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EDITORIAL NOTE

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

This tenth issue begins the fourth year of the Journal. Our publication now has an international circulation, and we should like to express our thanks to those who have assisted in its production and those who have given us encouragement in our enterprise.

FORMAT

We shall produce one issue per term (three per year). Articles are welcome from students of anthropology and from people in other disciplines. 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 Journal Editors, Institute of Social Anthropology, 51 Banbury Road, Oxford.

BACK ISSUES

We have a stock of back issues still unsold. Single issues are available at 30p. in the U.K. and $1 abroad. Complete volumes (I (1970), II (1971) and III (1972) are each available at the following rates: U.K. - 75p. to individuals, £1 to institutions; abroad $2.50 to individuals, $3 to institutions. The subscription for Vol. IV (1973) is the same. (All prices cover postage). Cheques should be made out to the Journal of the Anthropological Society of Oxford, and sent to the Journal Editors at 51 Banbury Road.

Some Reminiscences and Reflections on Fieldwork

I have often been asked by puzzled students how one goes about fieldwork, and how we fared in what must seem to them those distant days. It had not occurred to me as clearly as it should have done that the information we gathered and published might some time or other be scrutinized and evaluated to some extent by the circumstances of one kind or another in which we conducted our research. So I have jotted down these notes as a fragment of anthropological history. [1]

I

That charming and intelligent Austrian-American anthropologist Paul Radin has said that no one quite knows how one goes about fieldwork. Perhaps we should leave the question with that sort of answer. But when I was a serious young student in London I thought I would try to get a few tips from experienced fieldworkers before setting out for Central Africa. I first sought advice from Westermarck. All I got from him was 'don't converse with an informant for more than twenty minutes because if you aren't bored by that time he will be.' Very good advice, even if somewhat inadequate. I sought instruction from Haddon, a man foremost in field-research. He told me that it was really all quite simple; one should always behave as a gentleman. Also very good advice. My teacher, Seligman told me to take ten grains of quinine every night and to keep off women. The famous Egyptologist, Sir Flinders Petrie just told me not to bother about drinking dirty water as one soon became immune to it. Finally, I asked Malinowski and was told not to be a bloody fool. So there is no clear answer, much will depend on the man, on the society he is to study, and the conditions in which he is to make it.

Sometimes people say that anybody can make observations and write a book about a primitive people. Perhaps anybody can, but it may not be a contribution to anthropology. In science, as in life, one finds only what one seeks. One cannot have the answers without knowing what the questions are. Consequently the first imperative is a rigorous training in general theory before attempting field-research so that one may know how and what to observe, what is significant in the light of theory. It is essential to realize that facts are in themselves meaningless. To be meaningful they must have a degree of generality. It is useless going into the field blind. One must know precisely what one wants to know and that can only be acquired by a systematic training in academic social anthropology.

For instance, I am sure that I could not have written my book on Zande witchcraft in the way I did or even made the observations on which it is based had I not read the books written by that noble man Lévy-Bruhl, and I doubt whether I could ever have convinced myself that I was not deluding myself in my description and interpretation of the lineage system of the Nuer had I not, almost suddenly, realized that Robertson Smith had presented, in almost the same words as I was to use, a similar system among the Ancient Arabians. I do not think I could have made a contribution to an understanding of the political structure of the Shilluk and Anuak if I had not been deep in Mediaeval studies. And I could not have written as I did about the Sanusi had I not had in my mind the model of the history of other religious movements. These last examples illustrate a further point. Strictly speaking, mediaeval

Europe and religious movements might be held to lie outside social anthropological studies, but on reflection it might be accepted that this is not really so, that all knowledge is relevant to our researches and may, though not taught as anthropology, influence the direction of our interests and through them our observations and the manner in which we finally present them. Moreover, one may say that since what we study are human beings the study involves the whole personality, heart as well as mind; and therefore what has shaped that personality, and not just academic background: sex, age, class, nationality, family and home, school, church, companions - one could enumerate any number of such influences. All I want to emphasize is that what one brings out of a field-study largely depends on what one brings to it. That has certainly been my experience, both in my own researches and in what I have concluded from those of my colleagues.

It used to be said, and perhaps still is, that the anthropologist goes into the field with preconceived ideas about the nature of primitive societies and that his observations are directed by theoretical bias, as though this were a vice and not a virtue. Everybody goes to a primitive people with pre-conceived ideas but, as Malinowski used to point out, whereas the layman's are uninformed, usually prejudiced, the anthropologist's are scientific, at any rate in the sense that they are based on a very considerable body of accumulated and sifted knowledge. If he did not go with preconceptions he would not know what and how to observe. And of course the anthropologist's observations are biased by his theoretical dispositions, which merely means that he is aware of various hypotheses derived from existing knowledge and deductions from it and, if his field data permit, he tests these hypotheses. How could it be otherwise? One cannot study anything without a theory about its nature.

On the other hand, the anthropologist must follow what he finds in the society he has selected for study: the social organization of its people, their values and sentiments and so forth. I illustrate this fact from what happened in my own case. I had no interest in witchcraft when I went to Zandeland, but the Azande had; so I had to let myself be guided by them. I had no particular interest in cows when I went to Nuerland, but the Nuer had, so willy-nilly I had to become cattle-minded too, eventually acquiring a herd of my own as the price of my acceptance, or at any rate tolerance.

It will have been evident from what has already been said that it is desireable that a student should make a study of more than one society, though this is not always, for one reason or another, possible. If he makes only a single study it is inevitable that he will view its people's institutions in contrast to his own and their ideas and values in contrast to those of his own culture; and this in spite of the corrective given by his previous reading of anthropological literature. When he makes a study of a second alien society he will approach it and see its people's culture in the light of his experience of the first - as it were through different lenses, in different perspectives - and this is likely to make his study more objective, or at any rate give him fruitful lines of inquiry which might possibly not otherwise have occured to him. For instance, Azande have kings and princes and a fairly elaborate political organisation and bureaucracy. When I went to live among the Nuer after many months among the Azande, I found that although they had quite substantial political groups there appeared to be no political authority of any significance; so naturally I asked myself what gave a sense of unity

within these tribal groups, and in the course of my inquiries I was led to unravel their lineage system. Then, while the Azande were deeply concerned with witchcraft, the Nuer appeared to be almost totally uninterested in the notion or in any similar notion, so I asked myself to what they attributed any misfortune or untoward event. This led to a study of their concept of kwoth, spirit, and eventually to my book on their religion.

The study of a second society has the advantage also that one has learnt by experience what mistakes to avoid and how from the start to go about making observations, how to make short-cuts in the investigation, and how to exercise economy in what one finds it relevant to relate, since one sees the fundamental problems more quickly. It has its disadvantage that the writing-up period is greatly extended - I have still published only a portion of my Zande notes taken down during a study begun in 1927! It is the British intense emphasis of field-research which certainly in part accounts for the demise of the once much extolled comparative method. Everyone is so busy writing up his own field-notes that no one has much time to read books written by others.

The importance of a thorough grounding in general theory begins to reveal itself when the fieldworker returns home to write a book about the people he has studied. I have had much, too much, field-experience, and I have long ago discovered that the decisive battle is not fought in the field but in the study afterwards. Any one who is not a complete idiot can do fieldwork, and if the people he is working among have not been studied before he cannot help making an original contribution to knowledge. But will it be to theoretical, or just to factual, knowledge? Anyone can produce a new fact; the thing is to produce a new idea. It has been my woeful experience that many a student comes home from the field to write just another book about just another people, hardly knowing what to do with the grain he has been at such pains to garner. Can it be too often said that in science empirical observation to be of value must be guided and inspired by some general view of the nature of the phenomena being studied? The theoretical conclusions will then be found to be implicit in an exact and detailed description.

II

This brings me to what anthropologists sometimes speak of as participant-observation. By this they mean that insofar as it is both possible and convenient they live the life of the people among whom they are doing their research. This is a somewhat complicated matter and I shall only touch on the material side of it. I found it useful if I wanted to understand how and why Africans are doing certain things to do them myself: I had a hut and byre like theirs; I went hunting with them with spear and bow and arrow; I learnt to make pots; I consulted oracles; and so forth. But clearly one has to recognize that there is a certain pretence in such attempts at participation, and people do not always appreciate them. One enters into another culture and withdraws from it at the same time. One cannot really become a Zande or a Nuer or a Bedouin Arab, and the best compliment one can pay them is to remain apart from them in essentials. In any case one always remains oneself, inwardly a member of one's own society and a sojourner in a strange land. Perhaps it would be

better to say that one lives in two different worlds of thought at the same time, in categories and concepts and values which often cannot easily be reconciled. One becomes, at least temporarily a sort of double marginal man, alienated from both worlds.

The problem is most obvious and acute when one is confronted with notions not found in our own present-day culture and therefore unfamiliar to us. Such ideas as God and soul are familiar and with some adjustment transference can readily be made, but what about beliefs in witchcraft, magic and oracles? I have often been asked whether, when I was among the Azande, I got to accept their ideas about witchcraft. This is a difficult question to answer. I suppose you can say I accepted them; I had no choice. In my own culture, in the climate of thought I was born into and brought up in and have been conditioned by, I rejected, and reject, Zande notions of witchcraft. In their culture, in the set of ideas I then lived in, I accepted them; in a kind of way I believed them. Azande were talking about witchcraft daily, both among themselves and to me; any communication was well-nigh impossible unless one took witchcraft for granted. You cannot have a remunerative, even intelligent, conversation with people about something they take as self-evident if you give them the impression that you regard their belief as an illusion or a delusion. Mutual understanding, and with it sympathy, would soon be ended, if it ever got started. Anyhow, I had to act as though I trusted the Zande oracles and therefore to give assent to their dogma of witchcraft, whatever reservations I might have. If I wanted to go hunting or on a journey, for instance, no one would willingly accompany me unless I was able to produce a verdict of the poison oracle that all would be well, that witchcraft did not threaten our project; and if one goes on arranging one's affairs, organizing one's life in harmony with the lives of one's hosts, whose companionship one seeks and without which one would sink into disorientated craziness, one must eventually give way, or at any rate partially give way. If one must act as though one believed, one ends in believing, or half-believing as one acts.

Students have asked me how I was able, if I was, to grasp the Nuer concept or image of a cow, since clearly a cow means something more to them than it does to us. The answer again is that there is no choice. One cannot live in intimate association with a people who live by and for their cattle, spending all day, and sometimes all night, with them and their herds, watching them tend their beasts and singing and dancing to their beloved oxen, and hearing them talk about little else, without getting to know what cattle mean to them. Communication would be impossible and it would be impossible to establish any close bond with them. So among the Nuer I found it absolutely necessary to acquire a small herd of my own. We were then fellow-herdsmen with common interests, common language, common affections, and living the same life in the same conditions; and all this, if it may be so put, imposed on me what 'cow' means to a Nuer.

Here arises a question with regard to which my colleagues have not always seen eye to eye with me. In writing about the beliefs of primitive peoples does it matter one way or the other whether one accords them validity or regards them as fallacious? Take witchcraft again. Does it make any difference whether one believes in it or not, or can one just describe how a people who believe in it, think and act about it, and how the belief affects relations between persons? I think it does make a difference, for if one does not think that the psychic assumptions on which witchcraft-beliefs are based are

tenable, one has to account for what is commonsense to others is incomprehensible to oneself. One is in a different position with regard to belief in God, or at any rate I was. We do not think that witchcraft exists, but we have been taught that God does, so we do not here feel that we have to account for an illusion. We have only to describe how a people think of what we both regard as a reality and how in various ways the belief influences their lives. The atheist however, is faced with the same problem as with witchcraft and feels the need to account for an illusion by various psychological or sociological hypotheses. I admit that this is a very difficult philosophical question, for it might reasonably be asked why, other than in faith, should one accept God and not witchcraft, since it could be held, as many anthropologists do, that the evidence for the one is no greater than for the other. The point is, I suppose, that in our culture (leaving out past history and modern scepticism) the one makes sense and the other not. I raise the question even if I cannot give a very satisfactory answer to it. After all, it does make a difference whether one thinks that a cow exists or is an illusion!

Since this question of entering into the thought of another people has been raised, I might touch on a further implication. I wonder whether anthropologists always realize that in the course of their fieldwork they can be, and sometimes are, transformed by the people they are making a study of, that in a subtle kind of way and possibly unknown to themselves they have what used to be called 'gone native'. If an anthropologist is a sensitive person it could hardly be otherwise. This is a highly personal matter and I will only say that I learnt from African 'primitives' much more than they learnt from me, much that I was never taught at school, something more of courage, endurance, patience, resignation and forbearance that I had no great understanding of before. Just to give one example: I would say that I learnt more about the nature of God and our human predicament from the Nuer than I ever learnt at home.

III

It is an academic issue of some importance and one which is often confused and sometimes leads to rancour: what is the difference between sociology and social anthropology? I have discussed this question elsewhere and will not go into it again here, especially as it is only peripheral to my topic. But I would like to touch on a query put to me from time to time by sociology students: why do anthropologists in their fieldwork not employ some of the techniques used by sociologists in their's, such as questionnaires, sampling, interviews, statistics and so forth. The answer is that, though I suppose the situation is now somewhat different, in my day the use of such techniques among a primitive people would not have been to any extent worthwhile, or even possible at all. The peoples I worked among were totally illiterate, so the distribution of questionnaires would have been a waste of time. With a homogeneous rural or semi-nomadic people sampling, such as is required in a socially heterogeneous urban community in our own country, is not only unneceessary but more or less meaningless. Set interviews in the anthropologist's hut or tent, as distinct from informal coversations are generally impossible because natives would not co-operate; and in any case they are undesireable because they are held outside of the context of a people's activities. I made it a rule never to take a note-book with me in public, not that people would have had any idea of what I was doing, but because I felt that somehow a notebook came in between them and me and broke our contact.

I memorized what I saw and heard and wrote it down when I got back to the privacy of my abode. Statistics have a very limited value even when the required numerical data can be obtained - had I asked a Nuer woman how many children she had borne she simply would not have told me, and had I asked a Nuer man how many cattle he possessed he would have, unless he knew me very well indeed, all too likely have withdrawn into an unbroken taciturnity, or perhaps have been violent.

When I say that in my time set interviews were out of the question, private conversations with a few individuals, those whom anthropologists call informants (an unfortunate word) must be excluded. There are certain matters which cannot be discussed in public; there are explanations which cannot be asked for on the spot (as for instance during a funeral or a religious ceremony) without intruding and causing embarrassment; and there are texts to be taken down, which can only be done in seclusion. It is necessary therefore to have confidential informants who are prepared to attend regular sessions, maybe daily; and it is evident that they must be men of integrity, truthful, intelligent, knowledgeable, and genuinely interested in your endeavours to understand the way of life of their people. They will become your friends. Among the Azande I relied mostly on my two personal servants and on two paid informants, but as usual in Africa, there were always people connected with them coming in and out of my home. The one young man whom I came across who was capable of writing Zande was for a time my clerk, having been sacked from the C.M.S. Mission for having married a divorced woman. Among the Nuer and Anuak and Bedouin I never found anyone who could, or would, become an informant in the sense I have set forth above, and so I had to do the best I could, gathering information from all and sundry. One has to be very careful in one's selection of informants, if one has the opportunity to be selective, for it may be found that it is only a particular sort of person who is prepared to act in this capacity, possibly a person who is ready to serve a European as the best way of escaping from family and other social obligations. Such a man may give a slant to one's way of looking at things, a perspective one might not get from others.

Sometimes it is said that the anthropologist is often hoodwinked and lied to. Not if he is a good anthropologist and a good judge of character. Why should anyone lie to you if there is trust between you? And if there is not, you might as well go home. If you are in the hands of an interpreter it is true that it can be a hazard, but if you speak the native language you can check and re-check. It would be improbable in these circumstances, unless everybody is telling the same story, for a man to get away with an untruth. There may be, and very often is, a difference, sometimes a considerable difference of opinion between one informant and another about a fact, or its interpretation, but this does not mean that either is telling a lie. Natives are not all of the same opinion any more than we are; and some are better formed than others. There may, of course, be secret matters about which an informant does not wish to speak and he may then prevaricate and put you off from pursuing a line of inquiry for one reason or another. Till towards the end of my stay among the Azande my inquiries, even among those I knew and trusted most, about their secret societies met with lack of response. Informants, who were members of these societies, pretended to know nothing about them. As they were sworn to secrecy they could scarcely have done otherwise.

However, to an observant anthropologist a lie may be more revealing than a truth, for if he suspects, or knows, that he is not being told the truth he asks himself what is the motive of concealment and this may lead him into hidden depths.

Perhaps here is the place to discuss another question which has frequently been put to me. Does one get the native view about life (and about women) from men only or can one get to know the women as well and see things from their viewpoint? Much depends on the people one is studying and the status of women among them. During an abortive (war broke out) field-study in an Upper Egyptian (Quft) village I never spoke to a woman or even had more than a flitting sight of one at night. Bedouin women in Cyrenaica did not veil and could be conversed with if not with intimacy, at least without embarrassment. The Zande were almost an inferior caste, and unless elderly matrons, shy and tongue-tied. In Nuerland, where women have high status and assert their independence, they would come and talk to me whenever they chose, often at times most inconvenient to me. It seemed to be an endless flirtation. Certainly it was they and not I, who made the going. On the whole I would say that the male anthropolgist, not fitting into native categories of male and female and not therefore being likely to behave as a male in certain circumstances might be expected to behave, does not come within their range of suspicions, judgements and codes. In a sense he is, since he lies outside their social life, however much he may try to identify himself with it, sexless. For example, nobody in Zandeland objected to my chatting to their womenfolk, but had an unrelated Zande done so there would have been serious trouble; in pre-European days indemnity would have been demanded or emasculation would have ensued.

It is asking a rather different question, whether a woman anthropologist can obtain more, or better, information about women's habits and ideas than a man can. Here again much depends on the kind of society. Obviously in an urban Moslem society, where women are secluded in hareems only a woman has access to them. But I would say that elsewhere I have seen little evidence that female anthropologists have done more research into woman's position in society and in general their way of life than have male anthropologists. I would add that I doubt whether it is even an advantage for an anthropolgoist to be accompanied by his wife in the field. They then form a little closed community of their own, making it difficult for both to learn the native language quickly and correctly and to make the required transference which only the feeling of need for close company and friendship can force a man to make. However, I would imagine that the man with a wife in the field gets at least better fed; but since I was not married when I did my research this hardly comes within my reminiscences. But I cannot resist the observation that, as I see it, what eventually ruined our relations with the peoples of the Southern Sudan were motor-cars and British wives.

Another matter which has some bearing on the subject of informants is the anthropologist's relations with other Europeans in the areas in which he is working. In the Southern Sudan in my day this presented no great problem. There were only a handful of administrative officials, a few missionaries and an occasional doctor. With one or two exceptions, I found them kindly, hospitable, and willing to be helpful. Sometimes they were able to give me information which, though it was not always accurate or from an anthropological point of view adequate,

saved me time and enabled me at least to make a start. This was particularly the case with the American Presbyterian missionaries in Nuerland, with Mr. Elliot Smith among the Anuak, and Archdeacon Owen among the Luo of Kenya. In this matter the anthropologist has to be wise. After all he is, so to speak, an intruder into their territory, a territory about which they have often and for a long time been considered, and considered themselves to be, the main or even sole authority. There is no need or purpose in his being condescending, and if he has got the sense not to be, they will not in my experience hold back a willing hand. Let him therefore always remember that, at any rate at the beginning of his research, though he may know more general anthropology than they, they possibly know more about the local ethnographical facts than he. Also let him remind himself that if he cannot get on with his own people he is unlikely to get on with anyone. And, furthermore, they are part of what he is supposed to be studying.

But I must caution students not to accept, above all in religious matters, what they may find in mission literature. The missionary generally only knows a language outside of the context of native life and therefore may well miss the full meaning of words which only that context can give him. The fact that he has been among a people for a long time proves nothing: what counts is the manner and mode of his residence among them and whether God has given him, among other blessings, the gift of intelligence. I have advised caution above all in religious matters. It is, or should be, obvious that since the natives do not understand English the missionary in his propaganda has no option but to look in the native language for words which might serve for such concepts as 'God', 'soul', 'sin' and so forth. He is not translating native words into his own tongue but trying to translate European words, which he possibly does not understand, into words in a native tongue, which he may understand even less. The result of this exercise can be confusing, even chaotic. I have published a note on the near idiocy of English hymns into Zande. Missionaries for instance, have used the word mbori in Zande for 'God', without any clear understanding of what the word means to the Azande themselves. I have been attacked, with a good deal of malice, by Fr. Giorgetti for pointing this out. Even worse things have happened in the Nilotic languages, or some of them. I am not going to pursue this matter further now beyond saying that in the end we are involved in total entanglement, for having chosen in a native language a word to stand for 'God' in their own, the missionaries endow the native word with the sense and qualities the word 'God' has for them. I suppose they could hardly have done otherwise. I have not in the past made this criticism of missionaries because I did not wish to give offence and because I thought any intelligent person could make it for himself.

Here might be a suitable place to discuss a related topic. I do so very briefly because Dr. Beattie in Other Cultures has said a good deal about it, and very well. How much help can the anthropologist get from technical experts who have worked in his area of research-agronomists, hydrologists, botanists, doctors, vets and others? The answer is that he can gain information he cannot himself obtain and that some of it may be relevant to his own problems and lines of inquiry. Only he can judge what has relevance and what has not. Succinctly stated, a physical fact becomes a social one when it becomes important for a community and therefore for the student of it. That the Nile and its tributaries rise in their beds at a certain season of the year is a fact that it is essential for the Nuer, and me, to know,

but exactly why they rise hardly concerns either of us. That the Azande are unable, whether they would wish to or not, to keep domesticated animals, other than dog and fowl, on account of tsetse morsitans is obviously a fact important to know, but knowledge of the pathology of the trypanasomes is not going to shed much light on the social effects of what they do. But one must beware of accepting what anyone tells you about native life, whatever his special qualifications may be. An awful example would be de Schlippe's book on Zande agriculture, for what he describes in it are less Zande modes of cultivation than those imposed on the Azande by the Government of the Anglo-Egyptian Sudan. Had de Schlippe been able to speak and understand the Zande language he might have realised this. Also beware of a joint team of research. It can only lead to waste of time and irritation. Meyer Fortes told me that when he was in such a team in Ghana he spent much of his time and energy in trying to explain to other members of the team the significance of their observations, and when I became a member of a team for study in Kenya I was the only member of it who turned up and did anything. When I did my research in the Sudan there were no agronomists, entomologists, and so on, so I had to do the best I could to be my own expert. Perhaps it was just as well.

All that was required in one's dealings with Europeans in a country ruled by the British were tact and humility. Things have changed. In the first place, it has become increasingly difficult, often impossible, to conduct anthropological research in many parts of the world. Clearly, at present, one would be unlikely to be encouraged to do so in soviet Russia, and at any rate some of its satellites, or in China. In present circumstances I would not, though I speak Arabic, care to try to do research in most of the Arab lands. Even were I given permission to do so, there would be constant supervision and interference. In such countries the anthropolgist is regarded as a spy, his knowledge likely to be used in certain circumstances by the Intelligence of his country; and he is also resented as a busybody prying into other people's affairs.

Even when this is not the case and in countries where no acute political issues are involved, there may be, and I think generally is, a hostile attitude to anthropological inquiries. There is the feeling that they suggest that the people of the country where they are made are uncivilized, savages. Anthropology smells to them as cultural colonialism, an arrogant assertion of European superiority - the white man studying the inferior black man; and they have some justification for their suspicions and resentment, for anthropologists have in the past only too readily lent and sold themselves in the service of colonial interests. The late Dr. Nkrumah once complained to me that anthropologists tried to make the African look as primitive as possible: photographing people in the nude and writing about witchcraft and fetishes and other superstitions and ignoring roads, harbours, schools, factories, etc. Indeed, anthropology has, I think rather unfairly, and without its intentions and achievements being really understood, become a bad word for the peoples of new and independent states, perhaps especially in Africa. So I have for many years advised students about to embark on fieldwork to claim that they are historians or linguists, subjects which no one can take offence at; or they can talk vaguely about sociology.

On the question of the length of fieldwork, I would say that a first study of a people takes, if it is to be thorough, up to two years. (My stay among the Azande was twenty months). I do not think it can be much less (in spite of the American way of doing research). Ideally, the programme would be something like a year in the field and then a break of some months to chew the cud on what one has gathered, discussing with friends problems that have arisen in the course of one's work, and sorting out what has been omitted and overlooked during it. Then back to the field for another year. This has not always, or even often, proved to be possible. Furthermore a student must, if anything is to become of his research, have at least another year for writing-up. This, again, is not always, or even often, possible, and the fieldworker may be compelled to accept a post in which he is plunged into teaching, and the results of his research become stale. How often has this not happened?

IV

Obviously the most essential of all things the anthropologist must have in the conduct of his inquiries is a thorough knowledge of the language of the people about whom he is going to tell us. By no other path can their thought (which is what I have myself chiefly been interested in and why I have spent a lifetime in anthropology) be understood and presented. So in the researches I have made, other than the ethnographical surveys (through the medium of Arabic) to oblige Prof. Seligman, I have struggled with and mastered the native language - Zande, Nuer, Anuak, Bedouin, Arabic, and even Luo and Galla to some extent. All English anthropologists today, unlike their predecessors, Rivers, Haddon, Seligman and others, would pay at any rate lip-service to this requirement and would claim that they have spoken native tongue well. They may have done so, but they seldom display evidence of their ability. Even when I have little doubt that they understood languages, a critic may, and probably will, at sometime in the future ask what their credentials are. In the past these could be presented in the form of texts (with translations), but today this cannot be accepted as certain evidence, for as most 'primitive' societies become literate it is possible for the anthropologist, as it was seldom, often never, possible in my day, to find people to write his texts for him and to translate them. I met only one Zande who could write at all coherently, while among the Nuer, Anuak, Bedouin Arabs and other peoples there was no one; so I had to take down texts myself, and in the hard way, there being at that time no tape-recorders, an instrument not always an advantage. Being brought up on Greek and Latin, texts were for me a necessary accomplishment and my passion for them was inflamed by Malinowski who in his turn had been inspired in this matter by the Egyptologist Sir Alan Gardiner. The trouble, however, is to get vernacular texts published - who can or wants to read them? I have done my best for Zande. It has cost me much time and money; and I have given up all hope of publishing others in that language or in other languages.

One of the things I have often been asked is how does an anthropologist make even a start in his study of a primitive society. I must answer the question in the light of my own experience, which may not be quite the same as that of others working in different conditions. [2] It helped of course that most of my research was carried out in a country, the Sudan, at that time ruled by the British

and with a government and its officers friendly disposed to anthropological research. What helped also, I think, and even more, was that the British were few and far between, that in other words one could be liked or disliked, accepted or rejected, as a person and not as a member of a class of persons (which was very unlike Kenya, where it was hard to decide who were the more unpleasant, the officials or the settlers, both of whom were so loathed by the Africans that it was difficult for a white anthropologist to gain their confidence). But given favourable conditions, such as generally obtained in the Sudan, it has always seemed to me to be perfectly simple to walk into a so-called primitive society and sojourn there. Why should anybody object since one does no harm and is a guest? Would not I feel the same if one of them came to live near me? I did not expect, as some American anthropologists appear to, to be loved. I wanted to give and not to be given to; but I was always received with a kindly welcome, - except among the Nuer, but they were bitterly hostile to the Government at the time. I suppose that if one knows one is going to be so received one just turns up and hopes to get to know people, and in my experience they are happy to be known. It may happen that an anthropologist who has encountered difficulties among one people might not have done so among another. To this extent it could be said that there is an element of chance.

There are really no directions that can be imparted about how one gets to know people. Somehow or another one finds a couple of servants, or more likely they find you, and one or two men who are prepared for a reward to teach you the language; and these people tend to identify themselves with you so that nothing you possess is 'yours' any more, it is 'ours'. Then they get some kudos for having - I was going to say owning - their white man, and are happy to introduce him to their families and friends, and so it goes on. There is an initial period of bewilderment, one can even say of despair, but if one perseveres one eventually breaks through. I have always found that the best way, largely unintentional on my part, of overcoming my shyness and sometimes my hosts' suspicion has been through the children, who do not have the same reserve towards a stranger, nor if it comes to that, did I on my side towards them. So I started among the Azande by getting the boys to teach me games and among the Nuer by going fishing every morning with the boys. I found that when their children accepted me their elders accepted me too. Another tip I venture to give is not to start trying to make inquiries into social matters - family, kin, chieftainship, religion or whatever it may be before the language has to some extent been mastered and personal relationships have been established, otherwise misunderstandings and confusions may result which it may be difficult to overcome. Anyhow if you do what I did, refuse, or was unable, to make use of an interpreter you cannot in the early stages of research inquire into such matters. The way to begin is to work steadily for twelve hours a day at learning the language, making use of everybody you meet for the purpose. That means that you are their pupil, an infant to be taught and guided. Also people easily understand that you want to speak their language, and in my experience in your initial gropings they are sympathetic and try to help you. The strictest teachers were the Nuer, who would correct me, politely but firmly, if I pronounced a word wrongly or was mistaken in its meaning. They were quite proud of their pupil when he began to talk more or less intelligibly. Then, being mute to begin with, one learns each day through the eye as well as by the ear. Here again it seems to people both innocent and reasonable, if sometimes a bit amusing, that you should, since you have sprung up from nowhere, to

join them, take an interest in what is going on around you and learn to do what they do: cultivating, pot-making, herding, saddling camels, dancing, or whatever it may be.

I will only add to these random remarks that I have always advised students going into the field to begin by learning a few new words each day, and by noting materials things. Every social process, every relationship, every idea has its representation in words and objects, and if one can master words and things, nothing can eventually escape one. A final hint: get away from servants and regular informants from time to time, and meet people who do not know you; then you will know how badly you are speaking their tongue!

V

It may well be asked, and it sooner or later has to be, what should one record about a people one makes a study of and how much of the record should one publish. I have always held, and still hold, that one should record in one's notebooks as much as possible, everything one observes. I know that this is an impossible task, but long after, maybe many years after one has left the field and one's memory has faded, one will be glad that one has recorded the most familiar and everyday things - what, how and when people cook, for example. I have now lived to regret that I did not always do so. And how much that goes into the notebooks should go into print? Ideally, I suppose, everything, because what is not published may be, and generally is, forever lost - the picture of a people's way of life at a point of time goes down into the dark unfathomed caves. And one cannot know how valuable what may appear to one at the time to be a trifle may be to a student in the future who may be asking questions which one did not ask oneself. I feel it therefore to be a duty to publish all one knows, though this is a burden hard to be borne - and publishers think so too. One is burdened for the rest of one's life with what one has recorded, imprisoned in the prison one has built for oneself, but one owes a debt to posterity.

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

E. E. Evans-Pritchard.

Notes

1. This paper is based on talks given in the Universities of Cambridge and Cardiff.

2. Notes and Queries was certainly of little help to me. I carried my books in my head, but for the record I will say this: before I went to Nuerland I talked over with Max Gluckman the problem of books and we decided that if I could take only one to guide me it should be Lowie's Primitive Society. It was a very good choice.

Tylor's Solar Sixpence

E. B. Tylor, in contrast to Andrew Lang (Marett: 80, 82) and Herbert Spencer (Tylor 1877: 155), is often seen (Dorson: 187-191) as an avid supporter of the astral and meteorological readings of ancient Indo-European mythology popularized in Great Britain by Friedrich Max Müller (1864: 369-524; 1868: 80-111) and other philologists. In print, especially in the 1860's, Tylor accepted their view that misunderstanding and personification of general attributive names for clouds, planets and stars especially the sun in its course (Tylor 1865 in Marett: 81-83; 1869: 532; 1877: 149-150, 155) had yielded the names and attributes of many Vedic, Greek and Roman deities and heroes. He accorded to their analyses sound principle, largely valid detail and an unassailable basis in ancient knowledge (1868: 226-227). In his own published examination of not solely Indo-European myths and beliefs, Tylor in the 1860's, however, held to no comprehensive theory (Marett: 83); in separate works he resorted to simple solar or more complex euhemerist or sensationalist explanations (Tylor 1861, 1865 in Marett: 81-82, 84-85; 1869: 524).

But by April 1868 Tylor wrote a lengthy private parody of Müller's (and his own) solar mythmaking called "The Mythe of Day". It was composed considerably before the publication of the famous extended travesties of solar theory by R. F. Littledale (1870) and Andrew Lang (1886) (Dorson: 163 n.1; Marett 82 n.4). Its subject, unlike theirs, was taken not from contemporary history - Max Müller or Gladstone - but from childhood. It was a nursery rhyme, obscure yet commonplace as the illustrations for it emphasized.

The Mythe of Day[1]

(by E.B. Tylor).

Crushed by the commentators & historians, the relics of the great Solar mythe of our race have found refuge in the nursery, or have gained a more honourable though less honest position as fictitious history. Thus no student, familiar with the mythic deposit in English chronicle, could mistake the sense of the radiant Sun scorching with ardent ray the earth exposed to his beams, which figures in the tale of Alfred burning the cakes left in his charge [s. 2] by the departing but returning night. And thus in the still lingering Sagas of the nursery, History & Mythe are blended in a compound, which no skill save that of the Aryan mythologist can now analyze. The first lines of one such Saga throw an interesting light on the transition period, when the minstrel no longer received from his rapt hearers payment altogether in kind, but when nevertheless a coined currency had only in part come into use, as we read,

"Sing a song of sixpence (& also
A pocket full of Rye".

[s. 3] The mythic song commenced,

"Four & twenty blackbirds baked in a pye".

The simile of the pye representing the underlying earth, & the

overarching heaven or crust is found among the negroes of West Africa, who describe Heaven & Earth as the two calabashes which cannot be opened; or in the ancient Aryan symbol of the World-Tortoise whose flat under-shell is the earth, whose body is the air, & whose arching carapace is the sky.

We need hardly say, that the four & twenty blackbirds are the twenty four hours, which lie between Heaven & Earth. But the dawn came cutting with its [s. 4] first wedge-like incision that we so often have looked on with delight, as we watched the sunrise after a night spent in the giddy dance, which still keeps up the undying symbolism of Solar worship. The pye was opened, & (a touch most true to nature)

"When the pye was opened
The birds began to sing,
And wasn't that a dainty dish
To set before a king?"

The king, before whom the dish was set, still has it displayed to his burning gaze as he climbs to his meridian, & pours upon the Earth below the golden shower of Dawn, the bright sunshine, which, to show a [s. 5] thought of the regularity -- & accuracy of natural phenomena, he is said to count out in his counting-house.

"The king was in his counting-house,
Counting out his money"-

[A]^2

His wife, the imperial Selene, was still in the parlour within the door of night, preparing the moonlight by copious meals of streaming honey (the bread is [s. 6] bee-bread).

"The Queen was in her parlour
Eating bread & honey".

[B]^2

But dawn arose before her master, tis therefore called the maid (an allusion to which survives in her red hands, or "rosy fingers" "ροδοδάκτυλος Ἠώς"), & she spread across the sky the Clouds, which are the radiant clothes to be poisoned by the baleful evening, & to become [s. 7] the fatal, clinging, burning robe of the Deianeira of the sunset.

[C]^2

At last the day's work is done; the clouds are spread & dried on the "lines" of solar rays; the money is counted; the honey eaten; the four & twenty blackbirds think on the whole they have had enough of day & song, & that it is time to roost. Rest & joy to them, but death to the Day, [s. 8] the hanger of the Celestial garments on the Sky-line. The Magpie of Night raises her devouring beak above the horizon, & the foremost projection, the nose of Day, is snipped off.

[D]^2

Into the horrors of the full absorption of day into the hungry ravenous night, the tender-hearted mythologist forbears to enter.

April 1868 --

1

This eight-page manuscript of "The Mythe of Day" is in the writing of Tylor's wife Anna on stationery embossed with the heading "Crux Praesidium et Decus" [Cross, Protection and Honour]. It was perhaps never published, although a synopsis appeared in Primitive Culture (1871: I, 287-288) with an altered interpretation of the last verse. The blackbird was now the sunrise at the end of dawn rather than the magpie of night at the end of day. I have reproduced the manuscript with the kind permission of A.L. and F.M. Tylor. I have used "&" to represent Tylor's " + "; my additions of accents, numbers of sides and alphabetical references to illustrations are given in brackets.

2

Edward or Anna Tylor made comical line-drawings for "The Mythe of Day" which cannot suitably be reproduced here in full. [A] shows a figure with a sunny head on a man's body sitting in a chair and dispensing spiral coin-rays. [B] has a well-dressed woman with a crescent moon for a face holding in one hand a piece of bread and indicating with the other a beehive on a table. [C] depicts a maid in starched dress, apron and cap hanging cloudlike clothes on a clothes-line. The sunny-headed figure, with his hands in his pockets and puffing on a large, redolent cigar, shambles up the stairs toward her. In [D] a blackbird flies by and bites her nose, as she raises her arms in fright.

3

Deianeira: the wife of Herakles who gave him a burning robe poisoned with a centaur's blood.

* * *

Subsequently Tylor did criticize in print the extravagance (1871: I, 287-288; 1877: 155) and ignorance of the historical bases for myths (1879:388) of some solar theorists, but not of the "more cautious and conservative" Müller (1876:236). Tylor, himself long cautiously trying to dislodge study of the origins of mythology from the hands of the philologists (ibid.; Marett: 80) and the rasher of the social scientists, now stressed the diverse, overlapping sources for myth among different peoples and in different periods. He no longer accepted solar imagery alone as a deity's justification; it might perhaps supply the motive for certain of the god's actions (Marett: 83, 93). With his usual spirit of compromise, Tylor allowed that intricate solar imagery and verbal misunderstanding might be central to the "pure", that is subjective (Marett:84) and theistic (Tylor 1880:14) myths surrounding the lofty Indo-European gods. But not all people and thoughts were Indo-European and Indo-European ones were not necessarily early or primitive. Tylor tried subtly to stress the more earthly, objective origins of myth among early and primitive peoples. Not names (cf. Müller 1864:447-448), but perceptions of things underlay language and myth, Tylor thought (Marett: 92); not incomprehensible supernatural deities, the last stage in the personification of nature, but more naturalistic, "animatistic" (Marett: 91 and n.5) representations of reality were primary to primitive religion (Marett: 84-86, 96-97; Tylor 1871: I, 271). The "visible, palpable, active, individual objects" (1866: 81) that it incorporated might include the sun, but did not pale into insignificance beside this luminary. In any case, primitive theories about the sun and other material objects ought not to be despised; they were the "rude science" of their day as well as the mythology of the future (1866: 72-73; 1869: 524; 1877: 149-150; Marett: 93,96).

Joan Leopold.

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Two Styles in the Study of Witchcraft

The recent interest of historians in the subject of witchcraft places anthropology under an obligation to look very carefully at its achievements in this field. We might feel flattered that other scholars have felt fit to declare that their own researches can progress only if they make use of our writings (Thomas: 436n) but for this to be true we must be willing to examine critically what we have written ourselves. For, though Evans-Pritchard's Witchcraft, Oracles and Magic among the Azande is a landmark, the last generation of anthropologists has not, in fact, made any great theoretical progress with this topic. To the extent that this is true I am more doubtful than some historians of the value of what we actually have to offer them.[1]

In this paper I shall marshall a series of ideas from other disciplines in an attempt to open up some promising new lines of thinking on the subject of witchcraft.[2] My particular concern is, with the aid of such concepts as 'semantic field', 'persons' and 'performative utterances', to advance our comprehension of this one problem. But there are issues involved here which transcend this single subject, so I have thought it appropriate to entitle the paper in such a way as to suggest Louis Dumont's 'deux théories'. Those deep differences of outlook which clearly exist within kinship studies in fact permeate every area of social anthropology, so I shall present my re-thinking of witchcraft as a particular example of two very different styles of social anthropology in general. These two outlooks are not to be subsumed under the labels 'functionalism' and 'structuralism', for I hope the kind of concern I raise here will enable the discipline to pass beyond structuralism, as well as beyond our older English style of anthropology.

* * *

I must begin by briefly commenting on some recent works on witchcraft which I find inadequate. Two of the three books I shall discuss are dedicated to Evans-Pritchard, yet clearly represent, in the main, that style of anthropology from which he himself has quietly dissented. In 're-thinking' witchcraft I shall certainly not deny the achievements of this tradition. Mair is therefore right to declare (1972: 40) that one need not scrap everything that has been done in the past thirty years; but, of course, no one has ever suggested this. But I do accept Beidelman's contention that we 'need a rethinking of the approach itself, rather than simply more studies'. (1970)

Beidelman's brief paper is a rather dissonant epilogue to ASA9, for few other contributors in fact offer any fresh approaches. 'Boundarism' in the editor's introduction is certainly a step forward, but the very title of the volume indicates the difference in interest between most authors and Evans-Pritchard. The whole subject seems largely to be stuck in that 'micro-sociology' version of anthropology; Marwick's 'social strain' hypotheses, 'mystical idioms' and the 'dissolution of relationships', and so on, loom large. Thus Esther Goody attempts to explain why it is that Gonja associate evil power with women. Yet we need to be told a great deal if we are not simply to see this as a piece of sociological metaphysics. Goody must at least provide us with a full grammar of the male/female opposition in that culture; and fully constituting the symbolic order might weaken the desire to indulge in sociology. ASA9 is a very uneven volume, but when we recall that Evans-Pritchard's work arose out of studies of English intellectualism and the writings of Lévy-Bruhl, Douglas' remark that his Zande monograph was about 'knowing' serves only to remind us that the more important of

his insights have not been developed.[3]

The second work dedicated to Evans-Pritchard, The Allocation of Responsibility (ed. Gluckman; 1972) is a set of offerings by the Manchester school.[4] Again I can only say that the least interesting aspects of his work have been taken up. It is a typical case of that easy resort to sociology which leaves the real anthropological problems untouched and which has left major areas of our discipline frozen for a whole generation.[5]

Mair's general book on witchcraft (1969) is little more than a simple description of other people's work; it is almost entirely innocent of theory. In one chapter she discusses theories of witchcraft, but does not really pass beyond functional writings. It is indeed rather strange that Mair should have written such a book, for she contributed nothing to ASA9, and her publicly expressed interest is in politics, jural relations and applied anthropology - language and symbolism being subjects which might concern other anthropologists. This bias is not without influence upon the position taken in her discussion of witchcraft. Thus, those who disapprove of such terms as 'supernatural' powers she castigates as 'purists' (ibid: 7). She distinguishes between witchcraft and sorcery in terms of the possibility of finding evidence, irrespective therefore of what the natives say, and presumably in terms ultimately of what the anthropologist himself thinks plausible or not (ibid: 23). She also discusses the 'universal image' of the witch when her ethnographic examples show there is no such image: we need only consider whether witchcraft is said to be hereditary or not, whether witches are claimed to be consciously evil or not, to realise the disparate phenomena which have been subsumed under one label. Finally, Mair suggests that analyses from Evans-Pritchard onwards 'have argued that (such beliefs are) by no means irrational in the context of the African's limited understanding of causation -'. I am not sure that others would have chosen that precise phraseology.

* * *

Having made clear my attitude to one style in the treatment of witchcraft, I shall present another view which rests in a rather different conception of anthropology itself. I offer no second theory, merely a series of ideas which singly or in combination might advance our comprehension of the topic. I shall begin by adopting the strategy utilised by Lévi-Strauss with totemism and more recently by Needham with kinship,[6] namely to deny the 'phenomenon' a real existence by asserting that the very acceptance of a separate problem of 'witchcraft' is part of the problem, and one source of our inability to resolve it satisfactorily. I shall contend that 'witchcraft' is wrongly isolated and in that sense is unreal, and consequently that a possible means of analytic advance will be to dissolve it into a larger framework. It may well be that 'the' problem of witchcraft has been constituted in anthropology because of the existence of 'witchcraft' in the history of our own society, and this factor may have destroyed what we have learned about the translation of culture with other problems. For the fact is that English witchcraft is <u>not</u> like the phenomena so labelled in other cultures. Some 'purism' may here be salutory, for there are dangers in both acknowledging that phenomena are different and also calling them by the same name. Here I am not referring to superficial differences, but the very fundamental gulf between the intellectual structure of Tudor and Zande society. English society possessed a word 'witchcraft', but anthropologists have committed a possibly grave error in using the same term for other cultures, of which historians must certainly be aware when they use anthropological writings.

English witchcraft flourished in a culture which possessed such categories as 'natural philosophy' and a theological system upon which witchcraft beliefs were, in part, parasitic. Are we to speak of Zande 'witchcraft' in a culture which lacks these categories of thought which served to define the 'witchcraft' of our own society? Where the intellectual configuration which forms the conceptual environment is so different can we really expect to find the 'same' phenomenon in both?[7] We have possibly been misled here by the availability of a term supplied by our own history which has very probably acted as a general means for the illicit importation of a whole host of cultural terms for the description of another society. Where language does our thinking for us, Wittgenstein's motto 'let us not be bewitched' seems highly appropriate.

I shall commence the discussion of the larger framework which is to absorb 'witchcraft' by referring to Winch's paper 'Nature and Convention'. His remarks here are very important in view of the seeming extreme relativism of The Idea of A Social Science (1958). In his article he argues, against those who associate the conventional with variability, and the natural with invariability, that the possession of some types of norms is not optional in society 'because the idea of their non-adherence is made unintelligible by certain features of the concept of the social life of human beings'. The social invariable I suggest here may aid with the problem of witchcraft, but as a cultural universal it may well prove useful in other discussions.

The data with which the social sciences deal are not 'behaviours' in 'space' but the 'action' of 'persons' in a 'shared conceptual and moral space'. Society as a normative system and a system of ideas could be offered as a truism were it not for the conception and methodology of most social sciences attempting to recast human phenomena in such a way as to make them unrecognizeable. But it is in the framework of a 'moral space'[8] that I shall endeavour to lose witchcraft. For this limited purpose I shall propose two primary structurings of moral space: firstly a system of action concepts and action-evaluation concepts; secondly, a system of 'person' categories. I hope in this larger context 'the' phenomenon of witchcraft will lose its identity, and will appear rather like an alien who having watched a game of chess had decided to write a treatise called 'bishops'. No understanding of 'bishops' is possible save in the context of the whole rule system which constitutes chess, for in Saussurean terms, a bishop means nothing by itself but derives its value from all the types of pieces that are not 'bishops'. I am thus suggesting that a study of witchcraft per se is nonsensical, and ipso facto a comparative study of witchcraft an absurdity raised to a higher power. A sign of conceptual advance in this field will perhaps be our ceasing to write on witchcraft. So I disagree with Standefer (1970) who saw the first problem as that of defining witchcraft; I shall endeavour to deny the phenomenon; to define it away.

Proposing that the first articulation of moral space is a set of action and evaluation concepts brings to our attention at once the fact that anthropology has actually done very little work upon this subject. Incidentally, of course, we have gained some knowledge, but our interests have perhaps directed us away from what must be, by any standards, a most important problem. How, for instance, are we to explain 'sacrifice' for example, or ritual in general, if we have made no concerted attempt to constitute the action concepts of the culture in question. There seems no reason to expect primitive cultures to lack a repertoire of such concepts, as rich as that which exists in ordinary English.

Indeed, it could well be that 'folk-social psychology' in pre-industrial cultures would prove to be more discriminating, and it may well be that our own 'scientific' psychology with its penchant for supposedly precise technical terms has impoverished our own culture in this respect. It is at least significant that a recent and very important book in social psychology (see R. H. Harré and P. Secord: 1972) should unashamedly return, with a host of philosophical justifications to the importance of ordinary language. Its claim that the established scientific model gives us a sham-exact knowledge of less than we already knew is surely entirely correct.

I cannot leave this first structure of the moral space without commenting on the word 'moral', which is of course one of our own culture's 'action and evaluation' concepts and in connection with which, therefore, a whole host of translational problems arise. For my universal structure, 'moral' or 'ethical', because culture-bound, are decidedly unsatisfactory, but I cannot here suggest any other terms. For the 'invariable' framework, I need a set of terms on a higher level than that used for comparative purposes, so 'moral' is inappropriate by at least two orders of discourse. With all its specificity, I must continue to use these cultural terms: the 'theoretical' level of discourse cannot be expected at the very beginning of the inquiry. But perhaps I can offer some compensation here by suggesting a few ideas which might at least start the investigation.

I am in agreement with Collingwood and MacIntyre in regretting the loss of the historical dimension from philosophical discussions. We have often been offered general theories of ethics, yet it is surely important to notice that the moral 'ought' appeared at a certain time in our own culture. Collingwood, for instance (1944) suggests that Greek philosophers lacked this concept and therefore it is only by a mistranslation that we can say that Greek moral philosophy and Kantian moral philosophy are on the 'same' subject matter.[9] As MacIntyre says (1971: 154) we need not so much a general theory as a history of moral notions. If we are to attain a better view of what constitutes the 'social' and the 'humane', obviously investigation must be historical and comparative. Here, that inquiry which goes under the name of 'the sociology of knowledge' might prove valuable. Wilden, for instance (1972: 212) draws our attention to certain possibly sociological aspects of the Cartesian '<u>cogito, ergo sum</u>'. After all there are certain social conditions in which one would perhaps not begin such a premise with a verb in the first person singular. (Perhaps we should now wish to say something like '<u>loquor, ergo sociale animal sum</u>'.) Certainly, for instance, there are social conditions under which the Kantian 'imperatival' and Puritan view of the 'moral' would be unintelligible. Douglas[10] (1970) suggests that different types of social structure may relate to very different notion of 'sin', 'evil', 'self', and so on. Detailed investigation of the history of the semantic fields embracing such concepts as 'self', 'person', 'moral', 'idea', 'natural', and so on would probably prove extremely valuable. If anthropology is 'man-talk' then study of these basic items of humane vocabulary must sometime be carried out as a preliminary to wider investigations.[11]

The second primary articulation of moral space is a system of person categories.[12] We have a total field of 'persons' through which will be variously distributed ranges of predicates ascribing attributes and powers. Thus, to take a system of terms rendered as 'witch', 'sorcerer', 'diviner', 'prophet', 'priest', we shall expect significant differences in the symbolism of these different persons:

thus, witches may lack certain attributes that other humans display, and be thought to manifest powers not possessed by others. By detailing this particular problem, hopefully attention will be drawn to an aspect not sufficiently treated by the broadly functional approach to witchcraft: namely, the full symbolic systems have not been mapped out for what is probably a most important conceptual system in all cultures. We have been told, for instance, that a witch possesses 'supernatural' powers without being informed on what ranges of predicates are ascribed in that culture to non-witches. And if we fully constitute a 'person' field[13] out of the categories and discriminations made in other cultures we shall perhaps be able to look more closely at the classifications we ourselves make. For the particular purpose of this paper, the image is of an 'ethical game': the person not only wrongly called a 'witch' but also ripped out of context is only one piece on a moral board. The moral game involves other pieces with varying specifications of powers, and it is in this game that the separate problem of witchcraft should be lost.

* * *

Notions such as 'ethical space' and 'moral geometry' may have struck some as metaphysical, but it provides a framework whose internal boundaries may be empirically determined. It is a matter of ethnography how many 'pieces' each culture puts upon the moral board and what particular discriminations it makes between them. This variability will be increased by virtue of the intersection of the two primary articulations I have discussed by other conceptual structures.

I stress the empirical nature of the task of determining the articulation of moral space because much of the work already done which has not made explicit the types of considerations I have discussed here have fallen very far short of the required standard. One source of this failure is undoubtedly the enormous influence of Evans-Pritchard's brilliant Zande study on subsequent studies. Yet the Zande is only one culture and there is no need to make their cultural configuration a model for other societies; we must not simply assume that features of their belief system will be found elsewhere, for this allows the Zande monograph to dominate our thinking. Thus, if in one culture we have a major distinction between 'witch' and 'sorcerer' which is concordant with 'psychic power'/'use of objects', 'unknowing'/'conscious', and so on, we must not simply assume this pattern will be replicated elsewhere, but must, by detailed study, attempt to compose the conceptual structure of other cultures. That is, we must take each case as it comes. The few excellent monographs we possess unfortunately tend to act as structures into which other fieldworkers can without real thought slot their data: there is no telling how much we have lost in this process. And, further, it would be an error in any case to isolate the pair 'witch'/'sorcerer' where this distinction does exist, for these two categories and the nature of the opposition between them get their sense only from the full system of moral categories. Another caution is also in order in view of the possibility of our history providing the category 'witchcraft' and so allowing the transmission of a whole host of cultural terms for descriptive purposes. Many have expressed the 'witch'/'sorcerer' opposition in terms of the notions of 'spirit' or 'psychic' as opposed to 'material object'. In view of the complex theological history behind the term 'spirit' and the detritus of so many scientific epochs which has gone into our word 'matter', it may be wondered how legitimately these terms may be foisted onto other cultures. We can never be sure exactly how odd our own categories of thought are.

One advantage of using the model of a moral field is that it allows an empirical approach to internal articulation; another gain perhaps arises in considering the 'range' of the moral space. Above, I marshalled a set of moral pieces - 'witch', 'sorcerer', 'diviner', and so on whose analogues have been reported for many cultures. But it may be that the alien nature of these perons to our own society will lead us intuitively to close off this area and thus to misrepresent badly the conceptual structure of other cultures, With the idea of person categories, it is obvious that we can proceed from those already stated to embrace 'king', 'mediator', 'chief', and many others. To say that we had passed from 'magical beliefs' to the realm of 'politics' would be inappropriate for the view of an articulated moral space will enable us to eliminate such ethnocentric terms by focussing upon the culture's own constitution of moral space. For instance, among the Safwa (Harwood: 29, 137-8) it is claimed that 'witchcraft' and 'sorcery' operate in two types of relationships - those of affinity (transactional) and those of descent (incorporative). Here one might be tempted to speak of two domains 'kinship' and 'mystical beliefs' but in fact we have two refractions of one larger system.

In relation to 'politics' this ought to be well known. Coomaraswamy, for example, presents the Indian theory of government as an instance of the union of contrary principles. 'King' and 'priest' are associated with a whole series of conceptual distinctions, and government itself, what one might have been tempted to isolate as a 'political' sphere is in fact merely one expression of a total ideological scheme.[14] Hopefully the perspective I have here advocated for the dissolution of 'witchcraft' may contribute to a much larger reshaping of anthropology. The general point is that the particular lines of division within social science departments in English universities do not necessarily provide the appropriate schemes for segmenting other cultures. The remark is obvious, though the chapter headings of our text books seem to deny it.

* * *

I have now sketched the framework of an articulated moral space, and have briefly made some points about its value and use. In the general context of searching for relations between social anthropology, language and philosophy, I should now like to make several more suggestions. They do not follow from the 'moral space', and they are independent of one another.

Winch, in 'Understanding a Primitive Society' develops usefully several notions to be found in the later work of Wittgenstein. We might note (a) the close relationship between action and concepts, since concepts express our interests, and (b) that the task of understanding requires not only grasping rules but also realizing the point of the rules. Or perhaps in Hampshire's words: 'we have to explain types of discourse by reference to the institutions and forms of social life with which they are associated'. (1970: 14)[15]

Evans-Pritchard's Zande book, written partly as a critique of Lévy-Bruhl, has become prominent in what has been called the 'rationality' debate. It is cited in connection with such issues as coherence and falsifiability and used in discussion concerning the nature of science. This would be the subject for a separate paper. All I wish to say here is that through the exchange, science itself seems to grow more like a primitive system of beliefs , for out of inductivism, logical positivism, Popperism, and the view of scientific change associated with Kuhn, our ideas of 'fact', 'evidence' and what it is to 'falsify',

and so on, have grown more mysterious. The wide disagreements within the philosophy of science are themselves significant. Furthermore, there can be no total doubt in science, because certain propositions must be held indubitable in order to possess the language to formulate objections to others: talk of 'secondary elaborations' and 'circularity' in the context of Zande beliefs ought not to obliterate those conceptual features which scientific systems share with all products of human thought. There is, unfortunately, a considerable reluctance to follow up those connections sufficiently. As Habermas has argued (1972: 67) positivism has destroyed epistemology, and science has achieved a scientistic self-understanding which explores methodology but protects science from genuine philosophic scrutiny. The problem of knowledge is no longer raised because what is knowledge is defined by the existence and achievements of science itself.

The idea I wish to develop from Winch is his stress upon 'the point' of the rules, for there is an obvious way in which Zande moral notions are 'social' in which scientific systems are not: namely, the relations between thought and action are different. Zande moral notions are clearly intimately related to the evaluation of action, and perhaps it is in considering the relation of knowledge to interest that we can grasp certain features of Zande thought. We know that Zande moral notions have a practical point, also that the system is not really coherent, because there are questions an anthropologist could raise which would have no interest for the Zande. That is, the anthropologist could reveal 'conceptual synapses', beliefs which are not brought together; essentially, questions that are not asked. These problems are not real to the Zande because of the point of the rules, because the relation their moral notions have with action deters them from pushing their beliefs to their logical conclusions. Thus Zande contend that witchcraft is hereditary and yet punish an individual witch. But this is a conceptual feature of other systems of notions, so closely related to social life. Thus, in our culture, our psychology tells us how much we are a product of circumstance, of the experiences of early childhood and perhaps of heritable traits, and yet the law punishes a culpable individual.[16] Our own law, then, operates significantly by not following up certain causal relationships, and by not asking certain questions.[17]

A second idea relating anthropology and language which might prove seminal is this, though we shall certainly here find our lack of competence in technical linguistics an embarrassment. Hare has argued that religious utterances fall somewhere between ethical discourse and scientific assertions. Provided we do not make our language games self-sufficient (accounting for diversity at the price of untranslatability) the idea of domains of discourse may prove useful. In English, for instance, we have terms like 'good', 'right' and so on, which do not behave like 'big' or 'red'. We have a domain of ethical terms and a subject called specifically 'moral' philosophy, and some would argue the 'naturalistic fallacy' as a boundary marking the fact that moral terms cannot be translated into natural terms such as 'effective', 'useful' and so on. We have no reason to think that other cultures will lack domains of discourse, and there may be linguistic markers for them. By the behaviour of words, then, we may be able to spot, let us say, a 'moral logic' in other cultures, which is to be recognised as a specific domain, just as in English we may not simply recast moral assertions as scientific propositions. And this view of a complex of domains of discourse, to be generated empirically, may help us with some of our methodological problems. For instance,

the 'rationality' debate, articulated as it has often been round stark contrasts such as pragmatic/expressive, liberal/symbolic, scientific/mystical, technique/ritual, might be reformulated or dispensed with. For if we replace these dualisms by overlapping styles of meaning, the task of understanding grows more difficult, but we might eliminate some falsely generated problems. Thus, if we can see all utterances as falling between the poles of pure cognitive meaning and pure emotive meaning (neither pole actually occupied by any utterance, and the space between delicately structured by a whole host of discriminations) then we shall perhaps avoid certain explanatory mistakes and conceptual errors.

For my last idea relating these problems in anthropology to language, I turn to the idea of the 'performative utterance' associated with the philosopher J. L. Austin. Ritual action is highly structured and in certain actions, the spell in magic, for instance, language may be central. Tambiah (1968)[18] has recently brought our attention to this subject, but it is with Finnegan's article explicitly on performatives that I shall begin. Her suggestion that such a perspective may be useful in understanding religion is possibly true, but her observation that 'doing things with words' applies well to the Limba view of speech, that in a pre-literate culture there is an acute awareness of the force of speech (illocutionary and perlocutionary aspects, to use some of Austin's terms) is surely very important. But it also reminds us that we know very little about other societies' beliefs concerning language. If we are to understand ritual, for instance, it will surely be crucial to know whether a culture possesses a whole system of beliefs concerning speech and action. There is no reason to assume primitive cultures lack a philosophy of speech, and if they are communication-minded it is reasonable to expect they will themselves look upon speech as a paradigm of social exchange. Unfortunately, in this most important area, if its interests are already set, we can not expect a great deal of help from sociolinguistics. Or it may be, as with other sciences, in a phase which only temporarily prevents the asking of the really significant problems.

A performative utterance, essentially, is one in which to speak is itself to perform an action, and not to state facts. Austin himself (1958, 1962) stressed that the distinction between constative and performative was not radical, but in witchcraft and other areas where belief and action involve the definition of situations and their reclasification we might perhaps gain something by looking at the performatory aspect. Thus, Lienhardt (1956: 327) says that in a sacrifice the Dinka create the situation which they name; that is, there is not here a statement of fact but the bringing about of a socially defined situation. In the Nuer ritual which severs ties which might be sufficiently close for relations to be incestuous we have a performance of clarification or redefinition. Would speech in these circumstances resemble the classical performative 'I name this ship x' which does not state a fact but which itself is the act of naming? Such a performative, as an action, is neither true nor false, but happy or unhappy.[19]

I am myself not sure of the value of the performative utterance as such; it might only deceptively solve problems. But Austin thought his distinction constative/performative would be absorbed into a general theory of speech acts, (see Searle: 1965, 1969) and this is probably the field of most interest. Exactly what contribution the work on speech acts will make towards a semantic theory is not clear. But perhaps in the elaboration of the theory of speech acts anthropologists

will have an important role to play, for much of supposedly descriptive 'ordinary language philosophy' has a highly intuitive and culture-bound quality. Work on 'speech acts' and 'domains of discourse' ought to be empirically based, and this will involve comparative research. After all, in view of the total experience of humanity, literate industrial cultures are statistically very odd. It is well to remember, as Macdonald puts it (1950) that cursing and casting spells are older uses of language than the making of dispassionate scientific statements. That broad view need not be lost when anthropologists turn on themselves and realise that the scientific use of language is in fact a good deal more complicated than most of its philosophers have assumed.

* * *

I have dealt in this paper with the specific problem of witchcraft, but clearly my polemic has revolved round some of the largest issues in social anthropology, including the nature of the discipline itself. Space forbids my carrying the argument into the territory of law and politics,[20] so I shall conclude with some very general remarks.

Fundamentally, two approaches to witchcraft, or two theories of kinship, involve two very basic views of what anthropology is or might be. It would be profoundly wrong to see the newer type of anthropology as able only to transform limited areas such as kinship and symbolism but forced to leave law, politics and so on unregenerate in the hands of social science. The real division does not come between the subjects anthropology studies, for the new style may apply to the whole territory: the division, rather, is between different anthropologists. Thus, we need not assume the present coexistence of growth areas and areas almost totally in the old style will be permanent. These latter areas simply require the attentions of new style anthropologists. My bibliography indicates where I think some of the important issues lie, and so where I think some help might be found: the high proportion of works by non-anthropologists is significant.

In discussing witchcraft I have actually been commending a whole style of anthropology. Some will have found the paper wholly unsavoury and will judge it metaphysical. So I ought to say that the philosophy which I have used here has, above all, retreated from ambition and has occupied itself with painstaking and minute conceptual investigations. I hope, therefore, to the extent that this paper is philosophical, that it will be seen as expressing a sense of complexity and misgiving, and not the reverse. It is the new style which envies the confidence of those who have sought or proclaimed a 'natural science' of society, functional laws, and the like. I have here eagerly sought in other disciplines for ideas which might enable us to advance to a minimal comprehension.

Needham (1970) envisaged social anthropology disintegrating, its fragments being swallowed by other disciplines. This might indeed happen, yet it is possible also that a judicious use of those other disciplines may allow sufficient transfusion for social anthropology to remain alive. Lévi-Strauss has invigorated the discipline by looking to language; there is perhaps still a lot of bold exploration to be done. This paper has given expression to doubt concerning the value of what we have already achieved; I cannot share the satisfaction of those who regard our results as so staggering that we can now abandon ambitious thought merely to polish up some minute region. The whole landscape may change if we do enough work on the foundations. This is to say, we must be humble enough to return to fundamentals. I

shall end by indicating three problems. Let us suggest that anthropology has as its object 'the social', has as its method the of translation, and has as its main problem the question of 'meaning'. Let us now state the obvious: we do not know what 'meaning' is; there is no science of translation; and we do not know what 'social' means. Perhaps we can entertain some hope, but I see no warrant for a sense of satisfaction.

Malcolm Crick

Notes

1. For a criticism of the historical work, see Keynes (1972).

2. This paper originally embraced two styles in the study of witchcraft, law and politics, which, for reasons of space, could not be printed here. The witchcraft section itself has been mercilessly pruned. The study originated in reflection on the possible ways in which linguistics and linguistic philosophy might aid us in rethinking some anthropological problems. I have not made a special study of witchcraft, and I shall not judge the paper as worthless if it merely serves to suggest ideas to others. The particular topic of witchcraft seemed appropriate, among other reasons, because the interest of Winch and MacIntyre in the work of Evans-Pritchard has been one way in which philosophy and our discipline have already been brought into some type of relationship. I am very conscious of the tentative and exploratory nature of this paper and can entertain that it will, like the Müller paper (Crick: 1972) soon appear to be thoroughly unsatisfactory. Others will perhaps feel that the type of theology and philosophy for which the Institute is by now infamous has its adherents in a younger generation. Many general intellectual debts will be evident, but I should be happy if this offering would remind others of the work of that fine anthropologist David Pocock who was my first tutor and to whom I owe a great deal.

3. For a fuller review of ASA 9 see Crick (1971); for 'castrations' of paradigmatic writings see Ardener (1971a).

4. The one exception, the article by S. F. Moore, is the [redacted] interesting in the volume. Gluckman here publishes his 1964-5 Marett lectures and opens by wrongly naming the dedicatee; Robert Ranulph Marett. I hope he will rectify his error.

5. I ought to emphasize that these caustic remarks concern sociology; sociologie inspired Evans-Pritchard and continues to stimulate those who are attracted to his style of anthropology.

6. Those who appreciate that Needham's work in 'kinship' has arisen as much from the work of Durkheim and Mauss on classification as from Lévi-Strauss' volume on elementary structures will realise there is no incoherence in Needham's position here as a kinship expert who denies the existence of kinship.

7. Similar considerations are involved in recent discussions of the word caste. See Dumont (1961) and Pitt-Rivers (1971).

8. I do not contend here that my remarks are absolutely alien to other writing on witchcraft, but by making certain problems explicit I hope to be able to start some fairly novel departures. I shall not in the paper elaborate all that might be said about the articulation of a 'moral space'. Fairly obviously there will be a hierarchy of articulations, and also discrepant structurings. Also, the notion 'space' itself is problematic. It might simply be that a picture of a semantic geometry holds us captive. Waismann's open-endedness is a recognisable advance on Frege's image of concepts as clearly bounded spaces, yet the very spatial imagery itself may be deceptive. From Wittgenstein's early mirror theory of meaning it looks as if he was bewitched by a spatial view of propositional structure corresponding to a real spatial structure. But perhaps the juxtaposition of 'meaning' and 'space' may prove seminal in this paper.

9. Anthropologists will find much of interest in Collingwood's Autobiography. His contention that in philosophy and science there is not just a succession of different answers to eternal problems, but that the problems themselves change has been taken up by Kuhn. His 'logic of questions and answers' has been discussed by Waismann. More importantly we should recall that Collingwood died in Oxford only a few years before Evans-Pritchard took up his chair here. If we look at his 1950 Marett lecture (his effective inaugural lecture) the version of history he there discusses in expressing his views on the nature of social anthropology is of the Collingwood variety. Collingwood in his work on aesthetics makes reference to the work of Evans-Pritchard; in the light of the logic of questions and answers it might be profitable to relate the Zande study of 1937 to some intellectual inheritance from Collingwood.

10. Douglas (1970) may be regarded as a contribution to the sociology of knowledge. It opens up an interesting field but I cannot see that Bernstein's dualism of elaborated and restricted codes is a very useful way in. The quality of ethnographical substantiation will also have to be higher.

11. Dr. Needham is currently working on the social organisation of sentiment. It is a sad reflection that anthropology seems to have left out most of the important problems.

12. A debt to Strawson will be evident here, though in my use of 'person' I should not like it to be thought that I am commenting directly upon his work.

13. Unfortunately there is no space here to discuss the sources of this idea of 'field'. The philosopher J. L. Austin used often to elicit complex structures from ordinary discourse by constituting a system of terms by working through a dictionary. The work of the German field semanticists has been commented on by Ullmann, Ardener (1971 b), Basilius, Bynon, Ohman, Spence and Waterman. (see also Cassirer: 1945). The 'systemic' aspects of Saussurean thought (valeur, constellation) are well known. I should briefly mention that I have not here challenged the notion of 'meaning as articulation' of which Lévi-Straussian structuralism is one version. Since we do not know what meaning is, it would seem wrong to discuss outright any approach simply because of a few defects. Within linguistics itself the possibility of a structural account of semantics is still an open issue, so I would be less critical of

Lévi-Strauss than some. There remains the possibility of going beyond the stark Euclidean nature of structuralism by refinement: I hope that ideas of the wortfeld and bedeutungsfeld may more adequately capture the complexity of a semantic space. (see Lienhardt: 1951, for a remarkably modern treatment in this context of 'apeth' and related Dinka notions).

14. See also the work of Hocart, who, incidentally, was looking towards language as a source of models. In the two books in the bibliography one can spot the idea of reconstruction deriving from nineteenth century Indo-European philology, and also the linguistic idea of breaking phenomena down to their simplest units. I have cited Coomaraswamy on Indian government, but Africanists need not feel they escape the force of these remarks. See Needham (1967) on 'complementary governance'. One of the more absurd aspects of the old style political anthropology is that it is the anthropologist himself who rips apart politics from religion or ritual. If he must then search for some 'functional glue' to relate ritual to the political system or to associate the political structure with religious ideas, he has only himself to blame.

15. My remarks on the relation between thought and action and, 'the point of the rules', cannot, without important loss, be translated into the functional language of sociology.

16. These remarks were extensively developed in the original paper as a style of anthropological thought on law to contrast with the older style content with 'jural sociology' or 'social control'. I can here only sketch some of the points I would have made. Many jurists have stressed that their investigations are 'practical' and not 'scientific'. The very legal discrimination conditions/ cause is itself to be related to practical interests. Anthropologists should investigate legal conceptual systems with Evans-Pritchard's idea of 'morally relevant cause' in mind, for Bacon's maxim 'in jure non remota causa, sed proxima spectatur' does nothing to suggest the very peculiar status of 'causation' in legal philosophy.

17. I cannot expand on this point here, but even when the inquiry involves such questions as 'mens rea' and so issues in the relationship between thought, knowledge and intent, action and responsibility, if a man can be declared a free agent then he becomes an isolated and culpable individual and for practical purposes is surrounded by a conceptual vacuum. Where the point of the rules is that people 'get done', then each man is an island complete in himself.

18. Malinowski's view of speech as action is an aspect of the pragmatism which pervaded all of his work. The idea of meaning as 'effect' in 'context' advocated in Coral Gardens and their Magic is grossly defective as a semantic theory. We might see his resort to child psychology to explain the 'magical power' of words as one manifestation of those nineteenth century assumptions which lie beneath much of his other work.

19. Perhaps the 'performative' illuminates some aspects of law. Thus, when a verdict is delivered, a jury does not state that a man committed x, it makes him guilty. For guilt is a social definition and a man may be guilty or not quite independently of whether he actually committed x. A verdict perhaps does not state a fact, but 'performs a definition'. And a verdict is reversible only by another legal performative utterance. (It should be added here that J.L. Austin and the jurist H.L.A. Hart had many discussions on philosophical and legal questions).

20. I cannot give here any indication of the details of this discussion, but it was articulated around the idea of two styles of anthropology. I have left in the bibliography some of the literature I had used in this discussion, which might indicate some of the issues raised. Some might think that transactionalism had already transformed politics, but I would urge them to read Gledhill (1971) for an excellent critique of anthropological 'game theory'. Admirers of Barth-Bailey anthropology are also required to assent to that conception of the 'social' to be found in Barth (1966). One could also absorb such seemingly unpromising areas as demography into the scheme of 'two styles'. (See Ardener 1962, 1972, 1973 for the new style.) Ideas such as 'folk-demography' or stressing the relationship of the classifying process to 'numbers' argues that a statistical flair is no substitute for intelligent thought. It does not oppose statistics, so is no justification for remaining numerically illiterate. On the other hand anthropologists by now should be aware of the possibilities of non-metrical precision. It is most unfortunate that it is only in the higher realms of mathematics that one realises that numbers themselves are conceptual systems. Since it concerns problems of system and coherence, the work done on Gödelian formally undecidable propositions might prove interesting to anthropologists. (See Gödel's Proof (1959) by E. Nagel & J.R. Newman.)

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Review Article

Language Use and Social Change, edited by W. H. Whiteley. Oxford University Press for the International African Institute, 1971. 406 pp., £3 net.

Language use is one thing, social change another. The title of this book faithfully conveys its ambitious aim, which is to characterise the theoretical and empirical problems of language use in areas of social change. Here are collected the 22 papers given at the Ninth International African Seminar at Dar Es Salaam in 1968, which had as its title 'Social Implications of Multilingualism in East Africa', and which was under the chairmanship of the late Professor W. H. Whiteley. Now that these papers are in print, however, they have received a new collective name - to suggest perhaps a generality of import, but also to fit the mould of such titles as 'Language in Culture and Society' (Hymes, 1964). The editor squarely identifies his book in the tradition of the socio-linguistic literature that began approximately around the time of Hymes' collection; suggestively he cordons off the papers that are 'General and Theoretical Studies' from those that are 'Empirical Studies within Africa'.

This book however is an attempt to stake out a special place for the phenomenon of the multilingual person or community. Lambert puts it well, albeit in extreme fashion.

> 'The bilingual child ... may well start life with the enormous advantage of having a more open, receptive mind about himself and other people, and he is likely to become especially sensitive to and wary of ethnocentricism. ...
> 'I believe that bicultural bilinguals ... and their children are ... the ones most likely to work out a new, non-ethnocentric mode of social intercourse which could be of universal significance.'

Maybe indeed the bilingual is the heuristic device par excellence for solving the world's problems; neverthless, multilingualism within the nation-state can itself create great problems that require the attention of the professional politician. As Mosha explains,

> '... the many problems ... relating to language ... must be solved effectively in order to give the processes of national development a chance to succeed by providing the developing nation with: (a) an adequate system of linguistic communication, national identification, and consciousness, and (b) a means to cultural unity.'

The scope of the enquiry, then, is very wide. Even the first phonemic sounds a baby utters carry their own social significance in such a situation. Everything about language, about language-and-culture, somehow matters, has to be taken into account. The disturbing result is, in this book, an extraordinary inconsistency on the part of the contributors to know what to put into the footnotes and what to leave in the main body of the text. How much more so the inconsistency concerning frames of reference, let alone some sort of theory. The crucial relationship between language and (a) and (b) is nowhere properly examined and elaborated. How and under what sort of circumstances does a language 'take off' into self-sustained growth? If, indeed, the job of the politician is to manipulate language usage and language

loyalty, then the duty of the sociolinguist is to distinguish carefully government propaganda from the fruits of his own researches. The studious evasion by 'applied linguistics' of such questions, and of the insights of theoretical linguistics, renders much of its work, as N. V. Smith says,[1] largely trivial.

In his Introduction to the volume Whiteley writes of language surveys that are 'concerned to establish the basic facts'; a body of facts, however cannot exist as a body, without a theory: the problem, then is rather how to provide a methodological frame for all the 'facts' that keep coming in, to establish a clear and consistent terminology which is so patently wanting in this book.[2] 'Sociolinguistics', writes Pride, 'studies the varied linguistic realisations of sociocultural meanings ...'; it studies the realisations, 'the facts', rather than the process of the 'unpacking' from the cultural idiom into language. Understandably, certain facts are more equal than others, depending upon one's point of view: a casual aside in one paper becomes the central theme in another, or, more pertinently, the other way about. Does a bilingual, for example, when in conversation with another bilingual (of the same two languages), choose one of his two possible languages out of any special reasons? Parkin's whole paper is devoted to analysing how language choice may be used to manipulate audience reaction at weak points in social structure (e.g. status differences at public meetings); Lambert in his article on the psychology of French-Canadian bilinguals, faintly acknowledges the possibility of manipulating audience reaction in a couple of passing comments. It is never clear to what extent respective authors are aware of shifting the emphasis, now on this, now on that. Language sensitivities, to take another example, are well-documented; Fishman's advice to the researcher runs nevertheless: 'If language issues are not particularly sensitive, he (the researcher) can ask directly by means of a census-type approach'. (How sensitive? Can such curt asides on central questions tell us anything?)

Once the researcher has collected his facts, it follows that linguistic variations correspond to a sociocultural meaning; hence instead of labelling them by any suitable algebraic notation one announces the language of power, the language of solidarity, informal language, transactional language, and so on. The assumption is that language is forever functionally specific, and manifestly functional at that; thus where 'parameters' fail to account for variation within a particular social setting or 'domain', a 'factor-analysis' is super-added, the notion of redundancy, it would seem, being out of the question. It would appear that sociolinguists have reacted too sharply to the position of the transformationalist grammarians: Postal's view[3] that 'There is no more reason for languages to change than there is for automobiles to add fins one year and remove them the next' derives from a view of language as a body of rules where the loss of a rule or the addition of a rule that would produce linguistic change is a formal but essentially a quite arbitrary matter. However the sociolinguistic converse is equally narrow - language is seen as so totally embedded in social reality that it cannot undergo change purely from factors within the system but only from systematic alternations between linguistic and sociolinguistic mechanisms.[4] By placing language back into society, total explicability, it is felt, is nearer to being achieved.

There is a good deal of information in the book on the relation between specific languages and specific cultures. Thus for example Barbara Neale begins her paper with the declaration: 'Any study of the Indian Community (in Nairobi) is an exercise in componential analysis, where language ... and other cultural characteristics are used to define

the individual....' Similarly Abdulaziz attributes 'its Bantu-based culture' as an 'extra-linguistic factor which has given Swahili its great assimilating power in East Africa.' He continues: 'The language is therefore not just a vehicular lingua franca. It is an important factor for establishing cultural, social and political values amongst its interlocutors'. Speaking English in Tanzania during the colonial era implied adoption of a 'Black European' mentality and the rejection of indigenous cultural values, we are told. Values are always difficult to handle, justify and define even at the best of times, but in the hands of the sociolinguists they have supreme explanatory power:

> '... present-day values in Kenya tend to polarise along the axes of modernity and authenticity, and the linguistic exponents of these are English and the local language respectively, with Swahili occupying an intermediate position, offering something of authenticity and something of modernity.' (Whiteley).

The difficulty of establishing absolute criteria to demarcate languages distinct from dialects means in practice that it is often the politician's stooge at the national Language Academy who decides which linguistic realisations match which sociocultural meanings; language is commonly used to manipulate the feelings of speakers in terms of their closeness with or distance from speakers of related languages or dialects: those men who 'standardise' languages demarcate, as Southall's article is at pains to show, linguistic boundaries and discontinuities that may never have existed previously. Terms like 'cultural vitality' and 'group's sense of identification' are the standard catchwords here. The total failure in Whiteley's book to distinguish the official view from the 'situation on the ground' is typified in the article by Joshua Fishman, who treats as his starting-point the variety of elite views on language problems; he bases a detailed typology on 'locally made (and unmade)' interpretations of 'perceived' national traditions without going into who makes the interpretations and how. Even however assuming that one is interested in the 'facts' rather than the process, if one wants to understand a language situation, elites are a bad place to begin: consider a comparison of what the Irish elite has to say about the importance of Gaelic with what the British elite has to say about the importance of Welsh. Curiously, though, Fishman does indicate in a footnote that there might be 'prolonged functional failures' of policies implemented by elites. It would be interesting to know how language would represent cultural values in such a situation. Neither the Basque nor the Breton case, to take two obvious examples from Europe, are however mentioned in the book.

Another way into the problem is to be found in the articles written by those contributors with educational interests. Lambert's views on the potential of bilinguals to move toward non-ethnocentric modes of behaviour have been quoted above; Southall, in a detailed article on cross-cultural semantic themes in East Africa - sociolinguistics of a very different kind from that found elsewhere in the book[5] - concludes that such themes are eminently suitable for use as a classroom device for teaching East Africans about themselves. Robinson, however, in an exposition in the tradition of Basil Bernstein, is yet another kind of sociolinguist: he rightly finds that by studying the varied linguistic realisations of sociocultural meanings one is doing little more than establishing correlational links between language and culture without examining how and why non-linguistic markers function alongside with comparable significance. Instead, Robinson shows how a 'restricted' code in language actually generates specific patterns of perception for a stable sub-culture within a society, and is hence functional rather than dysfunctional in society. Joan Maw, writing about her teaching experiences in Uganda, complains of the traditional

but misguided assumption implicit in educational policy in Africa that the indigenous languages are in the position of the 'restricted' code; in colonial times there was a functional purpose in confining the natives to knowledge of the 'restricted' code only - and hence Lugard's philosophy for Northern Nigeria, quoted by Mazrui: 'The premature teaching of (the) English (language) ... inevitably leads to utter disrespect for British and native ideals alike, and to a de-nationalised and disorganised population.' Such insights may well prove most valuable for a sociolinguistic contribution to social history. Ironically, though, we do not get in this book even some attempt to make correlations: we are left with cryptic comments like those of Fishman '... different patterns of dress, of diet, of recreation, and of education may coexist within one and the same speech community' without further elaboration. Similarly Gorman decides that ' ... a very marked shift in language behaviour takes place ... as children grow older', telling us nothing meanwhile on how this might correlate with non-linguistic political or social change. In this book language remains one thing, social change another.

This division of labour can prejudice the results of the linguistic analysis no less than it can the conclusions concerning the nature of cultural values. Abdulaziz, for example, shows in vulgar Marxist fashion that it was an 'egalitarian-centred interpretation of culture' for the 'broad masses of people in Tanzania' that in fact encouraged Swahili as against the 'small urban elite (reading and writing European languages) whose way of life may have little in common with the rest of the population'. Hence when he comes to describe English and Arabic loan-words in the language, he says of them that they are 'fully Swahilized in their phonological form', whereas we can see from the article by Mosha that linguistic assimilation of foreign loan-words is a complicated process about which it is hard to generalise. The article by Fulass makes a similar error, but in the opposite direction. Amharic, he tells us, has been obliged to borrow great numbers of foreign words that deal with 20th-century technological artifacts; these words are so numerous that they actually confuse rather than assist communication: 'The reader or listener is bewildered by his inability to understand texts or speeches in Amharic;' in a footnote he continues, 'This is not because of little education. Even university-educated speakers have such complaints.' Fulass proposes a competent Ethiopian body that would instead coin new words from existing Amharic roots. His comments and his proposal indicate, however, the common notion in this book of sociolinguist as political commentator rather than as abstract theorist. For the social change incumbent upon the importation of alien words and ideas necessarily involves the specialist purveyor and interpreter (it is a common fact that loan-words do not mean the same thing in their new linguistic home as they had done in the language from which they have been borrowed - hence the 'faux amis' and the occasional mistranslations even amongst professional politicians); and the linguistic exponent of this is the development of new linguistic varieties, or registers (can the average English native-speaker properly be said to 'understand' legal language, ostensibly in his own 'language'?[6]) The piece-meal approach of Fulass' analysis, which concentrates on lexis alone, cannot effectively convey what happens to language in moments of social change by merely broaching a list of foreign terms that somehow need to be 'assimilated' into the language. German newspapers, for example, contain quantities of loan-words from English that are rarely heard in conversation. Why? Fulass' view is that borrowing should only be 'seriously entertained' (by whom?) when 'certain elements of the cognitive, aesthetic, religious, philosophic, etc., aspects of the culture in which IL (languages of the industrialised nations) is

spoken'. It is almost as if he is excluding the possibility of social change when he writes (presumably without the German case in mind), 'We can now speak of the existence of a terminological - and conceptual - gap between the societies (sic) in which IL and OV are spoken.'

The scope of sociolinguistics has been identified by at least one writer (Dell Hymes) as the 'ethnography of speaking'[7]; that the phenomenon of the borrowing of foreign loan-words into a language cannot be comprehensively described by sole reference to face-to-face interaction however, is evident from the article by Mosha, who indicates that besides such motives as style, the need to differentiate within a semantic field, or the need to distinguish homonyms, there are other, social factors that activate it, such as membership of a given occupation, the prestige of the source language, and the extent of native literacy. Too close attention to speech and face-to-face interaction alone overlooks the role of channel, the mode in which language is transmitted (e.g. written or broadcast), which in turn has its registers (e.g. letter, newspaper or journal, TV or radio); Mosha notes that linguistic assimilation of foreign words can be tied up with the social pervasiveness of the denotator of the word in question. Ideally, then a sociolinguist should study both things, but in this book there is no case where this has been done.

Part of the problem is that between the flaps of this book are represented the views of people from widely divergent academic backgrounds who are merely gathered together in order to express themselves on the subject of language, and who do not identify themselves consistently as 'sociolinguists' - and this is true for most major collections of essays that are considered to be contributions to 'the field'; another part of the problem, which is concomitant with the first, is that concerning methodology there are, as admitted in the Introduction, 'gross differences'. This criticism could be muted somewhat were the contributors aiming themselves at formulating the most powerful generalisations, as hinted at in the editor's subdivision of the papers into general and theoretical studies on the one hand, and empirical studies within Africa on the other. However the case of multilingual Switzerland is not mentioned once in a book which purports to discuss the social implications of multilingualism, and terminological usages are so idiosyncratic as to confound rather than clarify. Thus for example Southall has 'single-language clusters' and Nida 'speech area', whatever they may be; the unexplained notion of language 'simplification' is used by Fishman and others as 'assisting' the spread of vehicular languages, and the expressions 'structure' and 'group' remain totally unexamined in the book, although many contributors seem happy to rest their hypotheses upon such shadowy entities. Hence the feeling referred to above, that much of such work cannot be anything but trivial.

In this connection the high value attached to the questionnaire as one of the most reliable discovery procedures is I think open to serious doubt. There are several problems involved. The first is that questionnaires ignore what Parkin calls folk assumptions in his article, or the difference between what people say and what people say they say (more on this point below); Gorman in fact recognises this problem but has no suggestions to make. The second problem is that within the book there is a large measure of disagreement as to what is actually to be put into the questionnaire: in Fishman's crude door-to-door language census there is the question 'Can you understand a conversation in English', whereas Berry puts in rather 'How well do you understand languageX?', and then follows this up with seven questions that refer to the use of X in seven different kinds of situation.

However even this latter more sophisticated type of questionnaire does not take into account a third problem, noted by Robinson, namely that different people see language (in general) as being useful for different things; hence children's answers to 'wh' questions (who, when, why, what, etc.) vary as to mode of answer and to amount and type of information offered; however Lambert's research, based on responses to texts read aloud, gives us no information as to their semantic content. A fourth objection rests upon doubt as to whether a child could actually answer Gorman's question:

> 'The child was asked to indicate which languages various members of his family could speak, write, read or understand and to rate their 'proficiency' in each mode of use along a four-point scale.'

Children, it will be seen, do not in a multilingual environment appear to understand much about adult language differences or allegiances, so it is hard to grasp what Gorman expects from such questioning. It seems, lastly, that Gorman implicitly recognises the limitations of the questionnaire method in his espousal of such 'other information considered to be relevant' as teachers' assessments of children's language attainments. Indeed, over-attention to the statistics involved, to the charts which are the end-product, plus the problems that surround sampling universes may well obscure the nature, or rather the description, of the language habits of bilinguals. The quantitative approach may be of use in epigraphy or in the study of medieval manuscripts, but for face-to-face interaction it hardly seems the most suitable technique.

The use by Parkin of the methods of social anthropology successfully shows that the Robinson-Bernstein elaborated-restricted code duet oversimplifies the nature of social stratification, particularly on the question of how much mobility there is between strata. The sociolinguist as political commentator must avail himself of such methods if Robinson is right when he says that 'any educational system controlled by the high-status groups will be designed to preserve the status-quo.' The situation with the methods of linguistics looks rather different, however: as Robinson points out, transformational analyses of 'elaborated' code users are probably not going to be able to predict validly the language capacity of 'restricted' code users. Curiously enough, Chomskyan techniques and terminology appear now and again in the book, but with some intriguing mutations (read 'mutilations'): Fishman and Cooper distinguish language proficiency from language usage in an attempt presumably to give their work the airs of Chomskyan respectability by paralleling the latter's competence and performance distinction, but how they can justify that 'reading' falls into the former category whereas speaking falls into the latter remains a mystery. Again, Gumperz and Hernandez describe some 'selection constraints' that operate in the speech of bilinguals who switch languages in mid-sentence, such that *he era regador (he was an irrigator) are ruled out as 'impossible'. Is this deep structure or surface structure? Are they distinguishing competence from performance? Can performance features, like slips of the tongue, interruptions, noise, etc. result in bilinguals uttering such 'impossible' sentences? One is left with the impression that the eclectic frames of reference dotted about the book satisfy the authors on the criterion of thoroughness, but in practice it is only misleading, counter-productive, and, again, trivial.

What, then, can be done? What can be done in order to avoid committing 'the field' to that sort of 'reality' where a valid contribution consists of (as in the case of one article in the book) the mere

arrangement on a chart of the various languages that elite families in the Cameroons used when visited by the sociolinguist at dinner-parties. To avoid being trivial it is necessary to develop a conceptual apparatus that can compare whole systems; it must be able to account for apparent negative cases, so that we cannot sympathise with Nida who found that the resurgence of the Guarani language in Paraguay had to be dubbed 'an apparent exception'. Sociolinguistics is still uncertain on the level of observational adequacy; it has made few steps in descriptive theory, let alone shown a preoccupation with universals; its handling of etics is at best awkward. The best parts of the book, however, are those that deal more or less with emic considerations, and it may be that emics per se could direct sociolinguistics at this stage more competently than other approaches.

The article by Mazrui starts with the question to what extent do members of a speech-community see their language as integral to cultural cohesion, in this case the 'Islamic languages' of Africa. Although Mazrui appears to believe that the latter languages are intellectually more advanced than their pagan neighbours' - begging the philosophical question whether language as a medium of communication can suitably convey the potent mysteries of religion - his discussion of Arab attitudes to their language is otherwise valuable. The Prophet was the divine ventriloquist in the holy Arabic tongue, hence Moslems believe in its total inimitability, and also that English, the language of the missionary schools in British Africa, enjoyed a similar status in Christianity - this is what prejudiced Moslems against learning English, retarding their 'involvement in this wave of modernity'.

This kind of analysis throws new light on the importance of the questionnaire that asks, 'Why do you want to learn language X?' If language figures prominently in a society's cultural goals, the answer to the latter question may well reflect a folk-sociolinguistics, as it were, namely how the speech-community sees itself in relation to the outer linguistic world, rather than reflecting any 'objective', absolute, etic considerations. Questionnaires may have to be treated as statements only about what people say they say, and not otherwise, as is generally the case in this volume. The analysis of foreign loan-word borrowings into a language (lexis is in general over-emphasised in this book, to the detriment of other elements of language) presupposes the notion of foreign-ness, but this is no absolute matter:

> 'Children from deep rural areas often do not realise before going to school that they are speaking or mixing up two different languages. This is due to absence of such socio-cultural correlates as would mark one form of speech with a particular racial or mother-tongue group. Moreover, tolerance to language shift and mixing is often high and involves the whole community. At school (however) there is the least tolerance to language shift, and children are at once made aware of the fact that there are two separate languages involved.' (Abdulaziz)

Here the notion of tolerance to language shift enters as a crucial variable; differences between languages do not necessarily exist as such but vary through space and time according to the demands of the cultural environment. In another example (taken from Alexandre's article), parents exercise their tolerance to language shift along the space dimension:

> '... The choice (of African vernacular or French) ... is, in many cases, fully conscious and motivated: when living in an African milieu parents make efforts to use French, when living in France to use the vernacular. In the first case they intend to train their children for maximal efficiency at school, in the second case they try to preserve their sense of national (tribal) identity while living abroad.'

The close mutual interaction between language and culture in the sense outlined above implies that statements about 'language purity', for example, have little meaning in themselves: Andrzejewski insists that the average Somali is a language purist who would demand the dismissal of a broadcaster who used too many loan-words; 24 pages later we find Mosha insisting that Muganda are not purists concerning their language. If this is so, then, language would appear to function differently in these two societies, to have different structural relationships with other cultural goals.

The article by Gumperz and Hernandez on the phenomenon of bilingual code-switching exhibits some of the features of functionalist socio-linguistics that I have been criticising. By starting out with the idea that a language shift serves functionally within a conversation, and by proceeding with the idea that (for two Mexican Americans in conversation) English is 'normal' and unmarked, they arrive at the point where they consider that the Spanish words used convey a 'social strategy'. Idiosyncrasy and momentary inclinations are ruled out, so the occasional Yiddish interjection in the speech of some American Jews is dubbed as a 'stylistic ethnic identity marker' - without any attempt to study the Jew's or the Spaniard's attitude to tolerance of language shift, but rather finding such utterances as deserving a label because they deviate from 'standard' speech. One wonders how they would handle foreign loan-words in early stages of assimilation. Their position, indeed, has all the evidence of being a one-to-one view of the relation between language and culture.[8]

Multilingualism, as the editor tells us at the end of his Introduction, preceded Westernisation in Africa, so it cannot be <u>the</u> critical factor in such aspects of social change as the discontinuity between generations that sees kin ties becoming shallower. It is not in fact clear from this book whether multilingualism as such is even a valid construct at all, particularly as the editor also feels that

> 'In the sense that functional specificity of language variants to particular domains or settings is a fact of social life anywhere, then the multilingual societies of Africa differ in degree but not in kind from monolingual societies.'

If this is so, where then to begin the analysis, if the conceptual apparatus needed for the enquiry into multilingualism is to require no special tools? Certainly not with prophecy - that is best left to the professional politician, though it seems the scholar is sorely tempted, as this book well attests. Nor with its converse back into time, with history; Polome's comment that 'The linguistic situation in Lubumbashi is a clear reflection of the historical growth of the town under the colonial regime' may well be true, but diachronic relations cannot help sociolinguistics much until its synchronic house can be put in order. Starting-points used to expedite the latter vary in the book within a wide range: Southall starts with cross-cultural semantic themes, Parkin with situations, Neale ethnic groups, (hence she is interested in 'language distribution'), Robinson linguistic codes, Fishman elites,

Lambert with psychological membership groups and sympathetic orientations, Criper and Ladefoged with levels of political administration. No-one, incidentally, has yet published a testing of Bernstein's hypothesis in Africa. This wide variety of academic language poses some problems of mutual intelligibility, since there seems little a priori agreement on what is a sociolinguistic statement; Alexandre confesses his work is based upon material culled from informants who, he says, told him what they thought he wanted to hear in order 'to humour my own prejudices'.

On the question of how to overcome the problems created by academic diversity in a field in which members from a number of disciplines are interested, but who somehow cannot agree with each other in such a way as to make it here impossible to compare results or produce wide generalisations or universals, it is worth drawing attention to the fact that in order to conceal the nature of many research projects as being merely attempts at the correlation of language and culture, 'the field' is justified post rem as being essentially 'interdisciplinary'. Fishman has some amazing proposals to collect people from different disciplines to contribute to this interdisciplinary subject, which,he says, no single discipline can describe adequately. Methodologically the virtue of such a many-sided approach would be that it would be able to indicate 'areas of interdisciplinary overlap as well as uniqueness'. Far, however, from showing signs, as Fishman would assure us, that it is suffering from an overdose of 'disciplinary redundancy', sociolinguistics, as represented in this book, is trying to pose a totally new question, different from the preoccupations of other disciplines. The borrowing of an idea or two from another discipline more often than not leads no further than to a ratification of one's own entrenched position rather than to an attempt to integrate them methodologically at a high level of abstract generalisation. Thus Bernstein has been quoted and used in this book, rather than tested or integrated. But at this stage in the development of sociolinguistics, use must consciously be made of the methodological premises of related fields: thus if Gumperz and Hernandez say 'Social structure, like syntax, aids in the interpretation of sentences' they are really required to follow the analogy through and see whether in fact social structure functions in the sentence in any fashion that would make sense to a linguist, quite apart, that is, from the need to develop ways of talking about social structure as one can about noun phrases and verbal complements. Indeed,can sociolinguistics provide us with a new type of phoneme?

How a new discipline comes to be born may well be a matter of gestation. In this sense this book is a valuable contributory seed, however the infant has already, prematurely, been named with the device of academic teknonymy which perhaps adds an insult to abortive injury. The umbilical cord is seen to be cut in the moment when authors begin only to quote each other and gradually to close themselves off from the intrusions of the outer academic world. This has already happened, which is what gives one the sense of the abortion. The parents are nonetheless easy to identify, and so it is saddening that Professor Whiteley was cut down so suddenly last summer that he did not live to see the offspring hopefully growing in the future into a creative maturity.

Jonathan Webber.

Notes

(1) See: N.V. Smith, Bulletin of the School of Oriental and African Studies, London, vol. XXXV, 3, 1972; p. 683.
(2) J.B. Pride, in J. Lyons (ed.), New Horizons in Linguistics, 1970; p. 301.
(3) P.M. Postal, Aspects of Phonological Theory, New York 1968, page 283; quoted in J. Gumperz and D. Hymes (eds.), Directions in Sociolinguistics, 1972 p. 516.
(4) Details of this theory of language change can be found in William Labov's article in Gumperz and Hymes (eds.), p. 516-38.
(5) I am thinking of the distinction made by Ardener in E.W. Ardener (ed.), Social Anthropology and Language, ASA vol. 10, Tavistock 1971; p. lxxvi - lxxvii.
(6) For the notion of register consult Halliday, McIntosh and Strevens, The linguistic Sciences and Language Teaching, London 1964, ch. 4; reprinted in Joshua A. Fishman (ed.), Readings in the Sociology of Language,Mouton 1968, pp. 139-169, especially pp 149-156.
(7) Hymes' original article on this is to be found in Gladwin and Sturtevant (eds.), Anthropology and Human Behaviour, Washington D.C., Anthropological Society of Washington 1962; reprinted in Fishman (ed.), p.99-138.
(8) Gumperz and Hernandez did however discover in the course of their research that in the Mexican-American case English was generally used to introduce new information, whereas Spanish provided 'stylistic embroidery to amplify the speaker's intent'. If generally valid, this could constitute a valuable sociolinguistic contribution to theoretical linguistics within the framework of the approach of Halliday to be found in his article in Lyons (ed.); p.141-65, esp. p. 143..

Social Anthropology and the Old Testament - Present, Past and Future

The Old Testament is a collection of books which can be studied from several angles. Apart from its obvious interest to theologians and historians of religion, it is the major source for knowledge about the ancient Hebrew language, ancient Hebrew history and law, and ancient Hebrew social life and institutions. Moreover, while in practice some scholars have become specialists in only one or two of the latter areas mentioned, it is obvious that the theologian or historian of religion cannot afford to ignore any of these areas of study.

Yet for all that the Old Testament is the major source for knowledge about the ancient Hebrews, its evidence is fragmentary to such a degree that it can often only be elucidated with the help of neighbouring disciplines, that is, by means of a comparative method. Biblical Hebrew, for example, represents only a small proportion of the Hebrew that was spoken and written between 1200 and 200 B.C. (the approximate range of Old Testament literature), and it has long been the practice for languages related to Hebrew to be used in the interpretation of passages whose meaning is obscure precisely because there is much about Biblical Hebrew that is not known. In the sphere of history, the Old Testament evidence is fragmentary because the Old Testament writers selected only certain events for theological comment and ignored the rest, or because the events were the subject of reinterpretation and re-fashioning in the light of ancient Israel's subsequent faith and worship. In this area, much help has been gained from our knowledge of ancient Near Eastern history.

The fragmentary nature of the witness of the Old Testament to the life of ancient Israel is the essential basis for understanding why, at various times in its history, Old Testament study has shown an interest in Social Anthropology. For while modern Social Anthropology has denied that its job is to reconstruct the history of man's social institutions and beliefs, this was certainly not true of those speculations and enquiries about man in community which were the necessary forerunners of Social Anthropology, and which I shall also designate as Social Anthropology for the sake of convenience in this essay. A discipline which claimed to be able to reconstruct the history of the mental, religious and social development of mankind was obviously very attractive to scholars studying as fragmentary a source as the Old Testament. On the other hand, the more Social Anthropology denied that its job was to make such reconstructions, the less attention was paid to it by Old Testament scholars. In what follows, I shall sketch briefly some of the important points of contact between Old Testament study and Social Anthropology, and I shall comment on the present state of relations between the two disciplines and suggest future possible developments.

The modern period of Old Testament study began roughly in the second half of the eighteenth century; and although scholars working prior to this period had shown an interest in Social Anthropology, the beginning of the modern period saw the first attempts to think carefully about methodology. The scholar most directly responsible for this was the Göttingen orientalist Johann David Michaelis (1717 - 1791). Michaelis was an avid reader of the accounts of travellers and the like in the Near East, as well as in areas including North America, and Mongolia. He was early convinced, however, that such accounts were

largely not the work of trained observers, and that a properly-trained expedition to the Near East would shed light on the Old Testament in a way that the usual accounts of travellers and missionaries did not. Accordingly, Michaelis urged the learned world of his day to mount a scholarly expedition to Arabia, and he was rewarded with success when King Frederick V of Denmark agreed to provide the necessary patronage and finance. The expedition set out for Arabia in 1761, and in the following year, Michaelis published one hundred questions which he had addressed to the expedition.[1]

The expedition consisted of five members - a professor of Oriental languages, a professor of Botany, a doctor, a painter and a surveyor. Its aims included the study of the flora of parts of Arabia, the study of Arabic dialects, and the observation of the customs and social life of Arabs in those parts of Arabia which were thought to have been most free from foreign influence. This latter aim would, it was hoped, be of particular value for understanding social life in Old Testament times. This is not the place to record the adventures of the expedition, which was characterised by fearful clashes of personality, and the tragic deaths of four out of the five participants. Only the surveyor, Carsten Niebuhr, survived to complete as much as he could of the expedition's work, but his achievement was remarkable. Whereas he might easily have been written off as the least scholarly member of the expedition and therefore the least fitted to bring its work to completion, it was he who was most ready to learn how to adapt to the alien conditions in which he found himself. While his two professorial colleagues were concerned to maintain a rivalry with each other, and a superiority over the other members of the party, and especially over the 'natives', Niebuhr gained sufficient knowledge of Arabic dialects (he had begun to study Arabic under Michaelis in Göttingen), and gained sufficient sympathy with informers to be able to elicit valuable information.[2]

The anthropological presuppositions underlying the expedition are obvious. First, there was the idea of the 'changeless desert' which could somehow preserve a people in a state of social equilibrium provided that there was no outside influence. Second, there was the notion that if Arab tribesmen could be found whose material culture resembled, for example, that of the Old Testament patriarchs (Abraham, Isaac, and Jacob, as described in Genesis), then inferences could also be made from the one to the other about social institutions, and even religious belief. These presuppositions have survived into modern Old Testament scholarship; but if, from the point of view of modern Social Anthropology, these presuppositions were highly questionable, Niebuhr, and before him Michaelis, already perceived something of the importance of what later came to be called fieldwork.

In the preface to one of his accounts of the expedition,[3] Niebuhr stressed that the tragic loss of life that had been experienced should not deter subsequent expeditions. Death had occurred because some of his colleagues had been reluctant to adopt the 'native' diet and way of life; they had wanted to live in western fashion in the east. Niebuhr further stressed the need not only to know the language and to win the confidence of informants, but to listen to them without any preconceived criticism drawn from the listener's own religious or cultural background.

In the wake of Niebuhr's successful work, there was renewed interest in materials from the east which might illuminate the Bible. Many accounts of travels in the east from before the time of the Danish-sponsored expedition were published in works such as Paulus's

Sammlung[4] and there was a re-publication in German of the book by L. d'Arvieux (1635 - 1702) Voyage fait par ordre du Roy Louis XIV dans la Palestine which had first appeared posthumously in 1717.[5] Arvieux's book, which originated from some twelve years spent in the Palestine area from 1653 - 1665, had been noticed by Michaelis, who had also recognised its value for the interpretation of the Old Testament. Question 58 of the questions addressed to the expedition had asked its members to check the accuracy of Arvieux, and Niebuhr had given a favourable reply. If Niebuhr gave approval to a book that went back to the mid-seventeenth century, but which was to influence the Old Testament research of the late eighteenth and early nineteenth centuries, perhaps its influence was excusable. But the same could not be said for many of the accounts that appeared in Paulus's Sammlung, some of them being accounts going back to the sixteenth century, and provided by missionaries and similar 'committed' observers. It would seem that the lessons pointed by Niebuhr about how best to obtain objective information were slow to be learnt. The anthropological theory underlying the whole enterprise was nowhere better expressed than in the preface to the new German edition of Arvieux. This book, said the editor, 'accurately portrays the customs of a people that has preserved the pastoral, nomadic way of life of its ancestors, Abraham, Isaac and Jacob in a pure form, and free from foreign customs'.[6]

In the second half of the eighteenth century there was quite a different, but nevertheless equally important, use of theories based on anthropology in the interpretation of the Old Testament. The fons et origo was once more Göttingen, where the classicist, Christian Gottlob Heyne (1729 - 1812) put forward a theory of mythopoeic thought. Basing himself on accounts such as Carver's Travels through the interior parts of North America, Heyne argued that Greek myths should be understood as the product of primitive, and thus earliest, man's attempt to understand and describe the workings of nature.[7] Heyne's theories were applied to the interpretation of Genesis 3 by the orientalist Johann Gottfried Eichhorn (1752-1827), himself a pupil of both Heyne and Michaelis.[8] According to Eichhorn, the story of the 'fall' of man in Genesis 3 was a genuine account of the experiences of the first man and woman.[9] They had lived in a garden, but had become aware of the dangers of a certain tree because animals died after eating its fruit. When a snake ate the fruit and suffered no harm, the man and the woman were encouraged to do likewise. The fruit was in fact poisonous, and it affected their physical constitution so that they became aware for the first time of physical passion. A thunder storm in the evening caused them to flee in terror from the garden. This is what had actually happened; the extant form of Genesis 3 with its presentation of the events in terms of the divine - the divine prohibition against eating the fruit, the divine expulsion from the garden and so on-derived from the way in which the mythopoeic thought of earliest man had perceived and described the events.

Eichhorn's exegesis of Genesis 3 is today a bizarre example of what could be done even in what I have called the modern period of Old Testament study. It does, however, represent the first positive attempt to de-mythologize the Bible. Not for the last time was the Old Testament interpreted on the basis of a theory of primitive mentality which in turn depended on the accounts by travellers of 'primitives'.

The next important methodological step in the relation between the Old Testament and Social Anthropology was not taken until the latter part of the nineteenth century, when, indeed, there developed

one of the few serious discussions among Old Testament scholars about anthropological method. The protagonists were on the one hand, scholars who took an evolutionary view of the development of social institutions and religion, backed up by Tylor's doctrine of survivals; and the historical, diffusionist scholars often referred to as the pan-Babylonians.

One of the first Old Testament scholars to attempt to demonstrate the evolutionist viewpoint was W. Robertson Smith in his book Kinship and Marriage in Early Arabia.[10] In this work, Robertson Smith argued that the earliest form of social life among the Semites was that of the unit bound together by common blood and identified with a totem animal or object; within each unit the matriarchal principle was dominant. The argument was backed up by an exemplary use (or misuse!) of the doctrine of survivals (Robertson Smith preferred the term 'relics') in which from the Old Testament point of view, texts were interpreted with complete disregard for their literary context, and a search was made of the Old Testament for every possible personal or group name which might be derived from an animal, and thus be evidence for the totemic theory.[11]

In Germany, J. Wellhausen searched ancient Arabic texts, especially those of the pre-Islamic period, for survivals of primitive Semitic religion,[12] and the efforts of both Wellhausen and Robertson Smith in the study were reinforced in the field by an American, Samuel Ives Curtiss. Curtiss believed that it was possible to find survivals of primitive Semitic religion in present-day (i.e. around 1900) Syria and Palestine, provided that certain criteria were applied. First, to be primitive, a religious belief or practice should be contrary to Christianity or Islam. Second, it should, if possible, be found in areas where both Christianity and Islam normally held sway. Third, it should correspond with what had been discovered from the ancient literary sources. Curtiss's field researches were embodied in a book entitled Primitive Semitic Religion Today[13], one of whose main assumptions was that religions like Christianity and Islam were merely a veneer spread thinly over peoples who in fact bore witness to the sort of religion practised by the ancestors of the Hebrews some 2,000 years before Christ. The findings of Robertson Smith, Wellhausen and Curtiss greatly influenced the reconstruction of the history of Israelite religion. The ancient Hebrews were commonly represented as passing through animism and polytheism to monotheism, and their social development was classified as first nomadic, then agriculturalist, and then urbanised.

This picture of ancient Hebrew life and religious belief was challenged by the pan-Babylonian school, as were the assumptions on which it rested.[14] The school, of course, arose from the publication from roughly 1870 onwards of the recently-discovered cuneiform texts from ancient Mesopotamia, texts which for the first time provided first-hand knowledge of ancient Assyria and Babylonia. The evolutionist school deliberately ignored these texts on the grounds that although they were undoubtedly ancient, they were not primitive. The Semitic culture of Mesopotamia had been built on the foundations of the earlier non-Semitic Sumerian culture, and it was preferable to use the much later 'purer' evidence from Arabia for the interpretation of the early parts of Old Testament than the 'impure' Mesopotamian evidence, even though the latter ante-dated or was contemporary with the earliest parts of the Old Testament.

The pan-Babylonians, on the other hand, argued that it was impossible to ignore the newly-discovered history of the ancient world in Old Testament times, especially as the Old Testament itself claimed that its forebears had originated from Mesopotamia. They challenged the notion of the changeless desert, which was so important a part of the evolutionary argument. The fact that the history of the Arabian desert was unknown did not mean that it had had no history; and the pan-Babylonians, on the basis of Assyrian texts, posited the existence of a powerful North Arabian kingdom of Musri, which had exerted influence on the Hebrews in ancient times. Further, the pan-Babylonians questioned the doctrine of survivals as it was used to reconstruct the primitive Semitic religion. Far from being survivals, the practices adduced by Curtiss and the others were degenerations from an advanced pattern of culture that had spread from Babylon in ancient times, and had affected the whole of the ancient Near East. It was a mistake to imagine that Christianity and Islam were the veneer spread over a folk religion which had its roots in primitive times. Christianity, Islam and Judaism had themselves arisen out of the remains of the ancient Babylonian culture pattern.[15]

In view of the fact that the doctrine of survivals for the purpose of historical reconstruction has been generally discredited in the present century, it would appear that the pan-Babylonians were closer to a sound methodology than their opponents; though presumably few today would accept their exaggerated theories of culture patterns and diffusion. In the eyes of Old Testament scholars, the pan-Babylonians took such extreme standpoints on other issues that their whole position was disregarded. For example, they adopted an astral/mythological view of the origin of ancient historical texts, a view which at its most extreme derived the Passion Narrative of the Gospels from the Epic of Gilamesh, which in turn was ultimately based on speculations about the sun and moon and other heavenly bodies. Again, subsequent research has shown that their claims about the ancient North Arabian kingdom of Musri were unfounded.

Perhaps in the debate between the evolutionists and the pan-Babylonians, the issues were too much polarised; but at least the issues were recognised. In subsequent Old Testament scholarship, there has been a tendency to ignore the methodological issues, and to have one's cake and eat it. Thus many scholars have recognised the importance of the Babylonian material and of setting the Old Testament in its historical background, yet they have continued to use the doctrine of survivals and to observe contemporary Bedouin in order to understand parts of the Old Testament. If they have used a cultural model, whether consciously or unconsciously, it has been one in which centres of civilisation like Babylon, Egypt, Ugarit and even the Palestinian city states are seen as having generated spheres of cultural influence, but these spheres did not cover the entire ancient Near East; there were numerous 'gaps', and in these gaps, peoples like the forbears of the ancient Hebrews lived, largely untouched by the higher cultures of the area, so that it remains legitimate to deduce social and religious facts about these people from later peoples such as bedouin Arab tribes who have similarly, so the theory would suppose, had minimal contact with more advanced culture.

Whether this model is an adequate one in the light of the evidence available, is perhaps something that cultural anthropologists could tell Old Testament specialists. What is certain is that more evidence relevant to the construction of a more adequate model if one be needed, can be expected to be forthcoming. The pan-Babylonians

may have been wrong about Musri, but North Arabia has yet to be systematically excavated, and if and when this can be done, the 'changeless desert' theory may have to retreat a little further.[16]

I now turn to the present century, and to the contemporary use by Old Testament study of material or theories derived from Social Anthropology, and this can perhaps be best done by making comments under several headings.

The doctrine of survivals

Although often clothed in sophisticated quasi-historical dress, the doctrine of survivals continues to be used for historial reconstruction. A noteworthy example is the 'God of the Fathers' theory of the German scholar Albrecht Alt.[17] Alt uses inscriptions found in various parts of Arabia, and dating from the centuries immediately preceeding the Christian era, to reconstruct the religion of Abraham, Isaac and Jacob, who are usually dated in the 18th century B.C. by Old Testament scholars. The method used supposes that the religion implied in the phrases 'the god of X' (X being a man's name) in the inscriptions, can be used to understand phrases such as 'the God of Abraham' in Genesis 28: 13. The biblical phrases are treated as survivals, and are removed completely from their context. Interpreted in the light of the much later inscriptions, they allow us to reconstruct the religion of the patriarchs. In fact, we know next to nothing about the people who wrote the inscriptions, and data which might support the comparison is almost wholly lacking.

Primitive mentality

Theories of primitive mentality or of mythopoeic thought derived directly or indirectly from Social Anthropology have been greatly influential in modern Old Testament Studies. Lévy-Bruhl's theories of pre-logical thought have become the main-stay of the theory of 'corporate personality' which was first advanced among Old Testament scholars by H. Wheeler Robinson. Although, as I have argued elsewhere,[18] the notion of 'corporate personality' as understood in Old Testament study is complex and ambiguous, and in some of its facets is not to be dismissed out of hand, the following quotation from Wheeler Robinson indicates its more questionable nature.

> 'There is a fluidity of conception, a possibility of swift transition from the one to the many, and vice versa, to which our thought and language have no real parallel. When we do honour today to the "Unknown Warrior", we can clearly distinguish between the particular soldier buried in the Abbey and the great multitude of whom we have consciously made him the representative. But that clearness of distinction would have been lacking to an earlier world, prior to the development of the modern sense of personality.'[19]

In a different connection, Old Testament scholarship has used a theory based on Cassirer's interpretation of largely pre-fieldwork evidence in Kantian epistemological terms. The position expounded in the second volume of Cassirer's Philosophy of Symbolic Forms was taken up in the symposium of essays entitled Before Philosophy.[20] These essays, which describe the thought of the ancient Near East,

have as their theme the view that in the ancient Near East, man did not experience the phenomenal world as an 'it' but as a 'thou'; or to use theological jargon, what was believed to be the divine was always experienced as immanent in nature and never as something transcendent. Curiously enough, Old Testament scholars have used this theory about how ancient Near Eastern man experienced the world of nature, not to explain, but rather as a foil to the ancient Hebrews. It has been argued that the Hebrews experienced the world of nature in a quite different way from their neighbours, and that in this lies their uniqueness.[21] But in the whole operation, there has been a good deal of confusion between epistemology and psychology, and it has not been satisfactorily explained how the mental processes of the Hebrews came to be so different from those of their neighbours, quite apart from the questions of whether Cassirer's position is tenable and can be applied to the ancient Near East.

It is impossible to discuss primitive mentality without mentioning the influence of Frazer's theories of magic in Old Testament study. These are still widely held, in their most crudely causative form, by many Old Testament scholars. The latter are largely unaware of the recent emphasis by Social Anthropologists on the symbolic and expressive aspects of magic, nor has the obvious question been asked as to how life would have been possible if ancient peoples thought that like was affecting like all the time. Closely allied to causative views of magic has been the stress on the ritual theory of myth, and the magical (i.e. causative) function of myth and ritual performance. In this connection, diffusionism has also been strong. Certain schools of Old Testament scholarship have argued that in ancient Babylon, myth and ritual (magical) rites were performed, and that this must also have been true for the cities of ancient Israel by diffusion of a Babylonian culture pattern.

Terminology for social units.

If people know nothing else about the Old Testament, they know that there were once twelve tribes of Israel. But what is a tribe? This is a question which, as I understand it, could not be easily answered by anthropologists; and the truth of the matter probably is that the term tribe has been applied to phenomena of such complexity in the history of anthropology, that wrong comparisons have been made, and that a much more sophisticated terminology is required.[22] Old Testament scholars seem to be much more confident than social anthropologists that they know what tribes and clans are, and the scholarly literature abounds with attempts to reconstruct the history of the tribes before their settlement in ancient Palestine, in spite of the fact that it is also widely conceded that in one sense many of the groups did not become tribes until after they had become settled. A further common mistake made in Old Testament study is to confuse the classifications and descriptions of social structure that would be made by a trained observer, with the terms for social structure used among the people observed. Thus, it is usually accepted that in order to understand ancient Hebrew social structure, all one has to do is to analyse the relevant Hebrew vocabulary, in spite of the fact that when this is done, a good deal of inconsistency and overlapping is found. It then often happens that a scholar puts forward a consistent scheme for interpreting the data, said to come from an 'early' period of ancient Israel's life, and the inconsistencies are then explained in terms of development or breaking down of the 'earlier' system.

I think that enough has now been said about the modern position of the use by Old Testament scholars of data from .ocial Anthropology. At this point, having been critical by implication about my colleagues and fellow-workers, I feel that I must come to their defence. If the picture that I have presented strikes the Social anthropologist as appalling, I hope that he will allow that the demands made on the Old Testament scholar are in fact enormous, involving as they do the mastery of several ancient Semitic languages, the classical languages and modern languages, not to mention Theology and Ancient History. Further, current Old Testament study is based on foundations that go back a long way, and the amount of reading to be done to become acquainted with the discipline as such is daunting. However, if I defend my colleagues, I do not necessarily excuse them. Whether we like it or not, we are going to have to recognise that in areas in which Old Testament study impinges on Social Anthropology, enough is going to have to be known about the latter by some Old Testament specialists to prevent false models from being employed. If Old Testament scholarship has to become even more fragmented and specialised within itself, this will be an inevitable outcome of the advance in knowledge.

This brings me briefly to the future. The most interesting thing about the future is that recently, one or two social anthropologists have shown an interest in the Old Testament. One thinks particularly of Professor Mary Douglas[23] and Professor Edmund Leach.[24] From the Old Testament angle, Douglas's contribution has been the more helpful, because she has done her Old Testament homework more thoroughly than Leach, although the latter's contributions are always stimulating, if nothing else.[25] If social anthropologists wish to write further about the Old Testament, it is very much to be hoped that they will seek the ready cooperation which would undoubtedly come from the Old Testament side.

In the opposite direction, there is, of course, much to be done by Old Testament scholars themselves. For example, they are best placed to examine the history of their discipline, and to expose the anthropological assumptions on which it is based. There will in future, however, be much for Old Testament scholars to learn from social anthropologists about such subjects as magic, ritual, myth and sacrifice. Also, there is a desperate need for an expert in kinship systems and cultural anthropology to examine, together with an Old Testament scholar, the kinship systems of the Old Testament, as well as the models used for the general interpretation of the life of the ancient Hebrews in their historical and cultural setting. It would be of importance if such an investigation discovered that in fact the evidence was insufficient to allow any firm conclusions to be drawn. After all, Evans-Pritchard has defined the task of Social Anthropology as the study 'of social behaviour, generally in institutionalized forms .... either in contemporaneous societies or in historical societies for which there is adequate information of the kind to make such studies feasible.'[26] It would do no harm, and immense good to Old Testament study to know more clearly, if necessary, the limits of what it can know about the ancient Hebrews.

John Rogerson

Notes

1. J. D. Michaelis Fragen an eine Gesellschaft gelehrter Männer die auf Befehl Ihro Majestät des Königes von Dännemark nach Arabien reisen. Frankfurt 1762.
2. The fullest account of the expedition in English is that by Thorkild Hansen Arabia Felix, London 1964. It is also treated in D. G. Hogarth The Penetration of Arabia, London 1905.
3. Carsten Niebuhr Beschreibung von Arabien Copenhagen 1772.
4. H. E. G. Paulus Sammlung der merkwürdigsten Reisen in den Orient 7 vols. Jena 1792 - 1803.
5. E. F. K. Rosenmüller Die Sitten der Beduinen-Araber. Aus dem französischen des Ritters Arvieux, Leipzig 1789.
6. Rosenmüller's edition of Arvieux p. ix.
7. C. G. Heyne Opuscula Academica Göttingen 1779, especially Vol. I, pp. 184-206; Vol. III pp. 1-30. The title of the essay in Vol. III is 'Vita antiquissimorum hominum, Graeciae maxime, ex ferorum et barbarorum populorum comparatione illustrata'.
8. J. G. Eichhorn in Repertorium für Biblische und Morgenländische Litteratur IV 1779.
9. Biblical scholars of this period were familiar with polygenism theories, but rejected them in favour of monogenism.
10. 1st edition, Cambridge 1885.
11. cp. the review of Kinship and Marriage by Th. Nöldeke in Zeitschrift der Deutschen Morgenländischen Gesellschaft 40, 1886, pp. 148-187. Nöldeke expressed extreme hesitation about the use of the comparative method for the purposes of historical reconstruction, and also criticised Robertson Smith for using studies of 'primitives' in order to explain the Old Testament and ancient Arabic sources.
12. cp. the title of his book Reste arabischen Heidentums, Berlin (2nd ed.) 1927.
13. Chicago 1902; German edition Ursemitische Religion im Volksleben des heutigen Orients. Leipzig 1903.
14. For a brief account of the pan-Babylonian school, see H-J. Krauss Geschichte der historisch - Kritischen Erforschung des Alten Testaments, Neukirchen-Vluyn (2nd ed.) 1969, pp. 305 ff.
15. The most succinct presentation of the pan-Babylonian arguments can be found in H. Winckler Religionsgeschichtler und geschichtlicher Orient, Leipzig 1906.
16. The results of the most recent exploration of North Arabia are recorded in F. V. Winnett and W. L. Reed Ancient Records from North Arabia, Toronto 1970. The expedition was allowed to make surface explorations for one month in northern areas of Saudi Arabia.
17. A. Alt 'Der Gott der Väter' in Beiträge zur Wissenschaft vom Alten und Neuen Testament Stuttgart 1929; reprinted in Kleine Schriften Vol. I Munich 1959 pp. 1-78.
18. See my article 'The Hebrew Conception of Corporate Personality: a Re-examination' in Journal of Theological Studies XXI 1970 pp. 1-16.
19. H. Wheeler Robinson The Cross in the Old Testament London 1955 p. 77.
20. H. Frankfort (ed.) The Intellectual Adventure of Ancient Man Chicago 1946; British edition Before Philosophy Harmondsworth 1949.
21. For an example of this sort of argument see G. E. Wright The Old Testament against its Environment London 1950.
22. See the symposium edited by June Helm Essays on the Problem of Tribe Washington 1968, where it is argued, among other things, that the word 'tribe' has lacked sociological rigour from the outset, and that it is an example of a technical term taken over from a pre-scientific period of scholarship.
23. For example in Purity and Danger. London 1966.
24. For example in Genesis as Myth and Other Essays. London 1969.
25. See my article 'Structural Anthropology and the Old Testament' in Bulletin of the School of Oriental and African Studies University of London XXXIII 1970. pp. 490-500.
26. E.E. Evans-Pritchard Social Anthropology. London 1951. p. 5.

Book Reviews

Perspectives on Nomadism W. Irons and N. Dyson-Hudson (eds), Brill 1972; and 'Comparative Studies of Nomadism and Pastoralism', Anthropological Quarterly, Special Issue Vol. 44, No. 3, 1971.

A move seems to be afoot to establish 'nomadism' as an important discipline of its own. For too long, say some, anthropologists have thought of nomadism in a paternal manner as that esoteric offspring of geography and ecology dealing with desert herders and their tents, and of course the unique relationship between these people and their harsh, harsh environment. The problem is that other anthropologists, say these same malcontents, realise that the age of environmental determinism is past but wish it wasn't when looking at sun-burned desert-dwellers. It would be much more comfortable to be able to explain away coincidental cultural phenomena in terms of environmental adaptation when that environment is of so obvious an importance, than to search for other formative interrelations in the social milieu. Of course this does not work when the forces of comparative social studies take the field, and so attempts are made to show/causation can now be left on one side and behaviour patterns can replace it. Johnson for example (The Nature of Nomadism, 1969, Chicago) provides a very typical old fashioned structure based upon movement patterns, but it fits into the game played by so many before (Bacon, Patai, Krader, etc.) as to who are nomads and what are the common characteristics that allow us to use this special category in any useful way.

Today, anthropologists working in areas where the harshness of terrain, coupled with a seemingly arid cultural heritage, and where characteristics such as movement and herding are common factors, seem to feel that a framework of reference unique to these areas is essential. Possibly a compensation deemed necessary to replace what might appear to be the richer cultures in other parts of the earth. It is reminiscent of the situation analysed by Barth for the Basseri nomads of south Persia. He found no overt evidence of ritual behaviour, felt this augered against the structure of this kind of society, and interpreted the rigid timings and changes in the movement pattern as a substitute for the gap in the Basseri social model.

A spate of justificatory symposia, essays and books about nomads has recently been released upon an unsuspecting anthropological world. To those working in related areas a proportion of these studies are welcome. To others, they have a somewhat embarrassing 'justify the field' stress that seems a long way behind the analyses in most of modern social anthropology. That there is likely to be some relationship between the physio-biotic environment and the socio-cultural organisation of a group comes as no surprise to anyone. Neither does the idea that "social, political and cultural factors in the environment are often the determinants of adaptation" (Salzman, A.Q. intro.) and as most of the articles in the collections under review indicate, a balance of the two is the most obvious and certainly the least startling sociological fact brought out in these works. Salzman believes that the physio-biotic environment is only a secondary factor in a process of adaptation and he cries out for substantive generalisations, presumably to place the study of 'nomadism' on the anthropological map. But anthropological theory in the 20th century has not been geared to 'non-nomadic' societies - it was probably a

great mistake for Barth to call his work Nomads of South Persia for it seems to have given an identity to the student of the arid-land pastoral nomad that was not thought necessary before.

The two most recent important works on so-called nomadic societies are those edited by Salzman ('Comparative Studies of Nomadism and Pastoralism') and by Irons and Dyson-Hudson (Perspectives on Nomadism). Both of these are based upon successive symposia attended by almost the same people and can really be considered companion works. They contain many excellent individual essays, but it is for their contribution to area and group studies that they will remain important. It is interesting to observe the different ideals of the two writers in their respective introductions. Salzman, as already mentioned, is after substantive-generalizations; comparative studies that use the material already available in the ethnography. Dyson-Hudson, on the other hand, asks for realism, behaviouralism and detail. It is just because of the fragmentary data of the fifties and earlier that attempts to categorize, classify and homogenize nomadic societies have been so weak. This is exemplified in the articles where the author feels an obligation to pop in a semantic statement to avoid the condemnation due for misuse of the words 'nomadism', 'sedentary', or even 'pastoral'. Which returns us to the urgency felt by these writers for a theoretical framework different to those acceptable to other anthropologists. It is most enlightening to look through the bibliographies appended to the essays and to see with one or two exceptions, the dominance of Barth (used by everyone except Nada Dyson-Hudson) and the extraordinary lack of any other theoretical material. This again seems to be a reflection of the fear of the 'nomadists' of not being recognised as mainstream anthropology, but it is just that which makes this esoteric group so vulnerable, even though there is such a wealth of material in their work.

When the essential pastoral-farmer balance is discussed (Horowitz, Spooner, Bates) or the demographic-environment balance (Irons, Paine, W. Swidler), it is refreshing that it is done not as a means of establishing the identity of the nomadic group but rather as a use of variables and the relationships between them. Most writers have left Ibn Khaldun back in the 14th century and aren't too worried about the 'image' of the nomad and peasant (noble, free wanderer as contrasted with the oppressed, inferior farmer).

Finally, a word of praise for the introductory essay by Dyson-Hudson in Perspectives on Nomadism entitled 'The Study of Nomads'. It is one of the most useful, careful analyses of the contents of a book by its editor I have ever read. What is particularly good is the honest manner in which Dyson-Hudson looks at the symposium material. There is no attempt at conciliation between the authors and himself and an intellectual setting is established for the volume as a whole.

Both these volumes are important for anthropologists whether 'their people' migrate, live in tents, have herds, live in a semi-arid environment or not, for it is the fundamental problem of how to approach other cultures that is under discussion.

André Singer

Words about God. Ian T. Ramsey (ed), 1971. S C M Press Ltd. £1.50 paperback.

A selection of readings which begins with the Early Christian Fathers, Plotinus, Moses Maimonides and Aquinas, and ends with extracts from the famous fifth chapter of Nuer Religion, must be of considerable interest to anthropologists. One of Ramsey's special concerns is to show some of the ways in which the 'narrow' empiricism of the earlier decades of this century has 'broadened' into a form more amenable for a true understanding of religion. In 1946, Russell distinguished between 'knowledge by acquaintance' and 'knowledge by description'. We see here, albeit in an adumbrated form, the idea that there is a hierarchy of languages. There is, however, no recognition that anyone might be interested in understanding religion: 'every proposition which we can understand must be composed wholly of constituents with which we are acquainted', when acquaintance involves direct sense data of the type religion cannot afford. The extract from Ayer clearly shows the poverty of logical positivism when applied to religion.

According to Ramsey, Russell's 1948 work marks a break with crude 'physical realism'. Developing his theory of types, the conception of a hierarchy of languages now bears the message that meaningful language is not a totally homogeneous mass but is logically variegated. From this it is but a short step to the reading extracted from Waismann's paper 'Language Strata'. The verificationist theory of meaning used by Russell in 1946 is no longer in evidence; words like 'meaning', 'truth', 'verification' and even 'logic' are taken to be context-dependent, which entails that there is no sharp divide between meaning and non-sense.

Waismann, of course, makes good reading for Wittgensteinian fideists or Winchian-styled anthropologists. A true-one is inclined to say 'religious'-understanding of religion can now be imagined within the confines of empiricism. It is possible, as the extracts from Ramsey and Evans indicate, to be a philosopher of religion and a believer. However, Words about God has been compiled not so much to make this well-known point as to suggest the scope of the empiricist tools which are now available for those whose job it is to translate, interpret and characterise religious discourse and modes of 'thought'.

Let us approach this from the other side. To the best of my knowledge, anthropologists have not developed many tools of a comparable type. Such distinctions we have- magic/religion, age set/age grade, sorcery/witchcraft, preferential/perscriptive, metaphor/metyonymy- either belong to a lower order of things or are involved in analyses which rest on a prior understanding of the relevant phenomena. Since we have to begin with what participants have to say, it seems reasonable to suggest that it might be just as well to start catching up on lost time. This is where Ramsey's collection comes in: a set of tools which begin at the beginning with participant discourse.

The core of modern empiricism is relativistic: statements are construed as belonging to different logical styles according to context, how they are used, etc. Waismann, in the extract mentioned, asks how such styles can be characterised from within. This leads him to examine types of ambiguity - including the logic of metaphor - and the fascinating question of whether the fact that 'the law of excluded middle' cannot be readily applied to aphorisms, poetry and mysticism renders these modes of discourse illogical. Think, in this connection,

of Lévy-Bruhl, Firth, and the many others who construe religious talk as 'non-rational'.

Ryle attends to a closely related topic, 'systematically misleading expressions'. The grammatical or verbal form of an assertion, he argues, is not a sure guide to the logical form of the same assertion and indeed can be positively misleading. Attention to the logic of category mistakes, in the sense that inferences drawn from verbal form easily encourage inaccurate equations and assumptions, could well help anthropologists develop their own logic of questions. Is it, for instance, a category mistake to ask, 'well, don Juan, did I really fly'?, or 'what is the magical power of words'?

The extracts from Austin also deal with logical landscapes, only this time not with logicality as such, or questions, but the issue of how words are used. His distinction between performative and constative aspects of utterances, utterances which 'do' and those which 'say', might not appear to be of much anthropological interest. Personally, I do not think that Austin can provide us with a general solution to the problem of magic, but there remains Finnegan's application. More generally, the notion 'performative' allows us to take a new look at a traditional anthropological insight which goes back at least to Kant's 'regulative' view of religion: religious systems and to some extent magic and witchcraft beliefs can be read in terms of the logic of moral discourse. Although we find no mention of the notion 'performative', the extracts from Mansel, Hare and Hepburn all concern this point of view. Hepburn, for instance, argues that the historicity, even ontological truth, of religious stories is relatively unimportant: 'the moral pattern of life is the fundamental thing, the story its vehicle'. Doing is more important than saying, even though stories have a vital role to play.

Apart from Strawson's remarks on the relationship between formal logic and the logic of ordinary usage, and on the logic of persons, Words about God contains two other main perspectives which add to our understanding of the logical styles involved in religious language. In both cases the word 'metaphor' is all-important. That is why the index contains more references to this topic than to any other.

The second section of the book ('The language of Religious belief: some Classical discussions') is almost totally dedicated to extracts which approach religious discourse from the primordial division between metaphorical and literal readings. The great problem is: if the Bible is read literally it makes religious (and mental) nonsense, but if it is read metaphorically the ontological status of God is placed in jeopardy. Two solutions emerge. One is given by Aquinas, namely the middle way provided by the notion analogy; the other by Otto - talk of God is symbolic (largely metaphorical) but has substance because these 'ideograms' are grounded in the numinous.

Turning to the last section, 'The logical character of Religious language', we find that Ramsey and Evans have, very generally speaking, more to do with Otto than Aquinas. This is most apparent in the case of Ramsey, although we should mention that he gained many of his seminal ideas from Max Black. Very briefly, Black argues (but not in the extract included) that the theoretical models used in science function in a manner not all that far removed from the role of metaphors in poetry and common usage. They afford, that is to say, a unique and distinctive form of cognitive insight which cannot be translated into a non-metaphorical idiom. In this context the

'picture' theory of language is replaced by one in which 'model' discourse 'discloses' the phenomena being talked about without being able to capture it in a one-to-one replication fashion. Black specifically states that such models differ from the analogue variety by reason of the fact that they do not work by analogy but through a hoped-for underlying analogy.

Applying this to religious discourse, Ramsey has to make some adjustments. Why this is so need not concern us for the moment, but we should realise that this is where some of Otto's ideas reappear: religious disclosure models (i.e. most, if not all the Bible) are grounded in situations of 'cosmic disclosure'; are grounded, if you like, in certain special experiences. It need not worry us as anthropologists that Ramsey the empiricist philosopher is arguing for Christian claims because he nowhere, or at least importantly, introduces a priori claims of an objectionable variety. Instead, his descriptive approach irresistably reminds us of such works as Divinity and Experience. His 'anthropological value' is threefold: (a) religion is defended as religion, which means that the participants' universe is regarded as primary, (b) we learn much of the nature of models, the logic of metaphorical systems and why some such systems are more suitable than others, and (c) we gain new insights into the relationship between religion, science and poetry for the imagination always grasps the lesser known by following one basic strategy.

Ramsey, it will be realised, traces a firm path between the 'only literal or merely metaphor' choice. Anthropologists need no longer be trapped by the old positivistic oppositions such as 'at face value'/ 'x does not mean what it appears to', literal/metaphorical, informative/ expressive, etc. Evans adds further subtleties to this development. Inventing the notion 'onlook', he analyses this into such features as 'commissive', 'autobiographical', 'expressive', 'behabitive-postural' and 'verdictive'. He then classifies this 'looking on x as y' language in a broad literal/non-literal division, subdividing the latter into 'parabolic' and 'analogical' onlooks. Again, religious ontological claims are defended by saying that parabolic onlooks do not involve more 'as if' metaphors. We learn more about the 'is's' of Nuer Religion. And as the words 'commissive' and 'verdictive' indicate, Evans is developing aspects of Austin's position.

Finally, how does the extract from Nuer Religion fit into all this? Ramsey suggests it should be read alongside those from Maimonides. It is difficult not to agree with the implication that Evans-Pritchard's work belongs to the 'classical discussions'. Of course, this has to be the case: Evans-Pritchard wrote before the new solution to the metaphor/ literal distinction had been fully articulated. Accordingly, we can construct the following analogy: 'Maimonides, Otto and the rest: Evans-Pritchard if not Godfrey Lienhardt::the modern empiricits and philosophers of religion:?' A gap waits to be filled. Words about God suggests the tools we can use. Many modern theologians are writing for a secular age; some of them even kill God. So there is nothing to prevent us from profiting from their work. As Ramsey puts it, perhaps with anthropologists in mind, this book 'may help the reader to develop his own empirical approach to religious themes'. All I can add is, 'don't stop here. Try reading Models and Mystery (1964), Religious Language (1957), Prospect for Metaphysics (1961)...'

Paul Heelas

Man, Culture and Society. Shapiro, Harry L. (ed) Second edition. Oxford University Press, 1972. £1.60p.

Dehydrated food, though full of artificial flavouring and colouring, often fails to satisfy the palate. The same may be said of "potted" versions of any subject: economy is achieved, but with an awful loss of originality and interest.

This collection of essays, which originally appeared in 1956 was, at the time, one of the finest efforts to present anthropology as a "whole". The papers by Lévi-Strauss and Godfrey Lienhardt, again re-printed, soon became well known to undergraduates. The archaeological summaries were indicative of both the time at which they were written and of their authors. The whole was reasonably balanced.

It was inevitable with the passage of time and the modern craze for general readings in anthropology that a new edition of this book would appear. Some of the archaeological papers have been re-written reflecting the great increase in archaeological material and changes in ideas, though with no real appreciation of the new methodologies in this area. The paper by Meadow on the emergence of civilization is a good synopsis, but the re-printing of Gordon Childe's paper on the New Stone Age, a paper already dated in 1956, is rather shocking.

Social anthropology, it appears, either has not changed or the editor just thought the papers could not be updated. The only new paper in the whole of the 'cultural' section is that by Rappaport on "ecological anthropology", a misnomer if ever there was one.

The book, one feels, would have been better left as a model of past anthropology (Ruth Benedict included). One has visions of the book forever being "brought up to date", the original unity of the book disappearing as it grows in contributors and pages. As it is, the book has some articles on the archaeological knowledge of the present and anthropological models of the past. It is possible to divide archaeological matter into periods and areas, but the divisions of social anthropological subjects are no longer so clear as was once thought.

No doubt the book will sell to libraries as an "instant" reference book, but many readers will find it lacking somewhat in nutritional value.

James Urry

JOURNEY TO IXTLAN: The Lessons of Don Juan. Carlos Castaneda. New York: Simon and Schuster, 1972.

This book, the third in the don Juan saga, continues a number of trends developed in the first two. As, in A Separate Reality in comparison with The Teachings of Don Juan, the account was more personal as Castaneda accepted more of don Juan's teachings as at the least meaningful; so in this book Castaneda looms larger still, and don Juan loses all appearance of charlatanism. The 'non-ordinary reality' of the first book, the 'separate reality' of the second, drop out of the explicit picture - they have become an unqualified 'true' reality. It is in Castaneda's treatment of don Juan's 'other' reality, his view of its nature and status, that the most significant progression (and progress) is made through the series.

This is manifested in the content of each book. The first deals with don Juan's sayings as a detached system of belief. The second deals with the 'other world' of the sorcerer in relation to the psychotropic plants which help induce it. The third deals with Carlos Castaneda, and his relations with the world - especially his relationship with the 'other world'. After the anthropology and the psychology, we have at last come to the philosophy.

The latest work is the best of the three; at least it is the most satisfying to sympathetic readers. In each of the first two books one becomes frustrated with Castaneda for his insistence on 'looking', 'thinking', talking and, especially, his own 'rationality'. One is infuriated when he breaks off his apprenticeship at incomplete stages because of a supposed incapacity to enter the 'other world'. In the third book however Castaneda achieves the task of 'seeing' and 'knowing'; he admits both the achievement and the 'other world'. If he does not choose to enter the 'other world' permanently and completely forsake 'this world', we can at least respect his decision as one made of free will, not one forced on him through his own human inadequacy. This is the main reason why the book is so much more uplifting and optimistic than the first two, especially at a personal level. And as I have tried to explain, that is the primary level at which the book must be judged. The book is also the most satisfying yet in that, by at last accepting don Juan's premisses, Castaneda allows himself a better and more concrete starting-point (see Heelas, especially p. 135).

The book is also a much better constructed work than either of the other two. In place of the rehashing and somewhat forced 'structural analysis' that rounds off the first book, in place of the depressing tailing away into dejection and failure of the second, there is a truly dramatic climax. After seventeen chapters of old field notes (1960-1962) we end with three chapters covering the most recent experiences (1971), in which Castaneda 'stops the world', 'sees', talks to a coyote which in turn talks (or rather feel-talks) back; in which don Genaro makes Castaneda's car disappear (in fact he transports it to the 'other world', whence Castaneda has the unique experience of driving it back to 'this world'). The dramatic quality of this book in contrast to the previous ones is apparent in the shift from a sense of impending danger to Castaneda's identity and sanity, to impending physical danger from actual attacks (by various forces).

Nevertheless, the major failing of the book lies in its construction: although the first seventeen chapters are indispensable for an understanding of the events of the last three chapters, almost all the impact (and import) of the book come in these last forty pages. In themselves, the first 275 pages are of little value, adding but little to what we knew and felt from the first two books.

This is a very personal book; after reading it, it seems more natural to call the author 'Carlos' than 'Castaneda'; even the sorcerers are infinitely more personalized - if only because they hardly ever seem to stop laughing. And by personalizing his account, Castaneda has concretized it. Previously, in dropping out of the system as a failure, Castaneda left a bitter feeling that both systems/worlds were insignificant. He has now, by opting out of the system as a success, not only accepted and demonstrated the importance of don Juan's world, he has also reaffirmed the importance of (all) our own.

For those I have managed to enthuse, for those who are already enthusiastic, for all those who want to read for themselves the solving of this mystery, British publication of the book has been announced for this May. And the fourth episode, Tales of Power, is scheduled for publication in America next year.

Martin Cantor

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