

REVIEW ARTICLE

Modes of Thought, edited by Robin Horton and Ruth Finnegan.
Faber & Faber; £8.50. 1973.

Although this collection of papers is dedicated to Evans-Pritchard, the editors admit several of their contributors run 'counter to the spirit of his work'. These contributors, presumably Gellner, Barnes, Lukes, Horton and Wolfram, are clearly more interested in examining the interpretative schemata which necessarily belong to our own culture than in 'testing' this apparatus in the context of ethnographic material. This is unfortunate, simply because whatever the value of abstract analysis, there is nothing quite like that fruitful juxtaposition of ethnography and interpretative models to which Evans-Pritchard directed our attention. The danger of settling for abstract analysis of interpretative schemata is obvious: instead of seeking those ethnographic clues which might enable one to test their worth, or even modify, the models, our contributors almost inevitably lapse into dogmatism. Intellectualists, such as Horton, are so fascinated by their selection of scientific modes of thought as their interpretative schemata that they do not bother to begin with ethnography to see what that might tell them. So pleased with their discovery that interpretation has to be in terms of something, they concentrate on the 'something', not on interpretation itself. Their facts might suggest religion should be likened to science, but one wonders how readily this can be maintained in face of the complexities of primitive life.

Neither does it do our contributors much good to argue, in simplistic fashion, that anthropologists must analyse their own cultures modes of thought as closely as those of more alien forms of life. True, we have to study our own culture, but owing to the time lag which links anthropology with such sister disciplines as philosophy and theology, we find that much analysis has already been done for us. Evans-Pritchard did not sit back and write little pieces about the nature of western religion. Instead, he relied on the time lag, seeking one of his interpretative schemata in Rudolf Otto's The Idea of the Holy. It comes as no surprise to find that the most profitable contribution to Modes of Thought also involves an appeal to an established interpretative framework: Tambiah is able to suggest a new dimension of magic by rejecting the rather sterile oppositions inherited from the turn of the century, appealing instead to Austin's examination of speech acts.

Tambiah's article, 'Form and Meaning of Magical Acts: A Point of View', does not, however, quite live up to its promise. For despite his commendable references to ethnographic material, Tambiah is not as careful as he might have been in confronting magic with performatives. For instance, he does not show exactly how performatives of the type 'I do take this woman to be my lawful wedded wife' fall into the same category of events as magical acts of the type 'I cause you to die by sticking this pin into this image'. In the first case we can easily understand how a speech act can change the state of affairs existing in the world (for the change which occurs when one is married is essentially a conceptual one), but in the second example words are supposedly effecting a physical change which properly must be done by physical means. Tambiah could, perhaps, avoid this difficulty, but only at the expense of assuming that the participant does not really expect to kill his victim. The disadvantage of this is that it seems to deny the reality apparently attributed by many magicians to their acts, and that it raises the awkward question of what performatives have to do with analogy or the metaphor/literal distinction. Is Tambiah really justified in extending the notion of performatives from Austin's usage (where

we can understand how the world can be made to conform to words) to the context of magic where our problem has always been to understand the (apparent) belief that words and acts do more than we dare credit them with achieving? Tambiah obscures the oddity of magic, and clearly does not feel all that happy with his theory of performatives. Thus he has no qualms in describing those rituals which install chiefs as performatives. So far, so good, but such rituals are not necessarily magical. When he turns to magic proper, however, we find that all the long-standing tensions re-emerge: he seems to make an exception of those magical activities which are essentially designed to achieve practical results, and even with respect to more obviously analogical rites (especially those which aim at metaphorical transfer) there are signs that his performatives collapse into Beattian-like expressive utterances.

Even Tambiah, we begin to realise, is so enamoured of his interpretative schema that he seizes upon one such model to conclude that there must be one theory of magic. If he had limited himself to the more reasonable hypothesis that performatives help elucidate only those magical acts which seem to involve 'an operation done on an object-symbol to make an imperative and realistic transfer of its properties to the recipient', we might be less inclined to raise counter examples. If, that is to say, Tambiah had paid more attention to those ethnographic clues which might help us decide if the magician 'really' expects his rites to change the state of the empirical world, or whether he is merely making statements about his social or existential situation, he might have found it easier to locate the logic of performatives. He would also have found it much more difficult to avoid the conclusion that since performatives, properly speaking, do things to the world, magic cannot be interpreted symbolically (or analogically?) Yet if magic is read literally (or 'realistically' in Tambiah's language), such performatives are doing things which lie beyond the scope of Austin's usage.

The Gellner/Wolfram group of contributors are frequently clever, if not witty, but so far as I can see they add virtually nothing to the arguments which have already been bandied around in the rationality debate and other such contexts. As in Tambiah's article, the dominant theme is to specify, as exactly as possible, the differences or similarities between religion (sometimes magic) and science. Barnes, overstating his case in the process, argues that science is more like religion than has been commonly supposed. Horton and Lukes prefer to stress the verificationist principle, emphasise the scientific nature of science, and accordingly draw religion into science rather than the other way round. Whatever the emphasis, such comparisons all smack of Lévy-Bruhl: refusing to liken religion to anything but science, these contributors have no doubt been persuaded by the force of the argument that since science is our own supreme cognitive activity it must also be our supreme interpretative model. What is the use, they seem to imply, of comparing religion with religion?

It is at this point that Evans-Pritchard's spirit is really laid to rest. Maybe religion can be equated, to some extent or another, with science, but it first must be understood, and that requires prior phenomenological analysis where it does little good to commit the Lévy-Bruhlian fallacy. By insisting, from their own interpretative stance, that religion is a sub-species of science, the contributors in question have to commit all sorts of mental gymnastics with those ethnographic details they deign to discuss. By comparing religion and science they unavoidably find themselves emphasising the differences between these modes of 'thought', which is rather awkward when one's original intention is to prove the basic uniformity of all modes of thought. Hence their gymnastics: Barnes makes the

commitment of the scientist to his paradigm sound rather like the bond which ties a worshipper to his God, and Horton and Lukes, with their more traditional view of science, have to neglect entirely such religious aspects of religion as prayer, the possibility of their existing symbolic discourse, and, needless to add, worship itself.

In order to understand why these contributors feel obliged to equate science and religion, we might notice how they seem to ignore the difference between using the comparative method to establish contrasts, and using it to establish cross-cultural similarities. All seem to be agreed that the first step in the study of modes of thought is to apply some universal logical criteria with the power to expose contrasts between different types of belief systems. The favourite candidate, as Lukes cogently reminds us, is the verificationist principle: even Tambiah, who favours the incommensurability thesis (holding that magical acts cannot be judged by the same criteria as scientific findings), exposes the distinctive nature of magic partly by showing what magic is not. Only by applying the verificationist principle can one show what cannot be verified, thereby providing oneself with a certain amount of prima facie evidence that the phenomena in question is not meant to be verified. Barnes, we might add, favours another way of exposing contrast, namely the criterion of degree of anomaly present in any belief system. I wonder why the intellectualists do not take this up, because whereas the verificationist principle, for them, has the unpleasant effect of opposing science and religion, the anomaly criterion, in its supposedly Kuhnian guise, permits much closer identification.

No-one but the most die-hard Winchian would deny the role of such criteria in suggesting possibly significant contrasts between different ways of conceptualising the world. But Horton, Lukes and Barnes continue with the additional claim that one must also compare the substance of religion with the substance of science. Lukes spells out what is involved in this. He is not satisfied with using verificationist and other criteria to expose the uniqueness of religion; he also wants to claim that religious beliefs, odd as they might appear, are 'parasitic' upon those 'universal and fundamental' criteria with which we must begin. By this, Lukes seems to mean that the 'odd' beliefs must be assumed to belong to the same order of things as the beliefs in terms of which they are being judged. Hence the two sets of beliefs are fundamentally commensurable: science constitutes the reality of religion. Hence also the conclusion that religion is fundamentally in error, the job of the sociologist being to explain, in best nineteenth-century fashion, the origins and continued existence of the great illusion. To make another side reference to Barnes, it should be mentioned that he minimises this emphasis upon a sociology of error.

Lukes and Horton can have no idea of the different 'points of' or 'realities', possibly involved in magic and religion. They rule out those philosophers and theologians who insist that although religious discourse might ultimately be logically parasitical upon more orthodox forms of intelligibility (which after all, is the case of any metaphor or analogy), its meaning and reality 'takes off' to communicate relatively independently of verificationist criteria. Moral judgments, which so pervade most religions, are in error when judged against science, but who is to deny that they have a reality of their own which can be interpreted, to all intents and purposes, in its own right?

Lukes is even worse than the logical positivists: at least the latter allowed religion an autonomous existence, arguing that it should not be understood in the same way as scientific procedures. Lukes, on the other hand, feels that only by assuming the basically scientific nature of

religion can one avoid the 'temptation... of explain(ing) away false or inadequate attempts at explaining the world and reasoning about it as 'really' emotive, or expressive, or symbolic utterances, and thereby removed from the sphere of application of non-context-dependent criteria of truth and logic'. By his own argument, purely symbolic systems cannot exist; the domain of science is assured. So too is his type of sociology, a species which works with the curious logic of creating its own, frequently unnecessary, problems. For example, Lukes asks why the Azande do not perceive the futility of their magic. Part of his answer involves the idea that they build up 'secondary elaborations' to protect their beliefs 'against predictive failure and falsification'. This might be a correct interpretation, but what if we follow Tambiah and say that the beliefs simply do not relate to the world in the same way as those of science? Perhaps the Azande do not perceive the futility of their magic because it is not in the nature of their magic to fail by verificationist criteria?

It appears that Lukes might be creating his own errors and therefore his own sociology. He certainly does not allow much scope for turning to the richness of native life. He is even less inclined to seek out 'separate realities' because, like Horton, he is prepared to speak of the 'immensely superior cognitive powers' of science: whereas Tambiah is unsure of the nature of magic and has therefore to turn to ethnographic clues and various interpretative schemata, Lukes has no doubts about the nature of 'odd' beliefs. Basic similarities must lie along one stratum. And as for Horton, he is so satisfied with his picture of religion that he is content to brush away the Beattian challenge with, 'Misdescription...is...evident in the classification of statements about spiritual beings as symbolic rather than explanatory. Failure to account for the data is evident in all versions'. This is absurd: he elsewhere agrees with the fundamentals of Evans-Pritchard's symbolist analysis of Nuer Religion, and who is he to say that a given piece of discourse might not show both symbolic and explanatory aspects?

The articles by Gellner and Wolfram are both, in their different ways, of some interest. Gellner raises an important topic when he discusses the way in which primitive thought combines various aspects which we, in our divided lives, endeavour to keep analytically distinct, and Wolfram, with strong undertones of Pareto, sides with Tambiah over the reality of 'non-scientific' modes of thought. As for the remaining articles, those by Colby and Cole, Nagashima, Finnegan, Whiteley, Ita, and Jenkins, the emphasis swings towards the fruitful juxtaposition of interpretative schemata and ethnographic detail. Unfortunately, the quality of these contributions is very uneven, and, as if anything, too descriptive. Worst of all, none of them attempt to compare primitive religion with western theological, philosophical, or religious traditions. Almost as bad, the editors have not deemed it necessary to introduce an appreciation of Lévi-Strauss' contributions to the general subject under discussion. Their own introductory remarks on the matter completely miss Lévi-Strauss' basic point, namely that normal semantic criteria cannot capture the meaning of myth. On the credit side, however, one might mention Finnegan's exhortations directing us to the primitive's universe of discourse, and Whiteley's exacting analysis of Gusi colour-words and colour-values.

As for Horton and Finnegan's Introduction, one can only say that it accurately reflects the general tone of the book. Their appreciation of Evans-Pritchard is well timed, but one wishes that the rest of the Introduction had aimed at some of the more pressing problems raised by the stance adopted by the more interesting of their contributors. Why, they should have asked,

is there so much pronouncement and so little ethnographic application? Why are not some the issues decided where they should be, that is, in the context of ethnographic material? Why do so many commit the pars pro toto fallacy? Why this faith in one theory for whatever type of discourse is supposedly under discussion? Why such a faith in science as constitutive of religion? Why do so many contributors analyse science, not the various ways in which God can be related to the world? And why do not the editors emphasise the crucial problems - such as the metaphor/literal distinction and the different types of relationships which can exist between realities and different modes of discourse - which must be elaborated if we are to break with the Tylorian and Durkheimian schemata? Above all, why have so many contributors failed to heed Evans-Pritchard's advice?: just possibly, grand comparative questions might better be tackled if we had more sensitive case studies of particular ethnographic phenomena. Just because only a few anthropologists interest themselves in modes of thought is no excuse for premature generalisation.

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