also, and more importantly, to the growing recognition that social anthropology's new directions are away from rather than towards sociology, if, with the Dictionary of the Social Sciences, we define the latter subject as 'the scientific study of the social behaviour or social action of human beings'. I think that few social anthropologists today would define their subject as 'a branch of' sociology, as Mair did in 1965: even some sociologists now recognise that the two disciplines are different. Thus MacRae, in the article referred to earlier, remarked that the two disciplines 'were - not necessarily are - so close that there was nothing to stop the social anthropologist from doing sociology' (as in fact many did). The words 'not necessarily are' are significant, for they indicate MacRae's awareness that the growing edges of social anthropology are (as they have been for some years) increasingly on the non-'sociological' side.

Professor Evans-Pritchard has for long had reservations about the suitability of social anthropology for undergraduate courses, partly because so far the major contributions in the subject have been made by scholars who received their first training in other fields. There is much force in this opinion, but I would hold that enough social anthropology, some parts of it admittedly more valuable than others (and much of it due directly or indirectly to Evans-Pritchard himself), has been produced during the past half-century or so, for it now to be called into question. As long as seven years ago an anonymous reviewer in The Economist (not, I think, a professional social anthropologist) wrote: 'Social anthropology has come of age; it is a subject with a systematic body of knowledge developed enough to be presentable in the language of educated discourse' (13 June, 1964). I think that today this opinion can be defended, and that an undergraduate course in social anthropology, intelligently devised and taught in conjunction with allied disciplines, can not only inform but educate to an academic standard appropriate to a first degree. The teaching of social anthropology in schools does, however, give rise to grave, though not insuperable difficulties. Unless the subject be very carefully and sensitively taught, the impression which could be created by the tone, and more

especially by the titles, of some early classics might well do very much more harm than good. But whether we like it or not the demand for such courses exists and is growing, and one way or another it will be met.

In these days, when - it seems to me - interesting and sometimes original studies in social anthropology are being published from time to time (as well as, inevitably, a lot of rubbish), it is fair to ask exactly what it is that social anthropology's critics are complaining about. It is certainly possible to deny that any worth-while work is being, or has recently been, produced, but I do not think that this view can be sustained. Leaving aside the cosmic scholarly undertakings of Lévi-Strauss and his followers, whose attempts to establish the fundamental structures of human thinking can hardly, whatever one may think of them, be described as uninteresting or trivial, many more modest researches are, I believe, steadily advancing our understanding of human society and culture, both in concrete socio-cultural contexts (which is where research must anyway begin) and in general. And this I take to be what social anthropology is fundamentally about. In the course of the past two or three years I have reviewed - and therefore read - about half a dozen new books in the social anthropology of Africa, and I have learned something of interest from all of them, and a good deal from one or two of them[3]). Without claiming more than a superficial knowledge of most of the various areas of social anthropology, I can say that ever since I came into the subject there has always been something interesting going on in it somewhere. Is others' experience so very different ? And how much more than this is it reasonable to expect ?

I think that part of the difficulty is that social anthropology's critics are not always very explicit as to what it is that they think the subject <u>ought</u> to be doing. One cannot altogether avoid the impression that they are, perhaps unconsciously, looking for a kind of father-figure, a Messiah, who will lead them into a Promised Land with a new and revolutionary view of the human condition, in which all the old problems and ambiguities will disappear. They are dissatisfied with the piecemeal and for the most part gradual advance which must characterize by far the greater part of the development of any discipline. There have been, and no doubt will be again, revolutions (as well as rebellions) in anthropology, but revolution can hardly be sustained as a permanent condition. Working historians do not regard their subject as moribund because new philosophies of history are not continually being produced. They just get on with the job of writing history. There does not seem to me to be any very good reason why social anthropologists should not follow their example.

A further and more recalcitrant difficulty lies in the nature of the subject itself. Some social anthropologists find it hard to accept the untidy but (in my view) unescapable division of interest in social anthropology between the study of social relationships, 'action systems', on the one hand, and the study of classifications, symbols, and values, 'belief systems', on the other. As has been pointed out often enough, the two interests involve very different kinds of approaches and problems, but the study of either level very commonly - I would say necessarily in the case of the first - involves constant references to the other. If social anthropology were 'nothing but' the study of human social systems (and it is this 'nothing but' approach that I am criticising) then indeed it would be no more than 'a branch, or kind, of sociology', as Radcliffe-Brown and some of his successors have thought. And if it were 'nothing but' the study of category-systems, beliefs and values, it might well be regarded as a branch of philosophy, or psychology, or morals. In fact as it is practised it is, in large measure, all of these things.

We are all now well aware that in recent years the pendulum has swung away from the consideration of causes and 'functions' to the study of categories and meanings - to cognitive as opposed to 'social' structures[4]. This, as I understand it, is a central theme, fascinatingly developed, in Ardener's 'The new anthropology', though he emphasises that both paradigmatic and syntagmatic models have their places in our subject - the important thing is not to confuse them. In fact from the beginning anthropologists have been interested in, and have written about, ideal, conceptual data; it was only the (for a time) seductive charms of the organic analogy of the functionalists that persuaded them that they were not. It was not until the 1950s, partly through the impact of Evans-Pritchard's 1951 Marett Lecture, that this interest in concepts and categories began to become respectable again, at least in Britain. There were many references to this shift of interest in the 1950 and 1960s; I myself remarked in 1955, without any sense that I was saying anything original, on the current trend towards 'the study of systems of ideas and beliefs not exclusively from the functional point of view, but also as systems in their own right', calling for new types of analysis. Firth (1957), Pocock (1961) and a number of others have made the same observation, at greater or lesser length.

With this new and vigorous emphasis on social anthropology as essentially concerned with concepts and categories, with cognitive rather than 'social' structure, a concern with causal relationships has in some quarters become unfashionable, not to say downright unrespectable. But it seems to me evident that since what people do and say has consequences as well as meanings, we are bound, unless we take a needlessly restrictive view of our subject, to take account of both aspects, despite the untidy dualism, referred to above, which this involves. In fact we mostly have done so, though some have been more attracted to one dimension, others to the other. Indeed the pendulum may be thought to be beginning a counter-swing. I have already mentioned Barth, who, with his 'transaction' (a form of 'action') model has had a good deal of influence in some quarters. In a recent article (1970) Ruth Finnegan, whose specialism has been oral literature, writes (p. 193): 'Interesting as are ideologies, symbols and constitutional charters, the time is surely past when sociologists or historians or political scientists are content <u>only</u> to study such topics. They are also interested in the <u>actual</u> relations of individuals and groups, the interplay of power and the empirical facts on the ground'. So the pendulum swings; a motion which at least suggests that the clock has not run down. Of course, as Heelas, commenting on Ardener, points out, the ground - or earth - where empirical facts are supposed to be found does not exist in any simple sense. But what does ? For practical purposes Dr. Johnson's rebuttal of Berkeley's immaterialism is valid.

If we concede that social anthropology as it is practised has, whatever it 'ought' to have, both a sociological component (in so far as it looks at social action, choosing and decision-making, causes and consequences both intended and unintended), and a logico-philosophical, linguistic, hermeneutic and perhaps ultimately psychological component (in so far as it analyses human concepts and categories, the structures they exhibit and the conditions that underlie them), then we shall have to be a bit clearer than we generally are about social anthropology's relationship with sociology. I have said elsewhere (1964, pp.29-31) that it is <u>more</u> than sociology, as that term is usually understood, or at least defined, in that it studies ideas, beliefs, etc., as well as other aspects of culture such as art and oral literature, <u>in their own right</u> as well as in their relationship, if they have any, to systems of social action. But it seems to me that there are enough differences between what social anthropologists do <u>qua</u> sociologists and what sociologists do, even though some of these are

differences of emphasis, to justify keeping the two subjects distinct, at least for the foreseeable future. It may be useful to list here (but not to develop) six of what seem to me to be the most important of these differences.

First, although the two disciplines share a number of intellectual ancestors, their origins and histories were very different, and these differences have important implications, (some of which I touch on below) for the kinds of subjects they are today. Sociology grew from a philosophical interest in the nature of human society itself, combined (in Victorian England) with a practical concern with the problems of urban poverty and industrialization; anthropology looked outward at so-called 'primitive' peoples, first to provide supporting evidence for conjectures about the early stages in human history, later to learn about these peoples themselves.

So, secondly, the kinds of societies that sociologists and social anthropologists have worked in have for the most part differed sharply. Sociologists have mostly confined themselves to Western, industrialized societies, while social anthropologists have characteristically worked in remote and 'exotic' ones, usually small in scale, and in which most social relationships are face to face. That is, they have mostly worked in the context of 'communities', in Maciver's and Page's sense of that term (1950, pp. 8-9) which itself owes something to Tonnies' concept of Gemeinschaft. This is not of course to say that social anthropology as 'micro-sociology', can only be done in small-scale, 'simple' societies: as Banton (1964) has well said, 'the justification of social anthropology lies not in any claim to a distinctive subject-matter, but in the significant problems it has discovered, and the lines of explanation it has opened up'. But it is none the less true that significant problems and lines of explanation are likely to be different in different contexts.

The 'otherness' of the societies and cultures that social anthropologists have mostly studied has meant, thirdly, that they have from the beginning been centrally concerned with problems of translation and understanding - the hermeneutics of the subject - problems which are very much less acute, though they certainly exist, for sociologists. This interpretative process continues to be a primary concern of social anthropologists, as it is not for sociologists, who have been accustomed to work in milieux not totaly unfamiliar to them[5]). As Gellner has put it: 'Concepts and beliefs are, of course, of particular concern to social anthropology. Sociology can sometimes be a matter of ascertaining facts within an institutional framework which is taken for granted. The anthropologist can virtually never take anything for granted in this way....' (1962, p.153).

Fourth, it is a further consequence of the 'otherness' of social anthropology's traditional field that it could only be at all adequately studied by intensive fieldwork, by 'getting down off the verandah', in Malinowski's phrase, and living and working as far as possible as a member of the community being studied. This kind of 'total immersion' has neither played nor plays a comparable part in sociology. As already noted, usually the sociologist is already, in a sense, in the society he studies; the anthropologist has to get into it, and this can be difficult and even painful, as well as rewarding. It can be, and has been, argued that social anthropologists make a fetish of fieldwork, and certainly there is a danger of this. (The term 'fieldwork' is anyway particularly inept, but it is hard to think of a better one). Data have, however, to be collected, and this task, nowadays, calls for professional skills. As I have elsewhere remarked, 'if social anthropologists do not do their own fieldwork, it is certain that nobody else will do it for them'.

A further consequence of the 'otherness' of social anthropology's traditional field is (fifthly) that social anthropology has - so far - made much less use of quantitative methods that sociology has. This is partly because you cannot usefully quantify until you know what you are quantifying, and the understanding of familiar social and cultural data, a long and full-time job, is the social anthropologist's central concern. Also, it is plain that people's categories and classifications, a main interest of social anthropologists, are less susceptible to quantification than their patterns of social behaviour are. It is possible to overstress this difference between the two disciplines, as Edmund Leach perhaps does when, speaking of 'field sociology', he says (1967, p.77) that 'sociologists count things' (as opposed to understanding them), and rather implies that this is all they do. In fact sociologically minded social anthropologists are increasingly counting things too.

There is a sixth, final and rather important practical point to make. However one may estimate the degree of overlap which currently exists - or should exist - in theory between the two disciplines, as a matter of fact they are, for the most part, taught in universities as two quite separate and distinct subjects. Even when they are taught together in the same department, it is common for signs of fission to appear. It is only necessary to compare the entries under 'sociology' and 'social anthropology' in any bookseller's catalogue, or the bibliographies appended to introductory books in the two subjects, to see how very distinct <u>in practice</u> the teaching of them is. A limited quantitative analysis of a random sample of three introductions to sociology (Sprott 1949, Bottomore 1962 and Johnson 1961 - selected because I happened to have them in my study) revealed that less than 10 per cent of the very considerable number of books cited were what would usually be classed as social anthropology. Likewise, the coverage of works usually considered as 'sociology' in two popular introductions to social anthropology (Firth 1956, Lienhardt 1964) was barely over 10 per cent. So whatever may be thought desirable in principle, in fact the degree of overlap between the two disciplines, in regard to what students in each are expected to read, may well be of the order of approximately ten per cent. To suggest, as John Barnes does in an interview recently published (<u>Listener</u>, 5 August, 1971), that there is 'no distinction at all' between sociology and social anthropology seems hardly exact. They are, indeed, the closest of companion disciplines, sharing many common interests, and it would be both foolish and impracticable to attempt to draw a hard and fast line between them. But as things stand at present, they are clearly distinguishable from each other with regard to their histories, their characteristic methods and their main theoretical concerns, and only confusion can result from pretending otherwise.

I think, then, that the term social anthropology denotes a viable subject, grounded in a substantial and growing body of comparative data and theory, and oriented towards a wide and increasing variety of problems, on the levels of both 'action' and 'meaning'. This is enough to provide social anthropology with an identity, and to signal in the very broadest terms the lines of its development. It is neither possible nor desirable to be much more precise than this.

Let me conclude by taking a brief 'outside' view of social anthropology; from the point of view, that is, of some other disciplines, whose practitioners increasingly acknowledge that they have learnt something from social anthropology, as social anthropology has certainly learnt much from them. A few examples may make the point. Some professional philosophers (Winch, MacIntyre) have enthusiastically taken up Evans-Pritchard's famous study of Zande thought, and a recent collection of essays edited by the sociologist Bryan Wilson (1970) brings philosophers and social anthropologists

together in a debate on the kind of thinking involved in myth and magic. Two important recent books on witchcraft in England by the historians Keith Thomas and Alan Macfarlane acknowledge indebtedness to recent anthropology as well as to Evans-Pritchard's classic study. Both of these authors recently participated with social anthropologists in a symposium on witchcraft and sorcery (published as Douglas 1970). Theologians, lawyers and classicists have derived, and acknowledged, help from social anthropology, and continue to do so. Social anthropologists have collaborated even more extensively with the other social sciences, not only, or even pre-eminently, with sociology, but also (for example) with political science, social psychology and economics. The connection with linguistics is manifest. Even without a detailed inventory of recent cross-disciplinary bridges, an inventory which might in fact be very useful, there is ample evidence, for those who wish to take note of it, of social anthropology's contributions in a variety of contexts and over many years to longer-established disciplines. There is no reason why such contacts should not continue and increase. We need not wait for social anthropology's dissolution (as the Lévi-Strauss quotation given earlier in this paper might suggest) before the humanities can benefit from its findings. By their own account they are doing so already.

A dialogue must necessarily cease when one partner to it has been ingested by the others, and social anthropology's demise, and its fragmentation and assimilation into other disciplines, must evidently bring these processes of cross-fertilization to an end. This would seem to be a pity. So long as the exchanges involved are thought to be worth while by the parties to them, so long would there seem to be a strong case for resisting the death wish which the subject has generated in some of us, and for sustaining social anthropology, at any rate for the foreseeable future, as a distinct and separate discipline.

But not too separate. Social anthropology will indeed have no future if it is to be locked up in the ivory tower to which the protagonists of the purest and highest scholarship have sometimes seemed to wish to confine it. Such purists have sometimes given the impression that, in their view, social anthropology is not, and need not be, of the slightest practical use to anybody. Sometimes they even appear to be rather shocked that its findings should be made available in plain language to ordinary people. There are evident dangers in popularization. But the dangers of isolation and in-breeding are even greater. If social anthropology is prepared to 'come down off the verandah', and to rub shoulders with other disciplines and with other human concerns, practical as well as academic, without worrying too much about its state of intellectual and moral health, then, I suggest, the outlook for our subject is a good deal less bleak than some people seem to think.

John Beattie.

1) It might, for this reason seem unjustified to add yet a further note on the topic, especially as in a recent issue of this Journal (II, 1, Hilary 1971) Paul Heelas has provided a very fair summary of three of the most recent statements (Needham 1970, Banaji 1970 and Ardener 1971). These brief comments one offered only as a modest attempt to clarify - perhaps simplify - some of the issues involved.

2) There is, however, one context in which labels, even provisional ones, are, unfortunately, indispensable, and that is in the context of teaching the subject. The philosophically-minded social anthropologist may, if he can afford it, decline to concern himself with pedagogics and with the awkward practical problems which teaching involves; the professional teacher of the subject cannot. Students, both graduates and undergraduates, and even sixth forms, want to learn anthropology, and unless it is decided (as it cannot effectively be) that they shall not be allowed to do so, someone has to do the job of preparing curricula, deciding what shall be included and what excluded, and so on. And these matters cannot be determined in a vacuum; courses and curricula already exist, and it is the amending and expanding of these, rather than the composition from scratch of ideal curricula based on conceptions of what the subject ought to be, that is our practical concern. Of course if anthropology passes away it will no longer be there to study. But I am arguing that this is unlikely.

3) For the information of the curious, they are: Man in Africa (eds. M. Douglas & P.M. Kaberry), 1969; Tradition and Transition in East Africa (ed. P. Gulliver), 1969; Oracles et Ordalies chez les Nzakara (A. Retel-Laurentin), 1969; Witchcraft, Sorcery & Social Categories among the Safwa (A. Harwood), 1970; Kalahari Village Politics (A. Kuper), 1970; African Elite (J. Vincent), 1971; and Technology, Tradition and the State in Africa (J. Goody), 1971.

4) Of course not all contemporary social anthropologists go along with this trend. Fredrik Barth, for example, pleads for 'generative models' to explain process; individuals pursuing goals and making choices: his model derives from games theory, not from Chomsky and the grammarians.

5) This needs some qualification, but as a statement of historical fact it is essentially true.

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THE INTERPRETATION OF NDEMBU RITUAL ACTION

In this article, I hope to cast doubts upon the prevalent assessment of Turner's work by anthropologists which, whilst criticising his orientation in his interpretations of Ndembu ritual action, judges his method to be correct. I therefore attempt a critical review of Turner's method in which my concern is to demonstrate how certain of his basic theories on the nature of ritual symbols derive from the usage of a construct which is inappropriate to a study of the 'meaning' of ritual symbols. In the second section, I examine the development of Turner's 'bi-polarity of reference' theory, the origins of which are different from those of the theories examined in the first section. In the final section of this article, I attempt a re-interpretation of the purely observational components of the 'bi-polarity of reference' theory, on the basis of which I suggest a new type of interpretation of Ndembu ritual action.

TURNER'S METHOD

The construct which Turner presents as the 'meaning' of the ritual symbol is a type of 'gestalt'. Such a construct can possess only two properties: content and boundary. On the subject of content, Turner's ideas are more consistent than on the subject of boundary. Hence, as the content of the 'gestalt' he consistently envisages symbolic objects, symbolic actions and cultural beliefs. Such an analysis therefore proceeds by noting and collating the following classes of data:

(i) the symbolic objects and actions which occur in proximity to one another within a ritual performance;

(ii) cultural beliefs associated with the above symbolic objects and actions, achieved by means of the collection of indigenous exegesis of their usage;

(iii) a further set of symbolic objects and actions, occurring within the same and different types of ritual, which indigenous informants relate, by means of exegetical statements, to the first set of symbolic objects and actions;

(iv) a further set of cultural beliefs associated with this second set of symbolic objects and actions; etc.

Immediately it becomes apparent that, unless we wish to establish the boundary of the 'gestalt' by means of a criterion unrelated to its content, only two boundaries deriving from the nature of the content present themselves

1) The inclusion of classes (i) and (ii) and exclusion of classes (iii) and (iv) within the 'gestalt', so that the 'gestalt' contains only juxtaposed symbolic objects and actions and the beliefs associated with them; or

2) The duplication of the procedures adopted above an infinite number of times. In this alternative, we can either conceive of the boundary of the 'gestalt' as enclosing the totality of Ndembu symbolic objects and actions and the beliefs associated with each object and action or, regarding such a boundary as co-terminous with a construct of 'culture', conceive of the boundary of the 'gestalt' as the boundary of the culture.

For the sake of brevity, I shall term these two constructs (1) 'the finite content-bounded 'gestalt'' and (2) the 'infinite content-bounded 'gestalt''.

In the case of the finite content-bounded 'gestalt', such a construct has its uses if we then argue that an objective of ritual action is the creation, or maintenance, of such a 'gestalt'. For, since sets of symbolic actions and objects vary between different types of ritual, we can then argue that different rituals maintain different 'gestalten', which endure, in the minds of the ritual actors, for a short period of time after the termination of the ritual. We can now apply this idea to Turner's most extensively documented example of a 'gestalt': the mudyi symbolism within the Nkang'a ritual, first analysed by Turner in 'Symbols in Ndembu Ritual' (Turner: 1967: 20-5).

Turner notes that the mudyi symbol exhibits bi-polarity of reference, the two referents being: breast-feeding and relationships between matrikin (fellow-villagers). The mudyi symbol therefore asserts an identity between two discrete areas of experience: the situation of breast-feeding and relationships between fellow-villagers. The Nkang'a ritual is not performed in response to a crisis situation, hence the objective of maintaining such a 'gestalt' must be to deal with a situation which is endemic to relationships between fellow-villagers. I suggest that, if Nkang'a is performed in a village split by factionalism, a characteristic of 'long-established' Ndembu villages, such a 'gestalt' stresses the mutual interdependence of fellow-villagers in opposition to the independence of two or more groups of more closely related matrikin within the village from one another, which is the manifestation of factionalism.

Mutual interdependence is a property which is perceptually manifest within the 'breast-feeding' area of experience, but which constitutes a conceptual property, variable through time, of the 'relationships between fellow-villagers' area of experience. Hence, accepting the type of interpretation which I have presented, the ritual symbol is here seen to 'structure' one area of experience by means of a property perceptually manifest within another area of experience.

The utility of the finite content-bounded 'gestalt' thus consists in the differentiation which is achieved through its usage between different types of ritual, the 'cognitive structures' of which closely resemble one another. 'Cognitive structures' refers to an aspect of the Ndembu ritual system entirely neglected by Turner. Briefly, if we correlate compatibilities and incompatibilities demonstrated by ritual action between juxtaposed ritual objects, we discern that these objects are arranged into a system of symbol classes, co-members of which are substituted for one another within rituals. The fourfold system of classes discerned expresses the fourfold system of social and supernatural categories based upon the two oppositions of 'male'/'female' and 'fertile'/'infertile'. The 'cognitive structure' of all Ndembu rituals is then seen to be the spatial separation of symbolic representations of the 'fertile' and 'infertile' conceptual categories, most clearly discerned in the 'casting out' of the patient, or 'ritual subject', designated 'infertile', from amongst the social group, designated 'fertile'.) For example, if we consider the 'gestalt' maintained by the mukula symbol within the Nkula ritual (Turner: 1968: 82-7), the cognitive structure of which closely corresponds to that of Nkang'a, we discern that it is not merely different from the mudyi 'gestalt', but non-comparable with it. We can argue that the mukula symbol 'structures' the area of experience of the woman who menstruates by means of properties perceptually manifest within the area of experience of the man who hunts. Thus, it can be argued that the mukula 'gestalt' maintains the dual division of 'fertile' sexual categories by maintaining that an item which cannot be classified within the one - the menstruating woman - must be classified within the other - the male (hunter) social category.

My criticism of the usage of the finite content-bounded 'gestalt' construct in the interpretation of ritual action is merely that it reflects the specific characteristics of the crisis situation existing prior to ritual performance and does not illuminate how Ndembu represent crisis situations in general as the existence of the 'infertile' in the midst of the 'fertile', a representation which can be discerned only by means of an examination of the morphology of symbol classes and the re-distribution of members of these classes between spatial categories within the ritual performance. However, this is a criticism of orientation and not of method, and therefore not strictly relevant in the present context.

Turner does not use a finite content-bounded 'gestalt' construct, rather his 'gestalt' construct exhibits a confusion between the infinite content-bounded 'gestalt' which, as I have argued, can only mean the totality of cultural beliefs associated with ritual action and, hence, 'culture' or 'society' itself, and the bounding of the 'gestalt' by means of a criterion - 'dominance' - unrelated to its content. The infinite content-bounded 'gestalt' construct implies that the appearance of a symbol in one type of ritual 'recalls', in the minds of the indigenous actors, its appearance in another type of ritual. The supposition is valid given certain qualifications. For example, the 'gestalten' created by ritual performances are subject to erosion through time, otherwise we cannot argue that an objective of ritual action is their maintenance. Given the nature of Ndembu cult organisation (the exclusion of women from men's cults and 'vice versa'; adeptness of adult Ndembu in one or two types of ritual only; travelling of adult Ndembu great distances in order to attend rituals performed by their own cult) it is obvious that no individual can be aware of all the contexts, throughout the ritual system, in which a single symbol appears; and that the previous types of ritual attended by each member of a social group within which a ritual is being performed will be different, hence the context in which the same symbol has last been apparent to each member of the group will be different, so that the context 'recalled' will vary with each individual ritual actor. Therefore, the appearance of a symbol in one type of ritual can neither be said to 'recall' all the contexts within which it appears throughout the ritual system, nor can it be said to 'recall' the same context in another type of ritual for every ritual actor.

If we argue that the appearance of a symbol in one type of ritual 'recalls' its appearance in another type of ritual, it is then possible to argue that the other symbolic objects with which the first is juxtaposed in the other type of ritual are therefore 'recalled' by the appearance of the first symbol in the contemporary ritual performance. If we then regard the 'meaning' of ritual symbols as existing on the level of the set of juxtaposed symbolic objects, it becomes possible to argue that the meaning which is 'recalled' is different from the meaning which is manifest in the contemporary ritual performance. However, we must bear in mind that the 'recalled' meanings vary with each ritual actor and that no individual ritual actor can 'recall' all such meanings which the single symbol can be said to possess throughout the ritual system. The only sense in which all such meanings can be said to be 'recalled' within a single ritual performance is a collective one: between them, the total ritual assembly could 'recall' all such meanings. But it would here be methodologically incorrect to argue that all such meanings constitute the reference of a single symbol, so that the single symbol 'recalls' the totality of indigenous 'culture' or 'society', because the 'gestalt' construct refers to a collection of items existing within the minds of indigenous actors, an attribute which such an 'extended' reference of the ritual symbol does not possess.

Therefore, the only sense in which the 'recall' notion implicit in the infinite content-bounded 'gestalt' construct is applicable reduces to a matter of individual variability. Since, in an analysis of the types of common alterations which are effected within individual Ndembu by means of ritual performance, such individual variability must be discounted, the usage of an infinite content-bounded 'gestalt' is seen to be methodologically incorrect, since the notion of 'recall' implicit in such a construct reduces to such individual variability. However, it is on this very notion of 'recall' that Turner bases his theory of ritual symbolism:

"..even though only a single designation of that symbol is situationally manifest, the 'penumbra' of latent senses to be manifest in other 'positional' combinations is nevertheless present." (Turner: 1969a: 13).

In Turner's terminology, 'positional combination' refers to the single context in which a symbol is juxtaposed with other symbols, and 'positional meaning' refers to the meaning of the object-set (set of juxtaposed symbols) in a single 'positional combination', which is regarded as co-terminous with the meaning of any single symbol within it. Since, within the totality of Ndembu rituals, virtually every type of symbolic object appears juxtaposed with almost every other type of symbolic object, Turner is forced to impose a restriction upon the number of positional combinations, or object-sets, which he will consider in his analysis of the ritual system, the totality of which, he argues, constitutes the 'total' meaning of any single symbol.

The criterion which he adopts is that of 'dominance'. When he first introduces the criterion (Turner: 1962: 70), it is manifestly based upon the extent of ritual action directed at a ritual symbol within a ritual performance or part of a ritual performance. However, subsequent definitions equate this criterion with that of 'relating to supernatural beings or forces':

"..dominant symbols are closely associated with nonempirical beings.. nonempirical powers or kinds of efficacy." (Turner: 1967: 31)

The criterion is therefore applied by means of noting association with supernatural beings through exegetical remarks or observation which suggests that 'protective influence..is believed to emanate over everyone involved' (Turner: 1962: 70) from the symbolic object or object-set.

As might be expected, the construct arrived at by means of the infinite content-bounded 'gestalt' and the 'dominance' criterion is a haphazard collection of objects, actions and beliefs which bears little relation, on the one hand, to representations existing within the minds of indigenous actors or, on the other hand, to a consistent set of properties of symbolic objects selected by the anthropologist. Yet, rather than scrutinize the method by means of which the construct is arrived at, Turner proceeds to regard these 'properties' of ritual symbols as empirical realities and to explain their role in efficacious ritual performance, rather than to regard them merely as the inevitable conclusions of a confused and inappropriate methodology.

The 'dominance' criterion does not alter the boundary of Turner's 'gestalt' construct, although it is used to locate the boundary. The boundary remains infinite. The reduction of the number of positional combinations to be taken into account which he achieves by means of the criterion merely allows plausible exposition of the 'gestalt' construct. He is therefore able to use the 'property' of the infinite content-bounded 'gestalt' - that the reference of a single symbol can be said to be the totality of cultural beliefs associated with ritual action - and argue that this construct is co-terminous with 'culture' and 'society':

"..ritual custom..is the concentration of custom, its refined extract as it were." (Turner: 1968: 23)

He then develops the idea that the 'meanings' of 'dominant' symbols comprise a haphazard collection of conflicting and mutually incompatible objects, actions and beliefs in two ways. Firstly, he argues that the role of such a 'phenomenon' is the achievement of paradox. Paradox achieves a form of 'dérèglement' in the minds of the ritual actors which temporarily breaks through the 'habitual patterns formed by secular custom, rational thinking and common sense' and induces religious experience (Turner: 1962: 85-6).

Secondly, Turner relates this incompatibility between the various 'meanings' (positional meanings) which he attributes to the same dominant symbol to incompatibility between the principles of residential affiliation (matriliny and virilocal residence) which he presents in 'Schism and Continuity in an African Society', and, thereby, to a conflict of loyalties for the individual Ndembu between the two sets of kin involved. He then regards this 'conflict' between structural principles as a major cause of the crisis situation which impels ritual performance. The 'dominant symbol' is then regarded as efficacious in reducing the incompatibility perceived by indigenous actors between the structural principles because it cloaks the multiplicity of conflicting rules with the apparent unity of their symbolic representation.

He then combines the two 'properties' of the dominant symbol, that it expresses the totality of 'tribal custom' or 'society' and that it achieves the unitary representation or expression of multiple conflicting rules, by arguing that, not only does the dominant symbol achieve the unitary representation of multiple conflicting structural principles, but that it 're-socializes' the ritual actors by impressing upon them the totality of Ndembu tribal custom or 'culture'. Hence, the 'conflict' is not merely disguised by means of the unitary representation of the conflicting rules, it is, as it were, 'submerged' beneath the totality of Ndembu culture, set against which it becomes insignificant:

"..the..dominant symbol..in its aggregate of meanings stands for unity and continuity of the widest Ndembu society, embracing its contradictions." (Turner: 1967: 46).

Hopefully I have demonstrated, in this section of the article, how Turner's most basic theories on the nature of ritual symbols are derived from a series of illogicalities generated by a method which is inadequate for the tasks which he undertakes.

THE 'BI-POLARITY OF REFERENCE' THEORY

My reason for discussing Turner's theory of the bi-polarity of reference of ritual symbols independently in this section is that, unlike the theories of symbolism discussed in the last section, it does not derive from the properties of the type of 'gestalt' construct used by Turner. Rather the bi-polarity of reference theory can be viewed as an observation which is interpreted and developed by means of a Freudian concept of personality.

Exegetical texts collected by Turner reveal that Ndembu associate ritual symbols with body fluids and emissions, such as blood, semem, faeces etc. From this observation, Turner concludes that, when the symbols associated with these body fluids and emissions appear in ritual, they 'recall', for the ritual actors, the non-ritual situations in which they commonly occur. Turner's development of this idea is carried out in terms

of his earliest theoretical orientation: an attempt to incorporate a Freudian view of personality into Gluckman's 'Rituals of Rebellion' thesis (Gluckman: 1954: 1963). Conscious of the shortcomings of Gluckman's location of the source of 'conflict' in ritual - between social groups or categories - Turner locates the source of such conflict within the individual psyche, between the 'socialised' and 'unsocialised' aspects of the personality and envisages a 'transference of affect' from the latter to the former within the ritual performance:

"I would like to postulate that the whole strength of the rebellious affect which is released in and through the ritual is transferred to the 'official' social order, not merely purged and allowed, as it were, to evaporate." (Turner: 1955: 53-4).

In 'Schism and Continuity', Turner applies the Freudian notion to his own Ndembu material:

"Ritual is the social mechanism by which a group is purged of the anarchic and disruptive impulses which threaten its crucial norms and values. These impulses are present in the majority of its members and come dangerously near to overt expression if there has been a long series of quarrels between its members." (Turner: 1957: 124).

He now takes the existence of body fluids and emissions amongst the referents of ritual symbols as evidence for this formulation:

"At one pole (of the ritual symbol's meaning) there is a cluster of referents to organic and physiological phenomena; at the other, a cluster of referents to the norms and values of society..it is the socially recognised organic pole of reference that appears to rouse feelings and impulses in the Ndembu ritual situation..the emotions, which, as psychoanalysts have shown, may often be connected with illicit and socially reprobated impulses..are purified by their association with morality and law. It is as though the 'energy' of virtue flowed from organic and primitive sources, though the original goals of the drives were altered.. In this way the obligatory is made desirable, and the desirable allowed a legitimate outlet. Again it would seem that the needs of the individual biopsychical organism and the needs of society, in many respects opposed, come to terms with one another in the master-symbols of Ndembu society.. what can be shown to be infantile murderous and cannibalistic impulses are transmuted into zeal on behalf of certain moral imperatives and legal rules." (Turner: 1968: 18-19).

The relationship which Turner posits between the 'organic' referents of the ritual symbol and drive reduction cannot be proven or disproven within the limits of anthropological competence. But what is clear about the theory is that it is developed in isolation from Turner's own ethnographic material, then imposed upon his own material at a later date. The most apt type of criticism of such a formulation is, therefore, an analysis of these 'organic' referents of ritual symbols which is independent of any pre-conceived theory, which results in an interpretation of their role in the ritual performance which can then be compared with the role which Turner assigns to them. In the next section of this article I attempt such an analysis.

THE PHYSIOLOGICAL/BEHAVIOURAL ANALOGY

Since the 'organic' referents of ritual symbols include foods, specifically breast milk (Turner: 1968: 18) and animal meat (Turner: 1967: 78), I shall include foods in the present analysis, so that the object of the present enquiry is to examine how Ndembu classify foods and body fluids and emissions. In ritual, two types of food - cassava and animal meat - are used as symbolic objects, and the opposition between

the two foods is related to the opposition between 'inhabited' and 'uninhabited' territory, since cassava is grown in 'inhabited' territory (the streamside and the periphery of the village) and animal meat is hunted in 'uninhabited' territory (the bush). The white/red opposition of the colours of the two foods is also, I suggest, used in ritual to express the opposition between inhabited and uninhabited territory.

Turner notes that Ndembu associate body fluids and emissions with specific colours or combinations of colours (Turner, 1967, 74-9). The only fluids and emissions associated with the colours red and white or the colour combination 'red+white' are: the semen of a fertile man and breast milk. Hence, we can conclude that Ndembu associate foods and fluids and emissions which occur in a reproductive context with the colours red and white. Conversely, we can argue that the colours red and white, used in a ritual context, denote reproductive and nutritive physiological functions.

Other body fluids and emissions are associated with the colour black, or combinations of black with red or white. I list these associations.

semen/urine of sorcerers	red+black
menstrual blood	red+black
leprosy pus	white+black
venereal disease discharge	white+black
faeces	black

Since the colour black denotes the anal function, the colour combinations 'red+black' and 'white+black' denote an intermingling, or 'confusion', of reproductive/nutritive and anal physiological functions. Furthermore, the fluids and emissions associated with the 'red+black' and 'white+black' colour combinations are themselves associated, by Ndembu, with infertility or sterility. The menstruating woman is regarded as temporarily infertile, therefore she is secluded in a hut on the outside of the village hut circle and prohibited from entering the village or cassava gardens, so that the fertile/infertile conceptual opposition is maintained spatially. Sorcerers are regarded as sterile, (Turner: 1953: 15). Leprosy is associated with the makishi dancers, who represent male sorcerers in ritual, and the secluded male ritual subject, also regarded as 'infertile', since the contracting of leprosy is believed to result from 'fertile' social categories approaching these two 'infertile' supernatural and social categories. Venereal disease inhibits procreation.

Hence, the 'infertile' conceptual category, symbolic representations of which are 'cast out' of the village by means of ritual action, is associated with the confusion of anal and reproductive/nutritive physiological functions and is negatively evaluated. On the other hand, the 'fertile' conceptual category is associated with reproductive and nutritive physiological functions and is positively evaluated. This leaves us with a conceptual category, associated with the anal function, which is neutrally evaluated and represented, in ritual, by means of 'black' symbols.

The only usage of 'black' in Ndembu rituals in isolation from other colours is in the Nkang'a ritual after the bride and groom have slept with one another for the first time. The following morning, pieces of black malowa (river mud) are placed in front of the entrances of every hut in the village (Turner, 1968, 260). Hence, indigenous thought is here structuring the area of experience of the relationship between bride and groom by means of the 'defecation' area of experience. I suggest that the property of the latter area of experience which is perceptually manifest is the privacy surrounding defecation. Bride and groom, until this first act of

intercourse, have had a relationship which has existed only in a public context, so that the use of the 'defecation' analogue at this point in their relationship stresses the new private behavioural context.

We can therefore compare the 'private' behavioural context which is, I suggest, denoted by the use of 'black' symbols with the 'public' context of distribution and consumption of food (Turner: 1957: 31-2). It can now be argued that the use of red and white symbols denotes a 'public' behavioural context, which is positively evaluated, and that the use of black symbols denotes a 'private' behavioural context, which is neutrally evaluated. The negatively evaluated conceptual category of the 'infertile' can therefore be interpreted as the confusion of the two behavioural contexts.

Significantly, the majority of the crisis situations within the Ndembu village recorded by Turner which were redressed by means of ritual performance had their origins in quarrels over the distribution of meat. Such quarrels originate from the situation of the hunter making preferential distributions of meat in private to his close kin whilst custom stresses the public nature of distribution of meat, so that private and public behavioural contexts are here confused; or they originate from the hunter consuming his 'kill' in the bush and claiming bad luck in hunting on his return to the village, whilst custom stresses the public nature of consumption of meat, another confusion of private and public behavioural contexts (Turner: 1957: 31-2).

An examination of symbolization in Ndembu rituals reveals that symbolic objects associated with the colour combinations 'red+black' or 'white+black' are 'cast out' of the social group during the ritual performance. We can therefore interpret these ritual actions as the symbolic removal of the confusions of private and public behavioural contexts which constitute the indigenous paradigm of the crisis situation. Furthermore, since the ritual subject is also 'cast out' of the group, designated 'infertile' and associated with 'white+black' or 'red+black' colour combinations, we can argue that the plurality of behavioural confusions commited by more than one member of the group, therefore 'diffused' throughout the group, are 'focused' or 'projected' on to the ritual subject. The unitary location of the behavioural confusions within the ritual subject and 'casting out' of the ritual subject from the group therefore convinces the ritual assembly that the group has been 'purged' of the behavioural confusions which constitute the indigenous representation of the crisis situation, so that the crisis situation is redressed.

In terms of this interpretation, the pathological condition of the ritual subject is also indigenously represented in terms of the confusion of behavioural contexts. Hence, the curing of the patient takes the form of the spatial and temporal separation of the two behavioural contexts within the ritual: the ritual subject is first secluded in a menstruation hut (placed in a 'private' context) then 'brought out' for a communion meal with his/her matrikin (placed in a 'public' context).

Having demonstrated how my own interpretation of the role of foods, body fluids and emissions in ritual can be developed into a satisfactory interpretation of Ndembu ritual action, I shall conclude this article by clarifying this interpretation. By means of associated colours, Ndembu relate a classification of the functions of the human organism to a classification of behaviour and make a common evaluation of the classes of physiological function and behaviour in the following way:

EVALUATION	HUMAN ORGANISM	BEHAVIOUR
Positive	Reproductive and nutritive functions	Appropriate to the PUBLIC context
Neutral	Anal function	Appropriate to the PRIVATE context
Negative	Confusion of Anal and Reproductive/Nutritive functions	Confusion of PUBLIC and PRIVATE contexts

The behavioural area of experience is therefore classified by means of a perceptual framework derived from the 'organic' area of experience. Thus, the distinction between reproductive/nutritive and anal functions is perceptually manifest, but the distinction between behaviour appropriate to the public and private contexts is a purely conceptual one. Hence, the behavioural distinction is maintained by associating the confusion of the two types of behaviour with the confusion of the physiological functions, so that the revulsion commonly associated with the latter is transferred on to the former. The reader can now compare this interpretation of the role of foods and body fluids and emissions in Ndembu ritual with that of Turner and assess for himself which interpretation better explains their usage.

Gordon Geekie.

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JOHN MILLAR (1735-1801)

One of the most interesting and rewarding of sociological writers, though one seldom mentioned, and I think even more seldom read, is John Millar, a pupil of Adam Smith and from 1761 Professor of Law in the University of Glasgow. He was a man of liberal mind, what would, I suppose, today be called left-wing. His best known book, and the one which is of importance to us anthropologists, if we are concerned with the history of our thought, is The Origin of the Distinction of Ranks or an Inquiry into the Circumstances which give rise to Influence and Authority in the different Members of Society (1771; 4th Edit. with an introduction by John Craig, Edinburgh, 1806), a book which shows the strong influence of Montesquieu and also of Lord Kames and Adam Smith: the last two and Hume being Millar's friends. The idea of social progress, the child of the Enlightenment, was very much in Millar's mind, and not inappropriately. He aimed, as we all say we try to do, at separating what is general to mankind from what in particular societies is due to particular circumstances. By comparing different societies he sought 'to deduce the causes of different laws, customs, and institutions which, previously, had been remarked merely as isolated and uninstructive facts.' (p. XXV). In reconstructing the earlier stages of development from savagery to civilization he used what Mr. Stewart in his Life of Dr. Smith (p.35) called theoretical or conjectural history. Using this comparative method he classified human societies into four types or states: hunters and fishers, pastoralists, agriculturalists, and those engaged in commerce. There was nothing very original in this classification. All writers about social progress of his time had much the same; and it goes back, without the idea of progress, it is true, to Aristotle. Millar did not, however, as some did, suppose that every society of necessity passes through all these stages. He adopted the division as the most convenient for his purposes, which were to bring out the most significant changes which have led to civilization; and to define these as general rules or principles in the light of which particular forms of institutions can be seen to be illustrations of the principles. Deviations from them are to be regarded as due to special and peculiar circumstances. In the course of his study he used such information as was available about what he regarded as simpler peoples: North American Indians, Hottentots, West African Negroes, Tatars, Arabs, the ancient Germans, Greeks, Italians, etc.

Differences of rank and power are everywhere due to sex and age, and also to the need for leadership. But particular systems of law and government have been affected by all sorts of conditions: the fertility of the soil, the nature of its productions, the size of the community, their cultural development, communications, etc. But in spite of these differences the similarity of man's wants and of his faculties has everywhere produced a remarkable uniformity in the several steps of his progression. 'There is thus, in human society, a natural progress from ignorance to knowledge, and from rude to civilized manners, the several stages of which are usually accompanied with peculiar laws and customs. Various accidental causes, indeed, have contributed to accelerate or retard this advancement in different countries.' (p.4).

Among primitive peoples women are treated harshly, little better than slaves, and sexual congress is scarcely more than animal mating. However, in matrilocal and matrilineal societies they have a much higher position (so he thought), e.g. the Lycians, the ancient inhabitants of Attica, some of the North American Indians, and the Indians of the Malabar coast; also where polyandry is practised, e.g. in parts of the Median empire, on the coast of Malabar, and in some of the Iroquois cantons. Woman's condition improves when more attention is paid to the pleasures of sex and where her economic role is more important and valued. This supposedly took place in the pastoral ages. In general it can be said that the domestication of cattle gave rise to a permanent distinction of ranks, some people becoming

richer than others and passing on their possessions to their descendants. The influence and power these people obtained was thus passed to their heirs, so that the distinction of ranks was permanent. Woman's position was again advanced in the agricultural stage, which also gave rise to property in land and hence to an even greater disproportion between the fortune and rank of individuals. Finally, changes in woman's condition arose from the improvement of useful arts and manufactures. These improvements led to a wider society and one in which there was greater intercommunication. Women ceased to be restricted in their activities to the family and home, and they mixed in outside society. 'In this situation, the women became, neither the slaves, nor the idols of the other sex, but the friends and companions.' (p.89). They were now valued for their useful talents and accomplishments; and, with the increase in wealth, women of condition were admired for their agreeable qualities and for the amusement their conversation affords. (He notes that no writer of the Augustan age left a work of imagination in which love is supposed to be productive of any tragical, or very serious effects.) The progress of women is thus part of the general history of society. The book is a great polemic, and a worthy one, in woman's cause.

Age is very important in primitive societies. Children are entirely dependent on their fathers; and also old men are always respected and have authority. 'So inseparately connected are age and authority in early periods, that in the language of rude nations the same word which signifies an old man is generally employed to denote a ruler or magistrate.' (p.114). When families begin to unite in a larger society the father loses some of his authority to representatives of the whole society; and when there are commerce and manufactures the children are no longer to the same extent dependent on him. The family becomes dispersed, the children leaving it to receive instruction and settling afterwards where there is employment. Thus they are emancipated from parental authority. However, when there is polygamy the authority of the head of the family lasts. Children are so numerous that parental affection is lessened; and the dissention among the wives requires a firm hand.

Millar then discusses the authority of a chief over the members of a tribe or village. This arises because tribes are almost continually at war with one another and feel the need for a military leader. This leader is given the respect once given to the father. In the hunting and fishing stage the leader is chosen simply for superior strength, courage, and other personal accomplishments. But in the pastoral stage the influence of a leader depends also on his greater wealth, which makes others dependent on him. 'The authority derived from wealth, is not only greater than that which arises from mere personal accomplishments, but also more stable and permanent. Extraordinary endowments, either of mind or body, can operate only during the life of the possessor, and are seldom continued for any length of time in the same family. But a man usually transmits his fortune to his posterity, and along with it all the means of creating dependence which he enjoyed. Thus the son, who inherits the estate of his father, is enabled to maintain an equal rank, at the same time that he preserves all the influence acquired by the former proprietor, which is daily augmented by the power of habit, and becomes more considerable as it passes from one generation to another'. (p.152). Hence the intense interest pastoral peoples have in their genealogies. Authority is further enhanced in a society with agriculture. The chief, with his superior wealth in cattle and his numerous retainers, acquires a much larger estate than anybody else; and his retainers are increased and, since they live on his land, are still more dependent on him. Also estates are less likely to be destroyed or impaired by accidents than are flocks and herds, 'so that the authority which is founded upon it becomes more permanent, and is apt to receive a continued accumulation of strength by remaining for ages in the same family.' (p.160). The chief is first a military leader; then he begins to exert his authority in other ways, including jurisdiction in both civil and criminal cases. Then he gets a sacred character - for

example, it is said that he is descended from the sun - and he assumes priestly functions, or controls them. Finally, he takes on legislative functions.

Millar has discussed the powers of husband, father, and civil magistrate. He concludes by a discussion of the relation between masters and servants or slaves. He notes that there are but few slaves among the greater part of the savages of America and says that the reason for this is that they have no opportunity of accumulating wealth and cannot therefore maintain servants. Therefore, also, they kill their prisoners. The Tartars, on the other hand, have great flocks and herds and support a number of domestics. Hence they treat their prisoners with moderation. Slavery in the end disappears because in a technically well developed country little profit can be drawn from the labour of a slave who is not trained to manufacture. It is more profitable to pay wages than to maintain slaves.

Millar's book contains some ethnographical and historical errors, perhaps unavoidable at the time he wrote it, but it is in many respects nearer to a modern sociological treatise than any other 18th century book; and I have always been grateful to G.D.H. Cole for bringing it to my notice. We find the same (as in Montesquieu and Ferguson) insistence that in any systematic scholarship one has to separate the general from the particular and whilst accounting for the general by some theoretical formulae (principles or laws) which explain it, at the same time to account for the variations or irregularities by reference to variable circumstances (as we have to do e.g. with the laws of projectiles or falling bodies). One finds also in Millar's book that what chiefly interested him was a study of the development of institutions (progress), a study which for the earlier stages had to be carried out with the aid of what has often been called the comparative method, a method which gives us a schematic typology (stages), each stage having its special features by which it is defined. When he discusses the factors leading to changes in the status of women and of children and of social leaders he never appeals only, or even much at all, to psychology or philosophy but to other social facts. His explanations are sociological, especially economic, e.g. chiefs arise through war; property enables aristocracies and dynasties to persist; prisoners are treated well or otherwise according to their economic value; slaves are maintained only in societies where they produce more than the cost of maintaining them.

This might well be a treatise in modern sociology (elimination of incidents, perturbations, special and peculiar circumstances, and elements, persons, etc.): mass movements, great historical trends, progress in all its 18th century sense. Then his typology of societies, which runs right through our literature - mode of livelihood, economic for those who like the word. Then in relation to this classification he makes an analysis of rank, showing the causes and conditions of prestige and power and character in each type of socio-economic community. On the whole it is a sober assessment, not didactic or dogmatic: a clear and consistent inquiry with the limited aim of discovering the origin and development of class structure. At the time it was written this was, in my opinion, a remarkable achievement.

E.E. Evans-Pritchard.

THE USE OF ETHNOGRAPHY

'For the present, it is preferred that the main emphasis should be on analytical discussion rather than on description or ethnography'. Editorial Note.

* * * * *

I want to discuss the use of ethnography in two senses, i) as a source for analysis and for illustration in 'analytical discussion', and ii) as an activity in its own right, a description which attempts to say what people are like. The validity of the first procedure is dependent on the validity of the second.

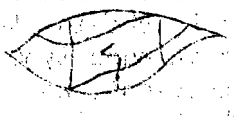
If I say that, to the anthropologist, 'theory' and 'facts' have never been mutually independent, and the writing of ethnography has necessarily been an exercise in analysis, I repeat the obvious. If I go on to talk of the shift from functionalist assumptions to transactionalism or the analysis of symbolic communication, I move into language which has a well-worn look. But, although the debates which sprang from say 'Rethinking Anthropology' may have run their course, that does not mean that the issues therein raised have been satisfactorily dealt with. The implications of the notion of 'social structure', for instance, need to be understood: is it possible to create ethnographic reality without some such notion ?

For the uses of the idea of social structure, let us go back to Radcliffe-Brown, who in 1940 mentioned

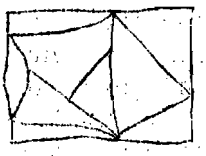
> 'a difficulty which I do not think that sociologists have really faced, the difficulty of defining what is meant by the term 'a society' ....
> If we say that our subject is the study and comparison of human societies, we ought to be able to say what are the unit entities with which we are concerned.
> If we take any convenient locality of a suitable size, we can study the structural system as it appears in and from that region, i.e. the network of relations connecting the inhabitants amongst themselves and with the people of other regions. We can thus observe, describe, and compare the systems of social structure of as many localities as we wish'. (1952:193)

This procedure can be demonstrated by the following diagram:

1. aerial view of 'convenient locality' (unstructured)



2. anthropologist's eye view (structuring)



3. anthropologist's model (structured)

We know that perception is active, not passive. Judging by Radcliffe-Brown, it looks as if the reason why sociologists have not really asked the question 'what is a society' is that they have necessarily created a society out of each set of observations. The visiting anthropologist, rather more at the mercy of the forces of nature and anomie than the surrounding primitives, has to make sense of what he sees, to structure it into manageable bounds. He tries to get some power

over this threatening Outside by naming it (the Bongo-Bongo, Kachin, LoWiili etc.)

James Thurber could never use a microscope - when at last he managed to see something and draw it, it turned out to be his own eye.... But, of course, what a participant observer records is the outcome of his interaction with the Outside, the Other which is very much there and with which he is trying to cope every day. The resultant ethnography is something else again - an attempt at an 'objective' view of how the system really works. If the language of 'social structure' etc. is used it is a misnomer to call this second process abstraction, for it is really reification or re-incarnation. Hence the difficulty of getting through 'structure', a defence system of concrete pillar boxes, to any life there may be behind.

In this view of ethnography, la pensée sauvage is shown to be universal. Anthropologists see structure because they cannot do anything else, and they can only translate what they see into concrete language: people must be characterised as part of a larger entity, equally an incarnation, called society. The existence of 'a society' is a given, it is not problematic; the questions asked, have, in the past, turned on the circumstances of its existence.

The anthropologist may be able to justify his structure as coinciding with a structure recognised by the inhabitants. I take an example from West African ethnography (since it was an examination of this which set me off on this essay). Nadel explained, in A Black Byzantium, why he thought that a Nupe society existed. He examined the processes of Nupeization and the ways in which a Nupe identity was promoted and acknowledged. 'The Nupe' are thus made credible, and we are as well told at what levels this identity exists, or is in abeyance in respect of other identities. Goody, on the other hand, attempted to differentiate an apparently amorphous mass of people, compared with the inhabitants of the Nupe kingdom. He traced the concomitants of two choices of inheritance regulation, and reified the resultant principles into two 'societies': 'The LoWiili' and 'the LoDagaba'. It is a pity that Leach was tempted to be frivolous about the organization of Goody's fieldnotes: people have argued about the insult instead of following up Leach's criticism that these are not 'societies'.

Whatever a society is, it is not presumably going to be defined in any simple or regular way as the sum of a set of isomorphic elements - social structure, political system, ritual intensity or whatever. Such assumptions have inhibited the comparison of political organization and the understanding of complex societies. Societies are not particoloured beach balls, differing only in size. Yet I wonder if the assumptions entailed in much of the use of ethnography are not simplistic in this way. Ethnographic illustrations, referring to 'the Tallensi' or 'the Azande' often seem to me to assume these isomorphisms. Mary Douglas' analysis of grid and group relies on the existence of societies as givens, identified by their names, and classified by the nature of their 'social structure' in concomitant variation with other variables. Indeed, the aim is to prove that the concomitances are mutually determining. It is not therefore the users of earlier, functionalist, ethnography only who may be tempted into assuming the existence of these relationships. The less interested anthropologists are in 'social structure' the more possible - one might say - that it becomes assumed by default. Any analysis is derived from a universe: the tendency is for this to acquire a socially bounded reality from its very selection by an anthropologist. Hence the value of those studies which are attempts to understand boundary making and maintenance at different levels.

Whatever the nature of ethnographic pre-structuring, the source of the anthropologist's generalisations has been a specific human experience. Yet we know that it is usually difficult to get even the feel of the actuality of the people observed, of the thinginess of things, from ethnographic accounts. Since the anthropologist was inevitably the mediator of the life which he translates into the language of his readers, his personal evaluation of it is surely a proper part of the ethnography. Where such an account is made, (usually as a 'popular' piece of autobiography) I believe it enriches the 'academic' presentation. Examples are the dual studies of pygmy life by Turnbull and of the Akwe-Shavante by Maybury-Lewis. I have suggested that we still need to ask what is a society; why not also consider what is ethnography ?

Elizabeth Tonkin.

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PROBLEMS OF PARADIGM DISCRIMINATION.

It is fairly clear that as soon as the attempt is made to elucidate the truth-grounds, or the rational grounds, for discrimination between paradigms then there arise a set of intractable, and to some extent imponderable problems. In the case of religious and non-religious paradigms one can, of course, deny outright that there are any paradigm-independent criteria of rationality which would enable paradigm discrimination to take place. The advocates of this view contend that criteria of rationality are paradigm-relative and hence there are not available to us any criteria of rationality which would enable us to judge between paradigms, and this is to rule as non-rational those processes of paradigm-discrimination and paradigm-displacement which, in fact, can be observed to take place. Nevertheless, in spite of what appear to be these obvious defects, such a view is implied in a Wittengensteinian "form of life" argument with its insistence that the criteria of assessment are intrinsic to the "form of life". And we find in the work of Peter Winch the implications of a "form of life" argument developed systematically and in some detail. One important implication of such a view is, of course, that the sociologist or social anthropologist is prohibited from making critical judgments about the beliefs he studies. But it is not only in the fields of linguistic philosophy and sociology that we find such a view for if we turn to the work of Thomas Kuhn in the philosophy of science we find the claim that we cannot have truth-grounds for theory-choice. We can find elements of the same thesis in the work of Whorf on language and, with certain provisions, in Mannheim's work in the sociology of knowledge. In one form or another therefore the claim that criteria of truth and rationality are paradigm-relative is widespread.

If paradigm-choice is ultimately shown to be a relative, arbitrary and somewhat non-rational affair then the modern purveyor of paradigms may well find that he is faced with a market situation in which the final and only remark he can make to his potential consumers is, as Aldous Huxley once put it, "You pays your money you takes your choice". However if this fate is to be avoided then one would need to show that there are paradigm-independent criteria of rationality which simply are the criteria of rationality: the existence of such criteria being a prerequisite of paradigm-discrimination. By making this move one could avoid the charges of "extreme relativism" or "irrationality" though one might, nevertheless, admit that there was a certain element of provisionality about the criteria one arrived at. But provisionality is not relativism. It is of some importance, however, that we are able to specify in some way the criteria of rationality which it is hoped can be provisionally accepted. Unfortunately, it is considerably easier to specify what will not do than what will do, and it is certainly easier to show that there is a process of rational discourse, which embodies appropriate and acceptable criteria, ranging across various disciplines than it is to show that there is a process of rational discourse, embodying appropriate and acceptable criteria, ranging across Weltanschauungen.

Initially let us see what will not do as provisionally acceptable criteria of rationality. This can be accomplished, somewhat indirectly, by considering the case of paradigm-choice between religious and non-religious paradigms. Both these categories of paradigms have been charged with possessing the feature of logical invulnerability and to be found in possession of this feature is to be found guilty of a serious rational defect - one might even say, if we accept this criticism, that any paradigm coming under either of these categories is ipso facto irredemably defective. One answer to this charge runs as follows:

to construe the alleged feature as a feature of logical invulnerability and hence as a rational defect not only involves a misconstruction, but it is a misconstruction which results from applying an inappropriate and restrictive standard of rationality. In short, what is required is a non-falsificationist thesis of rationality which will do justice to a wide range of intellectual and creative activities which may properly be called rational activities but whose procedures cannot adequately be characterised in verificationist/falsificationist terms. It thus seems plausible to contend that there is a scale of rationality ranging over such things as the choice of scientific theories, the nature of philosophical agreement and disagreement, critical exegesis, historical judgment and so on. There is, or so it seems, an "overlap" between the criteria of rationality employed by different disciplines: literary criticism is no less rational an activity than sub-atomic physics. But note: the comparison so far is between different disciplines not between different world-views. The sheer scope, range, and practical and moral import of Weltanshauungen make it substantially more difficult to conceive of what criteria of rationality could usefully be employed to discriminate between them. Clearly we can talk of good and bad science, good and bad philosophy, good and bad literary criticism but can we in the same sense so readily talk of good and bad world-views ? Perhaps, one might find parallels between the processess of rational discourse at work in relatively restricted areas like literary criticism and the processes of rational discourse at work in say Theravada Buddhism. What does seem more likely is that one will find parallels, of the required kind, between the literary critic qua literary critic and the anthropologist qua anthropologist. That this is more likely stems from the fact that there is some large measure of agreement amongst the respective practitioners about what constitutes good literary criticism and what constitutes good social anthropology and, one might add, there is a large measure of agreement, even amongst non-practitioners, about the relevance and importance of literary criticism and social anthropology. In both, it could probably be shown that the rational procedures of the literary critic and the social anthropologist do not presuppose, nor could be rendered in terms of, tightly knit decision procedures or a set of inductive or deductive procedural rules. Consequently, if we are to consider such activities as literary criticism and social anthropology as rule-governed and rational activities our notion of a "rule" has to be sufficiently broad to account for what actually goes on in these disciplines. At least one rationally acceptable precept is that in a serious study of a given phenomenon the techniques of investigation, and the kind of explanation or assessment which may be forthcoming, should be conceptually appropriate to the phenomenon under investigation. Such a methodological precept allows for the possibility that the investigation of a specified phenomenon may commence without having laid down in advance, as it were, tight decision procedures, for the nature of the phenomenon under investigation may require the investigator to make relevant judgments which cannot be rendered in such terms but, nevertheless the procedures may well be rational and rule-governed. Consequently, one may discern some overlap in the rational procedures of the literary critic and the social anthropologist: they may both be said to satisfy the logical and evidential senses of the term "rational". And even though their procedures are not cast in the falsificationist mould they are nonetheless rational. But then again the anthropologist and the literary critic are not in competition, but, in some central sense the Marxist, the Christian and the Buddhist are.

However, it is also the case that there is a "critical lack of fit", or an element of incommensurability, between paradigms and it is of some importance to notice that an overlap in their respective

criteria of rationality is a necessary precondition for talking about their incommensurability. We need a reasonable and sympathetic working knowledge of Buddhist doctrines in order to recognize that there is a "critical lack of fit" between them and say the Marxist Manifesto. In order to do this we need to be able to translate the Pali canon. In so far as we are committed to the view that we can, in fact, translate these canonical Buddhist texts, we are committed also to the view that there is some overlap in the rationality of the Theravada Buddhist and the translator which makes this possible. The question may arise, therefore, whether there is a suitable analogue to the notion of a "critical lack of fit" in say the disagreements between literary critics about the interpretation of a play or in the disagreements between philosophers or in the disagreements between anthropologists ? But even if we could detect suitable analogues would this not mean that we would have to say, as indeed we could argue for in the case of religious paradigms that such disagreements result from different paradigms employing different antecedent presuppositions. In the case of religious paradigms one might say that the critical lack of fit between paradigms is not necessarily a result of any rational defect but that it results from the fact that they carve the world up in different ways. For example, the Buddhist and the Christian paradigms seem, on the face of it, to be making competing claims about "what is the case". But on closer inspection we find that they constitute the world in such radically different ways that it becomes questionable whether they are dealing with the same phenomena, and, of course, if they are not dealing with the same phenomena, then in what sense can it be claimed that they are competing ?

This dilemma is not a superficial one. Paradigm disputation equally resembles an argument about what the evidence is as it does an argument about the correct or most plausible interpretation of the evidence. That there is some common ground between paradigms in order to get the dispute going seems undeniable just as it seems undeniable that there must be some implicit overlap in criteria of rationality if the translation of the Pali canon is to be accomplished. Difficulties arise in relation to the former because once the alleged paradigm-independent evidence is incorporated into a specified paradigm then its nature - and not only its significance - undergoes change. And this is not intended to be "over-charitable" to the rationality of paradigms. But it is to say that in a certain sense two paradigms could fulfil both the evidential and logical criteria of rationality and yet a dispute arise between them. Earlier it was suggested that the kind of assessment or explanation which may be given of a phenomenon must be conceptually appropriate and, of course, what is considered to be conceptually appropriate will depend on what one considers to be the nature of the phenomenon. It is precisely at this point that the disputes arise: some types and kinds of explanation will be ruled out of court or considered redundant. The Marxist will not concede that suffering, evil and death are in need of the kind of explanation given by the Christian or the Buddhist. How then is it possible to judge the plausibility of a paradigm's antecedent presuppositions ? One answer is that the only way is to work through the paradigm, as it were. But clearly if this is the only possibility then not only are paradigms world-constituting they are also self-verifying. It is also the case, however, that to do justice to the plausibility of a particular set of antecedent presuppositions, there is an initial requirement to give a phenomenological account, bracketing questions of truth, and elucidating the standards of intelligibility and judgments operating within the paradigm. The objection is that this is all that we can legitimately accomplish, for we cannot make further logical or cognitive judgments without presupposing an equally problematical set of antecedent

presuppositions. On this account the demand for non-context dependent criteria of rationality which would serve as a provisionally acceptable critical standard is misconceived. What this amounts to saying is that the logical behaviour of such words as "rational", "explanation", "plausible", "evidence" and so on, in their non-nursery contexts, is such that they are related and relative to a set of antecedent presuppositions which, in turn, are given their mature expression in a specified paradigm.

But whatever substance there is in this kind of characterisation of the logical behaviour of such terms as "rational" it can hardly be said to be an exhaustive account for there are a number of eminently rational precepts (e.g. that the kind of assessment and explanation given of a phenomenon must be conceptually appropriate) in which the appeal to antecedent presuppositions seems redundant. And yet the force and implications of such precepts seem undeniable. For example, it is of crucial importance to realise that the kinds of explanation of physical phenomena given by the physical scientist are conceptually inappropriate for understanding social phenomena. Causal explanations, at least of the type given of physical phenomena, are inappropriate because, to put it crudely, we cannot attribute intentions to an electron - we cannot ask an electron what its reasons for behaving thus are - but we can, and must, ask agents for explanations of their behaviour. Naturally, this presupposes that people, as opposed to objects, are of such a nature that they are capable of having intentions and performing intentional acts. But then this presupposition is not in dispute by any religious or non-religious paradigm. It is not the kind of presupposition we have in mind when we talk about differences in antecedent presuppositions. Hence, it need not worry us unduly that a precept presupposes a common way of differentiating and interpreting our experience of the world.

One of the implications of these remarks is that there is a need for a non-falsificational theory of rationality. For example, Kuhn's notion of a paradigm, and its analogues in non-scientific contexts, complicates the problems involved in assessing the rationality of a particular paradigm or paradigm-category. On Kuhn's account we cannot, with any precision, lay down in advance what will verify or falsify a whole paradigm. Descriptively speaking, paradigms can be shown to tolerate, or accommodate, all kinds of "refutations" so that if a falsificationist thesis is to account for the nature of scientific progress it needs to be amended to incorporate degrees of falsification. Such an amendment seems contrary to the intentions of the falsificationist thesis. There remains the interesting possibility that the incommensurability of paradigms is not the result of any defect in their rationality, but, rather results from the fact that they constitute the world in different ways. This possibility warrants further investigation particularly in relation to paradigm analogues in the religious and non-religious categories. Two implications seem apparent: (a) it may be possible to have non-context dependent criteria of rationality which would not be restricted to the verificationist/falsificationist type, and (b) judgments between paradigms might rest on considerations of the plausibility of a paradigm's antecedent presuppositions rather than resting on whether or not a paradigm meets the canons of falsificationism. Let us call this the non-falsificationist thesis of rationality.

The term "rational" tends to be applied in two related, though distinct, senses: we talk of the rationality of belief and we talk of the rationality of action. Let us consider the rationality of belief. Beliefs can be characterised as irrational if they are illogical in some sense (e.g. they are inconsistent or contradictory) or if the beliefs

have been arrived at in some way that is thought to be unsatisfactory (e.g. they are based on irrelevant considerations or they are based on insufficient evidence or they are not held open to refutation). For example, religious beliefs are often taken to be irrational because, it is held, they are not open to disconfirming evidence. But both the logical and evidential criteria of rationality require reassessment if we apply a non-falsificationist thesis. To demonstrate the extent of such a reassessment let us consider some of the arguments used by MacIntyre in his "Is understanding religion compatible with believing?" (in Wilson (ed)).

MacIntyre raises the following question: how is it that what appears intelligible in one social context can appear not to make sense in another ? He cites Christianity as a case in point and he further notes that the internal incoherences in Christian concepts did not go unnoticed in the Middle Ages, but they were tolerated. They were tolerated, according to MacIntyre, because they were indispensable to the forms of description embodied in the prevailing social structure: the concepts derived their point from the prevailing social patterns of behaviour. The process of secularisation deprived the concepts of their point, and, hence, the incoherences were no longer tolerated or tolerable. MacIntyre's argument rests on two distinct claims: (a) that we first identify incoherences in a given paradigm and (b) that paradigm displacement is not the result of rational argument, but, results from changes in the structures of societies. The second claim seems sociologically correct but it tells us little, if it tells us anything, about the rationality of a given paradigm. It is only when we have first established that a given paradigm is rationally defective, either in the logical or evidential senses or both, that we can ask the Durkheimian question why do the adherents hold on to such beliefs in the face of such problems. Then sociological explanations, or the like, become the only ones available to us. Methodologically, there are two conditions which need to be fulfilled before one can legitimately be in a position to establish that a given paradigm is rationally defective: the first condition requires us initially to detect the standards of rationality, or intelligibility, operative within the specified paradigm; the second condition specifies that we necessarily invoke our own criteria of rationality as the final critical standard. The first condition is a prerequisite of sociological investigation and the second condition makes critical evaluation possible. There is little need to quarrel with these two conditions but so much depends on what we take to be our own standards of rationality: we need to be fairly clear as to what constitute the criteria of rationality which we claim we are invoking when we commence sociological investigation. Matters of verification and falsification do have application within religious and non-religious paradigms. They do constitute an important part of a paradigm's plausibility, but, they do not encompass the entire plausibility-potential of such paradigms. Neither should they. Paradigms appeal to a common strand of rationality which allows the individual adherent to employ his own judgment in interpreting and evaluating evidence and counter-evidence.

For example, the Christian paradigm does not demand of its practitioners that they should ignore arguments which may be levelled against the paradigm; neither does the Christian paradigm demand that all recalcitrant evidence be treated as only apparently recalcitrant. Nevertheless, paradigms do tend to lay down the general direction in which such judgments should operate, but, it is difficult to characterise this as a rational defect. The Christian tradition delineates the relationship between "love" and "suffering" in a substantially different way from the Buddhist tradition; the ways in

which such relationships are delineated are a function of the interpretative capacity of the paradigm. And this is partly - and only partly - independent of the paradigm's identification of what constitute cases of "loving" and "suffering". A Christian paradigm, for example, specifies that a believer's final judgment should be not to deny God's love in spite of counter-evidence. But does this amount to a rational defect ? What is responsible for it being called a rational defect is, in part, a misconstruction of how the evidential role operates within a paradigm and, in part, it is the result of a restrictive view of what constitute the criteria of rationality. The role of evidence in a religious paradigm does not operate on a one-track or linear basis; neither are religious beliefs in any useful sense characterised as provisional or tentative hypotheses. The evidence presented by a religious paradigm is not presented in terms of a sound deductive argument, that is, in terms of an argument whose premises are taken to be true and in which the truth of the conclusion follows logically from the truth of the premises. Indeed, the sheer scope and the profound practical import of religious and non-religious paradigms makes it a rather hopeless task to seek paradigm justification in these terms. Theodicies indicate that typically religious paradigms are concerned to elucidate the meaning of suffering, evil, death and so on. Religious paradigms present important judgments on, and explanations of, such matters, but, to insist that such judgments and explanations be rendered in terms of deductively sound arguments or hypotheses is to apply a restrictive and inappropriate standard. A Christian paradigm simply does not operate like a rule-book on hypothetico-deductive method.

It seems correct to suggest that in the case of non-scientific paradigms we cannot lay down in advance a set of rules governing the use of evidence; rather the rules operative within a given paradigm may only be discernible by examining the judgments made by its practitioners. In a sense what those practitioners judge it reasonable to infer constitutes what it is reasonable to infer. Different practitioners may arrive at different conclusions depending on what weight they attach to various elements within a paradigm. Judgments of this kind do not fit into a simple linear pattern, but this does not mean that they are, by virtue of this fact, to be considered as rationally defective. Even in the cases where the beliefs are in principle falsifiable, but in practice not, the problem about their rationality cannot simply end there. MacIntyre's argument in so far as it rests on an assumption concerning the linearity and provisional nature of religious claims is, therefore, inconclusive and somewhat wrongly directed.

But though a non-falsificationist thesis allows for a broader-based concept of rationality the notion of a paradigm complicates the issue still further. One might be prepared to argue that the disputes arising between non-scientific paradigms cannot be settled by an appeal to further evidence as the paradigms, as it were, aim to accommodate all the evidence that is presented to them. It then seems plausible to contend that the disputes arise because paradigm's interpret the same evidence differently. But it is not clear that this account is straightforwardly correct. A Buddhist paradigm would characterise what is allegedly independent evidence in such a radically different way to a Christian paradigm that it begins to look as if it is implausible to say that we are dealing with the same evidence. This point requires further clarification. Given, as Ninian Smart argues, that existential questions about religious entities are more like their counterparts in science (e.g. Do electrons exist ?) than like the simpler existential questions of the nursery then it seems a genuine possibility that we may run into similar problems to those which Kuhn alludes to in relation to

scientific paradigms. I think it can be shown that we do. For example if we take, say, the nursery sense of "suffering" then we can identify (independently of an appeal to a scientific, religious or non-religious explanation) cases of suffering. But, when we ask for an explanation of the causes of suffering we are inevitably lead back to paradigm-tied explanations. The types of answers which are forthcoming will depend on the type of suffering one has in mind. The answers proferred by religious paradigms tend to make us see suffering where we formerly believed it to be absent. Even if one assumes that religious and non-religious paradigms appeal to paradigm-independent evidence one can still discern that such evidence undergoes important changes once it is incorporated within a specified paradigm. The kind of changes which such evidence undergoes cannot be fully accounted for in terms of the ways in which paradigms weigh and interpret the various evidential strands. Theodicies are informative in this as they raise the general problem of the relationship between paradigm-independent and paradigm-constituted evidence. It may be the case that the arguments which are raised about the univocal, equivocal and analogical uses of language are instances of this more general problem.

Consider the following oversimplified example. The first two of the Four Noble Truths declared by the Buddha are (1) that all existence is sorrowful and (2) that the cause of sorrow is craving. Prima facie it seems that the first assertion can be taken independently of the second. That is, it seems as if we can first know, in a paradigm-independent manner, that all existence involves suffering and then we can look round for an explanation which is given, in part, by the second assertion. Of course, the explanation in terms of "craving" is only partially adequate as we cannot understand the full import of what is meant by "craving" until we understand the concept of "nirvana"; only when this is accomplished is the explanation deemed to be relatively complete - at least, from the Buddha's point of view. What is clear is that the all-pervasiveness of "suffering" is explained by the all-pervasiveness of "craving": "craving" is both a necessary and sufficient condition for the existence of "suffering". But the Buddhist doctrine of "Not-self" importantly determines the meaning of the terms "suffering" and "craving". In the ordinary sense we can identify x as a case of suffering independently of knowing that x was caused by y. However, in the Buddhist paradigm once we have come to understand that the cause of suffering is craving we have also come to understand that "craving" is a central feature of all human enterprise as we normally conceive it. The nursery sense of "suffering" has been extended to cover situations to which it does not usually apply. The person who is normally described as being more or less content with his life-style (e.g. has achieved his professional ambitions or has a good relationship with his wife and family) would, nevertheless, on the Buddhist schema, be under the intoxication of wordly influence: he would be "suffering" whether he knew it or not. The whole force of the Buddhist paradigm is to help the individual to see that he really is suffering - that is, its aim is to help him see suffering where he previously thought it to be absent. It is no answer to the Buddhist to insist that he has committed the error of generalising one side of a polar term, for the distinction between "suffering" and "non-suffering" does have application within the Buddhist paradigm. What it is important to note is that the Buddhist paradigm does not rest content with simply pointing to commonly agreed features of human life (that men sometimes suffer); rather, the basic concepts of Buddhism (e.g. tanha, dukkha, nirvana) central to the Buddhist explanation of the world constitute the world in such a way that the appeal to paradigm-independent evidence involves a reconstituting of what that evidence is. There is a conceptual link, as well as a causal relation, between "suffering" and "craving". The position equally resembles an argument about what the evidence is as much as it

resembles an argument about the correct interpretation of the evidence. Generally, theodices possess this twofold characteristic: they initially appeal to paradigm-independent evidence, but, once such evidence has been incorporated into the paradigm, its nature and not only its significance, undergoes change.

What can be fairly said at this point is that the phenomenon of alternation taken cumulatively into account with other salient features of a sociological perspective begins to constitute grounds for denying the plausibility of religious paradigms. The sociologist can offer us rational grounds for preferring his sociological paradigm. Let us briefly summarise some of the relevant sociological considerations: (a) the sociologist can offer us an account of why the practitioners of religious paradigms hold on to their beliefs in spite of the fundamental problems about the truth-valuability of such beliefs (e.g. Durkheim), (b) sociologists can highlight the unintended consequences of particular religious meaning-systems and reveal hitherto important and unnoticed characteristics of social structure not accounted for from within a given religious account of what the world is like (e.g. Weber), (c) the sociologist can show that the demand for an over-all interpretation of human experience (that is the demand for a Weltanschauung) is equally as great as, if not greater than, the commitment to give a true account of the world (this element can be seen clearly in the work of Berger), (d) the sociologists can point to a fundamental shift in the "inner-meaning" structures of religious paradigms (cf. Luckmann), (e) it can be shown that religious paradigms are not displaced by rational argument but rather cease to be relevant because of large scale changes in the structures of societies (cf. MacIntyre), (f) the sociology of knowledge can indicate that in the case of Weltenshauungen socio-historical circumstance largely determine what is taken at any given point as constituting a plausible over-all interpretation of the world (cf. Mannheim). If one takes these points cumulatively one can see that the general disenchantment with the plausibility of religious paradigms is the result of a variety of sociological endeavours. It is also interesting to note that whatever the force of this cumulative disenchantment there is no appeal to what has become the characteristic philosophical critique of religion, namely, the claim that religious beliefs are rationally defective because they are unfalsifiable. Even in the case of Durkheim's critique the concern is to give a coherent account of diverse and incompatible religious belief systems: he is not concerned to say that religious beliefs are irrational because they are unfalsifiable - what he is concerned to do is to say that such belief systems are inadequate characterisations of what really is the case; they are to be considered "false" only in this sense. One might therefore be inclined to wonder why sociologists have not directly assaulted religious claims in the way that some contemporary philosophers have done. The reason is, I think, not difficult to find. Sociologists would be disinclined to derive their model of rationality from the physical sciences. They would not want of course to claim that the physical sciences do not embody an acceptable standard of rationality, but, they would want to claim, as for example Mannheim does, that the physical science model is simply not suitable for the social sciences.

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'THE ODD PHILOSOPHER'

This is not the occasion to return to the details of the anthropology/philosophy issue, but Tennekes' mention of the 'odd philosopher' (1971:38) points to an important difference between the two books under review. Put bluntly, Tennekes does not think much of the philosophical perspective whereas MacIntyre, now Professor of the History of Ideas at Brandeis University, continually brings his earlier training to bear on the conceptual problems raised by the activities of social scientists.

According to Herskovits, the 'cultural relativism' thesis involves 'a far-reaching re-examination of pre-existing commitments, a very real struggle between the intellectual and emotional components in attitudes long accepted and convictions long held' (1958: 266). This pertains to the questions which are raised by juxtaposing Tennekes against MacIntyre: should our re-examination, our 'programme' in the paradigmatic sense of that word, involve 'philosophical' investigation ?

For present purposes, we can accept Winch's condensed formulation of the programme which relates a philosophical stance to the activity of 'empirically' examining social phenomena. He distinguishes between 'empirical enquiries which must wait upon experience for their solution' and the examination of how concepts work (1958: 16). Since it is taken to be the case that 'in discussing language philosophically we are in fact discussing what counts as belonging to the world' it is one of the jobs of philosophy to show that much, if not all empirical enquiry raises conceptual questions. If anthropologists accept this view, then it inevitably follows that they engage in 'philosophy'. Two things follow from this. First, anthropology of the Radcliffe-Brownian variety stands at a further remove from (linguistic) philosophy than does that of the Evans-Pritchard species. This is because the two varieties apply different types of concepts; linguistic (philosophical) examination of participant discourse is more directly associated with the 'anthropology of meaning' than it is to the 'anthropology of general scientific laws'. It is the difference between the anthropologist who concentrates on working through native categories and the one who treats sui-generis 'meaning' as but a step on the path of applying such scientific concepts as can facilitate the techniques of comparative functionalism. Further, the 'philosopher', especially if he takes a Winchian view as to the nature of social science, can (so to speak) help Evans-Pritchard, whereas his linguistic perspective will probably mean that his relations with Radcliffe-Brown (or Murdock) will be directed through critically destructive channels.

Such considerations are important because they point to the selective impact of (linguistic) philosophy in purely beneficial respects. Thus since Tennekes regards anthropology in some sort of Radcliffe-Brownian sense (ibid: 78), the role of the Winchian philosopher will be relegated to criticism. In other words, if Tennekes extends the component of 'empirical enquiries' (as defined above), then he is (from his own, albeit mistaken, point of view) quite entitled to cast out certain aspects of linguistic, conceptual, analysis.

My second introductory remark is of a more general order. It assumes that the impact of (linguistic) philosophy is selective, and asks, who should we call philosophers ? skirting the issue as to whether philosophy can make substantive as opposed to analytical contributions, it is common-sensical that any analytical examination of social phenomena must rest on a set of procedural and interpretative assumptions. So far as I can make out, Tennekes applies the word 'methodology' to cover this stock of ideas. He suggests, 'It is feasible...to remain as much as possible within the boundary-zone between philosophy and empirical science

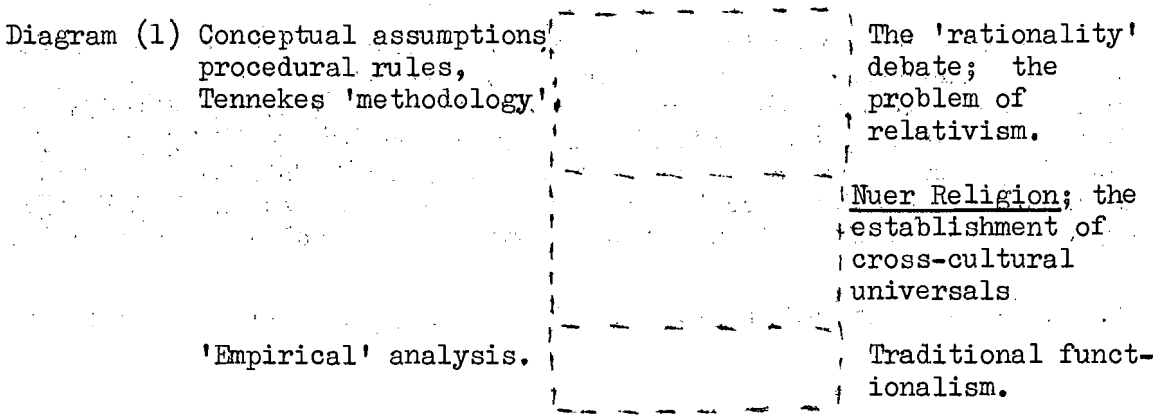
that is called methodology'. To my mind, this sort of assertion is absurd. It implies that philosophers are solely concerned with specifically philosophical questions and are attempting to develop a substantive body of knowledge by non-experimental (or 'empirical') methods which stand apart from 'methodology'. In fact, several scattered remarks show that this is indeed what Tennekes has in mind (see his references to Ortega y Gasset and Van Peursen). Herskovits also veers towards the 'master-scientist' position. What at least Tennekes does not realise is that much modern philosophy is not of this order, and that much is specifically designed to broach the type of conceptual difficulties which are particularly characteristic of the 'methodological' sector of social science.

It is completely futile to argue: I am an anthropologist, an 'empirical' investigator; it is not my job to examine conceptually my 'methodology'. For, and this is the whole point of my arguments, there are not philosophers and anthropologists. Instead, there are those who are lucky enough to have received a training which allows them to take a philosophical perspective, and there are those who, like Tennekes, retain their faith in the 'empirical' (see Winch ibid: 15-16). Look at the collection Rationality (1970) and try distinguishing philosophers from anthropologists on any other criteria than that of competence.

Before detailing a comparison of Tennekes and MacIntyre, it is useful to give some further indication of which anthropological problems are most susceptible to (linguistic) philosophical examination. Unless this point is cleared up, the defender of Tennekes could retort - "but given his problematic he has no need to turn to philosophy." We can imagine a hierarchical feedback system. Thus Nuer Religion can be examined, at the procedural level, from a conceptual vantage point (see Winch 1967). At the same time, no philosopher, with the possible exception of Gellner, would deem it necessary to make the actual 'empirical' examination of this aspect of Nuer life. Thus the practising 'empirical' anthropologist is hierarchically related to the philosophical standpoint. In the sense that no philosopher could argue about relativism without turning to a certain number of 'empirical' procedures and findings, the anthropologist is an integral component of his scheme. Conversely, the anthropologist cannot just go into the field and interpret. Hence Evans-Pritchard read Levy-Bruhl (a 'philosopher') before writing on Azande magic, and his knowledge of Catholic philosophy helped him analyse the Nuer's religion. But because 'empirical' examination cannot proceed without assuming a certain way or certain ways of looking at the world, and because the philosopher can always 'create' aspects of his arguments, the relationship is hierarchical.

We can now locate anthropological conceptual difficulties within this hierarchical scheme:

Diagram (1)



As one moves down the system, the upper layers become progressively less; the conceptual implications of such investigations become weaker. But this is not to say that they disappear: the diagram is supposed to show that conceptual and empirical investigations interpenetrate each other, are relative to one another. At the same time, the philosopher (especially the linguistic philosopher) has little to say about the lower levels: criticism is too easy. Conversely, we can see why most philosophers are today writing about the higher level topics. Langer writes, 'the concept of meaning...is the dominant philosophical concept of our time' (1962: 55). Thus recent shifts in anthropology have followed (?) modern philosophy, the result being such works as Rationality. What is more, the shift in anthropology has not been merely from function to meaning; it has also been from 'syntagmatic' to 'paradigmatic'. This latter does not relate to linguistic philosophy in quite the same way. Indeed, portions of A.S.A.10 very clearly show the tension which exists between the paradigmatic approach and such theories of meaning as have been developed by linguistic philosophers. Yet it is still possible to say that this central problematic - the question of how far paradigmatic styles of analysis should be extended - is being discussed in a 'philosophical' style. Ardener's 'The New Anthropology and its Critics' is as 'philosophical' as Winch's The Idea of a Social Science.

Tennekes and MacIntyre are, in their different fashions, addressing the relatively unformulated procedures associated with the question of cross-cultural intelligibility. This enterprise, to increase self-consciousness and critical coherence, is of vital import; as yet there does not exist a book in which the logic of this procedure is systematically portrayed and analysed.[1] There is no clear and logically complete exegesis of those difficulties consequent upon such notions as 'relativism', 'comparison', 'fideism', 'universals', 'evaluation', 'translation' and so on. The lacunae wait to be filled.

MacIntyre, as indicated by the title of his work, is interested in much more than understanding primitive societies. What he does have to say on this topic can be equated with several other articles (J.A.S.O. Vol I No.2 contains some references). This tradition is characterised by (generally) philosophers delving into the rubble-filled foundations of our discipline often to emerge with startling and logically plausible insights. Only rarely, and not at all in the case of MacIntyre, are these insights developed into logically complete systems. This means that it is not easy for the average anthropologist to 'read' their works. The same can be said of the relevant sections of the book under review: lacking an adequate handbook or 'map', the paradoxical situation emerges that the clarifications and arguments developed by MacIntyre act to increase some of our confusions. The lacunae are in a manner of speaking, aggravated; his narrow thrusts widen a field of thought which is already too complex for the typical anthropologist.

There is no reason for us to criticise MacIntyre for not providing us with a handbook. It would appear that this job best awaits an anthropologist, for without such a perspective the trained philosopher is in no real position to see what is, in an overall sense, required. Tennekes, I suspect, has taken on this enterprise. Unfortunately, as is so often the case, a second-rate scholar has stepped in to fill the gap. The result is that our expectations are not realised; his handbook

1. A. Hanson, who has written in this Journal, is working on such a book.

does next to nothing to fill the undoubted need. Even worse, by producing such a bad handbook, Tennekes is likely to mislead and discourage others. He has, if you like, smeared and distorted the lacunae. And unlike MacIntyre's work, his has the word 'anthropology' in the title.

Why does a reading of MacIntyre serve to expose the shallowness of Tennekes ? As indicated, everything, with the possible exception of mental endowment, hinges on their different attitudes to the philosophical perspective. Defining 'cultural relativism' as the thesis that 'all human action is profoundly determined by culture and hence bound by a given cultural situation' (op cit: I), he follows Herskovits (who in turn followed Siegel) and distinguishes three aspects: the methodological, the philosophical and the practical (ibid: 8). The second, which Herskovits describes as 'concern(ing) the nature of cultural values and, beyond this, the <u>implications</u> of an epistemology that derives from a recognition of the force of enculturative conditioning in shaping thought and behaviour' (1951: 24) is dismissed - 'I will limit myself to the (scientific) hypothesis (viz, as outlined above), for the philosophical thesis lies beyond the competence of empirical science' (op cit: 23). Thus Herskovits is described as 'pretentious' (ibid) for concentrating on the implications.

One difficulty is to understand what Tennekes means by 'philosophical'. On page 22 we find, philosophically speaking, 'cultural relativism implies that a judgment is considered valid when and in so far as it is culturally accepted', and on page 154 we read, 'the relativity <u>or</u> absolute validity of such value judgments'. Perhaps this is not of much significance, but the same cannot be said of the next point. That is to say, he does not remain faithful to his enunciated programme. At times he appears to be using the word 'methodological' in the Herskovitsian sense, when this procedure must be carried out before (if it is to be allowed) cross-cultural evaluations can be made. Thus, the, to complete the last quote, 'scientific determination of the universality of specific value judgments and value-standards as such say nothing as to the philosophical question of the relativity or absolute validity of such value judgments' (ibid). Yet we read, 'one can speak of value judgments with cross-cultural validity' (ibid).

Again, this in itself might not matter. It is true that Winch, in his discussion of moral universals, takes a philosophical perspective (see :1960), but in terms of his own system Tennekes is here regarding such universals as a factual component of social life. But at other points this excuse is less easy to apply. After denying any connection between the factual ('is') validity of value judgments and the philosophical 'ought' perspective (ibid) we find Tennekes concluding his work with the asseration that a) cognitive systems are necessarily evaluated by science, and b) 'in social sciences more specified and more controversial values play their part. These lead to valuations which, taken strictly, are not part of the scientific results, though they are not unrelated to them ' (ibid: 218).

We wonder what 'strictly' means: elsewhere he writes, 'facts are relevant for values and values for facts' (ibid: 210). More importantly, we cannot but wonder that Tennekes is not practising philosophy. Is he not discussing Herskovits 'the nature of cultural values'? Is he not arguing for some sort of 'philosophical' judgment ? Is he not playing around with the fact-value distinction ?

At least, Tennekes, whatever he thinks about the matter, is 'doing' philosophy; is working out logical implications. The trouble, one suspects, is that he takes such a narrow 'substantive' and absolutist view of philosophy. Philosophy has to do with the wrong sort of values. However, even with this conceptualisation, he has to admit that 'in the final analysis all 'relativism' is orientated (to philosophical relativism)', and that the relatively non-scientific values mentioned above 'figure importantly in empirical inquiry and automatically imply certain values' (ibid: 34,219). On a more reasonable scale, we might say that Tennekes is torn between an incorrect definition of philosophical relevance, and an implicit awareness that philosophy is essential. He is, of course, quite correct to suppose that 'empirical' conclusions as to the nature of cross-cultural unity (see his discussion of the 'biotic', 'psychic' and 'social' substrates) have much relevance for the conceptual examination of relativism and have a procedural significance, but our hierarchical model demonstrates that the true 'context of relevance' cannot be distinguished from 'philosophy'. 'Empiricism' can never be pure; resting upon assumptions which should be examined logically, this enterprise in turn provides more data for the 'philosophical' perspective which then generates new ways of looking at the facts.

Thus Tennekes does philosophy even as he denies it. Or at least, partially denies it, for at several points he has to admit the relevance (ibid: 39, 43, 58, 197). And at other suggestive moments his refusal to develop this orientation shows through like a sore thumb (ibid: Chpt.V especially p.191-204). In fact, many of the contradictions which weaken his argument would be resolved if he cast aside Herskovits' scheme to work instead with a more suitable framework. This is to say, his philosophy is poor. And it is not difficult to see why: 'I will largely limit myself to American cultural-anthropological statements... since it is especially in the United States that the case for cultural relativism has been presented by cultural anthropologists' (ibid: 2). This is factually incorrect, in that America is the context of the 'odd philosopher'. Since many British philosophers have discussed the problem, Tennekes summary blockade is of the order of a geographical Gluckman. Perhaps this is not being fair to Gluckman: regional naivety is even more vulgar than inter-disciplinary ignorance, and Tennekes combines the two. How can this possibly be the case for one who is probably primarily interested in the problem of evaluation ? (ibid: 145, 206). Surely Winch et al have something to offer ?

All this indicates that the anthropologist is not advised to read Anthropology, Relativism and Method except for one end. Shambling through a series of quotations, this 'foot-stool' scholar merely presents us with a reflection of current American anthropological thought on relativism. His classifications, we have seen, are as poor as theirs. At least, he tells us what work is being done. In this context, it is particularly interesting to realise that the group attending to cross-cultural universals do not, as summarised by Tennekes, realise the relevance of linguistic and kinship studies. Yet the former is precisely the field of which Ardener can say, 'the intuition that a total relativism is unproductive has been supported by the evidence from comparative study' (1971: xxi). It is indeed curious that just at the moment when anthropology is preparing itself systematically to relate the formal examination of universals to the Sapir-Whorf and 'context of situation' problematics, Tennekes should come along and blurr the impact - should scarcely even distinguish between 'structural' and 'functional' universals.

MacIntyre's conclusions cannot be mentioned in detail, but as a summary: a) 'What is at stake in these arguments (over the explanation of action ? Not only philosophical clarity, but also the question of the nature of the human sciences' (op cit: 204), b) human action can be explained in causal terms. c) 'The social scientist cannot evade the task of deciding what types of arguments and evidence are logically appropriate in different areas; he must be able to decide what constitutes the rationality of a scientific belief, or a moral belief, or a religious belief. But to do this is to do philosophy' (ibid: 259), d) it is valid to distinguish between irrational and rational modes of thought, e) the two types have to be explained differently, f) and such characterisations are obviously evaluative. g) Waismann is incorrect - there are 'expressions and criteria which transcend the divisions between his language strata' (ibid: 250). Additionally, MacIntyre has some most interesting things to say to the anthropologist interested in morality (see especially p.141), and his essay on comparative politics proves to be far more subtle than such remarks as are typically addressed to the comparative method.

MacIntyre shows most of us up. If it be the case that 'Happily or unhappily, the philosophers cannot be restricted merely to interpreting the social sciences, the point of their activity is to change them' (ibid: 259), then without an adequate background we are left as counters. For instance, the anthropology of religion is, in many respects, 'within' the rationality debate. So unless MacIntyre's remark, '(I can) find no reason to suppose that my investigations of Prichard's claims (he is a moral philosopher) and of the social background of these claims ought to differ radically from an anthropologically minded historians investigation of eighteenth-century Polynesia (i.e. the notion 'taboo')' is demonstrably wrong, we have no option but to widen the scope of our reading (ibid: 166).

Prima facie, to juxtapose Tennekes against MacIntyre bears a moral which should not be ignored. What should we do ? - manipulate the counters of bald and shallow assertions made in the past, or move on into more sophisticated domains ? Lead the reader into an unnecessary morass of details concerning the notion 'culture'[1] (does Tylor have to be quoted in the course of concluding 'man is not only determined culturally, but also biotically, psychically, and socially' ? (op cit: 105)), or get on with the job in an economic fashion ? Paradoxically, it is the economic MacIntyre who has to be read and re-read; Tennekes, unless one tries to sort out his confusions, makes light reading. But is this not to be expected ? Is it not MacIntyre who thinks, and Tennekes, at best, who recapitulates ?

Finally, I must admit that I am not at all sure that I have properly understood Anthropology, Relativism and Method. However, whereas MacIntyre can profitably be criticised, the tensions in Tennekes work between judgment/no judgment and anthropology/philosophy are such that we just do not know where we stand. Further, how much faith are we to have in a figure who can dogmatically distinguish between 'cultural' and 'social' anthropology then to assert, 'Culturology still is...in its infancy' (ibid: 49-50)? Or again, for someone who is prepared to make 'short shrift' of several certain issues, Tennekes is remarkably self-assured as he drifts from the free will problem to the nature of social science, to the nature of science...(ibid: 191).

In my opinion, the reader who can (perhaps) sort out Tennekes contentions might just as well think out the arguments for himself - or read MacIntyre and the rest.

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1. This is not to deny that the conceptual scheme as centred around the notion 'culture' is not of some relevance.

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BOOK REVIEWS:

The Translation of Culture. Essays to E.E. Evans-Pritchard. edited by T.O. Beidelman. £5.75p net. London, Tavistock Publications; New York, Barnes and Noble.

Beidelman is to be congratulated at having put together a fine set of essays in honour of Professor Evans-Pritchard. The articles are not always in a modern way related to the theme of cultural translation, but they cover a wide and interesting range of topics. There are purely ethnographical essays, like Needham's well chosen article on Penan Friendship-names; at the other extreme is a good article by Hallpike on cross-cultural comparison. The articles by Rivière, Fox and Hicks are basically ethnographic, but they are all extremely insightful analyses. Lévi-Strauss considers the myths and rites of two North American peoples and makes some interesting remarks on 'structure' and 'history'. Two contributors have attempted to rework some of Evans-Pritchard's field material. Beidelman's essay on Nuer priests and prophets is less interesting than his other splendid pieces on Nuer ethnography. That by Gough on Nuer kinship points out several omissions in the monographs, but one suspects some criticisms spring from a less than sympathetic reading. Her term 'paradigmatic' (p.92) is derived from the less than precise usage of Fortes and she does not grasp some of the Saussurean qualities of Evans-Pritchard's work. This volume also contains an excellent piece by Pitt-Rivers entitled 'On the Word Caste'. He is concerned with the properties a concept must possess to fit it for carrying an analytical load in anthropological investigation. It contains a lesson that many anthropologists have still to learn. The book also has a very valuable bibliography of Evans-Pritchard's writings. Altogether The Translation of Culture is a worthy tribute.

MALCOLM CRICK.

Gifts and Poison. F.G. Bailey (ed). £1.45p net. Oxford: Blackwell 1971.

This book is described as a 'micro-political' counterpart of the first volume in the Pavilion Series: Stratagems and Spoils by F.G. Bailey. There are thirteen papers, four by Professor Bailey himself and the rest by S. Hutson, J. Hudson, R. Layton, L. Blaxter, M.A. Heppenstall, P. Adams, N. Clodd, N.T. Colclough and R. Wade. The names of the writers do not appear on the papers themselves, and the reader is clearly invited to read the volume as a continuous whole. The striking thing is that this does work. The community of thought and style is such that the main body of contributors provide illustrations, from villages in France, Austria, Spain and two areas of Italy, of what is essentially one world view. It is rare for an Editor and his contributors (even where the latter are students of the former) to present such a unanimity of tone and approach. One can lose track of whose particular chapter one is reading. This has its disturbing aspects, but it is no doubt its own tribute to editorial and supervisory skill, while the tone and style are relatively straightforward and simple, thus making for readability.

Despite the clear stamp of 'moral community' on the book, it seems to me to be better than Stratagems and Spoils, its claimed exemplar. This is because of the clear difficulty that the contributors experience in using the quasi-games theory analogies of the earlier book. Somehow its 'one-upmanship' models do not satisfy in the actual village cases. The rather worn communication terms 'codes', 'messages', 'signals', and the like which the Editor now also uses seem to be applied even more metaphorically than is usual.

There is nevertheless a vein of integrity running through these papers. If Barth's mechanistic transactionalism is the 'highest stage of functionalism', one in which society is essentially a sum of all the individual pay-offs, Sussex transactionalism has passed into a zone tinged with a certain tristesse, even a reflective pessimism. Their sets of villagers are not the calculating teams of players in the game of 'micro-politics'. Some of them seem to be engaged in a nobler search for the preservation of some individuality, against the constricting pressures of envy and gossip. The Barthian transactions convey (precisely) poisons, as well as gifts.

The failure of the original Sussex transactionalism is thus signalled by a work which should have vindicated or exemplified it. The resulting theoretical void is filled by a partial return to a more Maussian view of exchange. The journey was not entirely in vain. The very unanimity of the intent and experience of the contributors (where all sought so keenly a snark, all to return instead with a boojum) accounts for that attractive integrity that I have remarked on, and which underlies the occasional confusions of the book as a whole.

At the moment the studies are poised unhappily between the 'paradigmatic' and the 'syntagmatic'. Having rejected transactional purity and an 'output' view of society, they do not have a clear apprehension of the best method to tackle the programmatic structures which will generate the meaning of their observations. (The most interesting passages occur where the authors sort out, for example - what it means to be jaloux or who exactly can be called signori). Once embarked on such tasks, the next step is to drop the terminology of 'transaction'. Only more confusion will result if it joins the ranks of mere dead metaphors which encumber the social sciences. In general the book does provide a kind of negative test of half-hearted games theory applications. In so doing it also points up a sometimes neglected aspect of Mauss's own theory of exchange. Some exchanges are fatal. That is one good reason why it is not a 'functionalist' theory, even of the highest stage !

EDWIN ARDENER.