

EDITORIAL

Social anthropology and psychiatry

Psychiatrists and their patients figure in jokes much more frequently than anthropologists and their informants. No doubt this is because the man or woman in the street in most 'advanced' societies is more likely to become a patient than to be made into an informant. And in most societies, especially 'advanced' societies, there are more psychiatrists around than anthropologists. Most people in such societies think they have a rough idea of what psychiatry is and what psychiatrists do, but very few people seem to know enough about anthropologists to fear them, hate them, despise them, or even love them. Perhaps this situation has started to change. One or two taxpayers' representatives have recently been wondering in print whether public money spent on research in social anthropology would not be better spent on something useful. And questions are beginning to be asked in the sitting rooms of taxpayers, if a drawing in a recent issue of *Punch* is anything to go by. Two parents with anxious faces confront their newly fledged undergraduate daughter with the appeal: 'Just in case anybody asks us . . . what's social anthropology?'

Attempting to answer this question in a few carefully chosen words is no joke. But some account of the problems of definition must, it seems clear, precede any discussion of the relationship between social anthropology and psychiatry. There exist today no fewer than five excellent, comprehensive, and readable accounts of the subject by outstanding British scholars (Evans-Pritchard, 1951; Firth, 1956; Beattie, 1964; Lienhardt, 1964; Mair, 1965); and a growing number of other publications, designed as much for the interested general reader as for the student, deal with specific topics and problems within the field of social anthropology. Nevertheless, the information has not filtered through; mystery and misunderstandings about the scope of the subject continue to be widespread, not least among those in other disciplines who are best disposed towards it.

'Social anthropology, defined operationally in terms of what social anthropologists have done during the last fifty years, is the study and comparison of tribal societies and of small fields of social life, with emphasis on the role of custom' (Devons and Gluckman, 1964). The tradition is one of observational and functional study of human behaviour in its social context; and the comparative method has usually been spoken of as the best way of reaching valid generalizations in the subject. These traditions are now being called in question by a number of workers, including those who feel that social anthropology, having 'reached a point of empirical plenitude and propositional futility' in spite of—and because of—'increasingly rigorous standards of fieldwork, and a vast accumulation of reliably ascertained facts', is now in a state of conceptual confusion (Durkheim and Mauss, 1963). And the intensive study of one society at a time by a man or woman working alone on the investigation of a hypothesis, collecting field material to provide data for new hypotheses, is really 'a method more in line with some experimental procedures in the natural sciences than any of the versions of the comparative method' (Fortes, 1970).

Seventy years ago anthropology aimed to study man, over time and space, as a biological and as a social being; it therefore included what is now the quite separate discipline of prehistoric archaeology. From this all-embracing subject grew several more or less distinct divisions, identified by such prefixes as social, cultural, psychological, and physical. Social anthropology was a peculiarly British development, though anything but insular in its origins and later growth. Among its most important intellectual ancestors were the French scholars Montesquieu, Comte, Lévy-Bruhl, Durkheim, and Mauss, whose abiding effect on the conceptual apparatus of the subject was mediated through the influence of their writings on the two men who between them shaped social anthropology as it is today: Radcliffe-Brown, an English product of the Moral Sciences Tripos at Cambridge, and Malinowski, born in Poland, originally trained there as a physicist and later a naturalized British subject.

From various parts of the British Commonwealth the subject not only drew much of the data which constituted its raw material but also attracted a high proportion of those pupils of Radcliffe-Brown and Malinowski who became its most distinguished contemporary teachers. A firmly established Anglo-American tradition, with strong French connections and characterized by a combination of scepticism and empiricism, is now widespread throughout the English-speaking world; its dominant concern has been described as being with those forms of behaviour which are not genetically determined (Leach, 1966), a formulation which avoids many of the problems to which other definitions give rise and leaves open a connection with ethology.

For it is when they come to define their discipline that profound differences of emphasis are revealed in the views of contemporary social anthropologists. One participant in the current debate on this issue has drawn particular attention to the smallness of the subject to explain why 'the idiosyncrasies of individuals count for so much', with the result that 'the situation has become confused by various declarations and rejections of theoretical allegiance' (Ardeier, 1971). It may be of interest to observe at this point that the Association of Social Anthropologists, to which belong most established full-time professional social anthropologists in Britain and the Commonwealth, together with a growing number in the United States and elsewhere, has a current membership of little more than 300.

There are a few social anthropologists who maintain that the subject, as something distinguishable from sociology, should not exist, or does not exist, or, if it exists, soon won't. Goody (1969) sees it as being too heavily committed to 'primitive' societies and to methods involving intensive field-work and the study of societies as wholes for it to be capable of making 'a satisfactory adjustment to a situation where societies are no longer primitive and the networks much larger than those of earlier times'; and he feels the methods have become 'a question of faith rather than reason, an end rather than a means'. Barnes (1963) and Needham (1970) provide other examples of what may be called responsible and qualified pessimism, a general attitude likely to be of interest to psychiatrists and others concerned with ethical and theoretical problems of research in the human sciences.

With so much confusion within the subject there should be no wonder that mystery and misunderstandings exist about it outside. But it is important that the existence of a minority with grave doubts and reservations about the future of social anthropology should not only be taken seriously but even emphasized in any account intended for readers not themselves directly involved or necessarily able to make out what the argument is all about. There are, of course, a number of social anthropologists who remain what may be called devotees of a kind of butterfly collecting, unrepentant and even uncritical followers of a somewhat crudely determinist empiricism. But the majority show no lack of confidence in their discipline and engage with varying degrees of zest in the fierce and active internal feuds which seem to have become characteristic of what a well-disposed, if critical and possibly envious, sociologist has described as 'a formidable academic clan . . . united in a common pride, for a common scientific endeavour, and for defence against all external attack' (MacRae, 1961). Such conflicts within the subject are nothing new; and one does not need to be a social scientist to see that internecine strife is not only a sign of youth and vigour but is also typical of small groups in general, particularly those whose members are linked by close and lasting ties and dominated by a number of highly individualistic leaders (living and dead), each with a more or less loyal following.

The different social sciences are distinguished from one another more by their methods of analysis and their problem-areas than by the kinds of things they investigate. As with other disciplines, to become a professional one must have carried out postgraduate research, with all that is entailed in the preparation of dissertations based on original work and the eventual publication of articles and monographs. What distinguishes the recruit to the ranks of professional social anthropologists is the virtually obligatory pursuit of intensive first-hand investigations in the field. This means spending one or two periods, usually of 12 months or more, living in a society other than one's own, learning and working through the medium of the local language and trying to understand how the local people see themselves and the world in which they live. All this is an essential preliminary to carrying out technical investigations of the theoretical problems with which one is particularly concerned.

This does not mean that established social anthropologists expect to spend more than a total of three or four years of their working lives on personal field investigations. Most of them are full-time university teachers. And, as has already been indicated, there are those among them who think the importance attached to the fieldwork tradition greatly exaggerated, who look upon it as a painful initiatory experience which with any luck erodes ethnocentricity and increases self-knowledge, without which one is at a disadvantage in estimating the value of others' fieldwork monographs, and without which one is in any case virtually excluded from acceptance on equal terms as a colleague by other social anthropologists. In certain respects there is a similarity here with the need for a physiologist with medical qualifications to have had at least some first-hand experience of sick people, if only as a clinical student and in preregistration house appointments. In this sense the resemblances between a training in medicine and a training in social anthropology at postgraduate level are closer than those between a training in medicine and comparable training in any other of the academic social sciences. And it could be argued that there are certain similarities in empirical approach, in the nature of the material, in the unavailability of truly experimental methods, and in the whole difficult field of confidentiality and ethics, between social anthropology and clinical medicine; just as there are other similarities between sociology and epidemiology and social medicine. And what has been said of the links between social anthropology and clinical medicine applies even more to the links with clinical psychiatry, in that both are concerned with behaviour and ideas in their social context. These analogies, though worth further exploration at another time, must not be pushed too far lest they dangerously mislead.

For most social anthropologists fieldwork continues to have a greater practical significance than its critics believe to be the case. It is not only a matter of being one's own ethnographer, of collecting material at first hand for later detailed analysis. Fieldwork also involves the salutary if sometimes demoralizing experience of discovering how inadequate initial explanatory hypotheses can be, of seeing the comprehensive model of how the unfamiliar social system works which one's mind develops in the first euphoric weeks or months of discovery gradually whittled away as more information is acquired; so that at the end of a year or two one emerges with a mass of data and the sober realization that the task of understanding and explaining seemed less formidable after one or two weeks than it does after one or two years. Quite apart from the aspect of personal ordeal and discovery, fieldwork involves testing the edge of theoretical concepts against the bone and gristle of the ideas and everyday existence of ordinary people living in another cultural world from one's own.

'The anthropologists have forced us to re-examine our notions of what is normal and healthy.' The importance of cultural relativity in the assessment of deviant or aberrant behaviour has nowhere been more clearly emphasized than in an essay which stands as a landmark in the shifting murky flood of writing on social psychiatry. But one doubts whether it is really true that 'the point has been so often made and is so obvious that I need not labour it' (Lewis, 1958). At a superficial level the issue is clearly understood and accepted. It is widely recognized that, in spite of a great deal of collaborative work by psychiatrists and social anthropologists, the difficulties of sorting out the presumed general human attributes of mental illness from those arising from the social and cultural settings within which occurs what may be called the process of symptom perception, recognition, identification, and referral are very considerable. The problems involved in closing the gap between psychiatrists and social anthropologists are less clearly appreciated. A beginning could be made if it were realized that the gap is indirect rather than direct, lateral rather than frontal—by which is meant a mutual failure to understand the other side's expectations and misapprehensions, so that more often than not when the two sides talk to each other they talk past each other.

Let us deal first with an instance of the expectations psychiatrists have of social anthropology, for these are less seriously misleading than the often gross misapprehensions social anthropologists, particularly British social anthropologists, have about psychiatry. In the same essay Sir Aubrey Lewis writes of the anthropologist being 'primarily concerned with groups of normal individuals who fulfil culturally approved roles, whereas the psychiatrist is mostly preoccupied with individuals who are somehow at odds with their cultural environment'. It may escape the reader of that passage, as it

certainly did not escape the writer of it, that roles necessarily involve relationships. The social anthropologist's primary interest is in relationships between social institutions, by which are meant standardized modes of social behaviour, rather than in relationships between persons or between groups or categories of people, though such relationships are usually involved in any consideration of relationships between social institutions. The crucial point to make, without going any further into the complexities of technical language, is this: relationships have no material existence, they are not given to the observer as data. Relationships can only be observed indirectly by interpreting behaviour, verbal and non-verbal. Relationships are inferred from what individuals say they do, from what individuals say they ought to do, and from what individuals are observed to do; and no psychiatrist needs to be told that these three basic items of data do not necessarily coincide. It is from these items, obtained first hand, that social anthropologists build up their account of relationships between institutions, groups, and persons, and from which they arrive at a description of the norms, both jural and statistical, which operate in the society concerned. They only deal with individuals at the highly important but wholly instrumental level of informants and actors, much as a physiologist carrying out a study of lung function deals with individuals as subjects; though in general it may be easier to get an individual to breathe into a bag on a stationary bicycle and correlate his performance with his height and weight than it is to get an individual to give an account of his beliefs regarding the causes of serious illness for correlation with what he does when he feels ill.

While not preoccupied with individuals at odds with their social environment, social anthropologists often find themselves concerned with such individuals in two quite separate respects. First, in many instances the most valuable informants on certain topics are those who, for a variety of reasons, personal and social, are in one way or another marginal men and women, observers of the social scene, those who—as Turner (1967) puts it—'would have been truly at home scoring debating points on a don's dais' and who welcome the opportunity afforded by the presence of an anthropologist for making explicit to a fellow intellectual and enthusiast things about their own culture which they had hitherto only known subliminally and which they had never before been able to discuss at exhaustive length. It will at once be apparent that the use of such specialist informants makes conspicuous a general problem in relation to any kind of data obtained from particular individuals at particular points in time—namely, the problem of reliability and universality. No doubt this is also a problem for psychiatrists making use of complex case material. While there are tests of internal logic and consistency which can be applied to any full presentation of complex ethnographic data, the fact remains that for practical purposes, as in the case of the use made by historians of a mass of obscure documentary source material, the honesty and judgment of the scholar must be taken on trust.

The second point is that the study of cases of individuals at odds with their environment may be one of the best ways of finding out the rules governing the 'culturally approved roles'. An obvious example is the use made of courts of law, of which the outstanding instance in social anthropology is the exposition of the concept of the reasonable man in the jurisprudence of the Lozi people of Zambia (Gluckman, 1955).

This particular example of a common misunderstanding among psychiatrists and others regarding the methodological and epistemological basis of contemporary British social anthropology has been dwelt upon because it is relatively 'hard' by contrast with other misunderstandings of what may be termed a 'soft' variety, the most widespread of which is the wholly false, if superficially reasonable, assumption that most social anthropologists subscribe to the tenets of the so-called school of culture and personality. One other difficulty must be mentioned, though so briefly and crudely that it is likely to lead to further confusion. This is the largely unspoken assumption that most social anthropologists are interested in seizing every opportunity to apply their subject to practical problems. The fact that this is by no means the case is baffling to members of other disciplines faced in their daily work with the need for action to improve the mental and material well-being of individual men, women, and children. It should not be thought that social anthropology cannot be applied in any way: far from it. But it must be said that if social anthropologists were primarily concerned to do

work of direct practical importance they would not have become social anthropologists in the first place.

Misapprehensions about psychiatry on the part of social anthropologists may be dealt with very briefly. In Britain at least they arise from the almost unbelievably widespread assumption that psychiatry and psychoanalysis are virtually synonymous. Should anyone doubt that this can be true, let him read the final chapter, entitled 'Possession and Psychiatry', in a recent publication by a leading contemporary social anthropologist in which Freud and Breuer are spoken of as the founders of modern psychiatry (Lewis, 1971). The same writer points out with some justice that, while most British social anthropologists unite in their antipathy towards psychological aspects of social phenomena, much of their work is in fact 'shot through with ill-considered and usually unacknowledged psychological assumptions', mostly of a very naïve psychodynamic variety. Much of the responsibility for this may be laid at the door of Durkheim, who did much the same himself not only in his study of suicide but more significantly for social anthropology in his work on religion. But it must also be recognised that the rejection of psychology arises largely from a particular concern with the one field which social anthropology has made uniquely its own, that of kinship, where classical studies by such men as Radcliffe-Brown of the structure of kin relationships could be pursued with the necessary objective rigour only by laying aside preconceptions of a crudely psychological kind about these most highly charged of all human ties.

Too much should not be made of these difficulties in expectations, the outlines of some of which have been briefly sketched; but there are important implications for any consideration of the relevance of social anthropology in the teaching of medical students, in the training of psychiatrists, and in strengthening those links which already exist between the two disciplines, particularly at the level of research. Those who support a larger role in the undergraduate medical curriculum for the social sciences because they feel that this is likely to equip the doctor of the future with knowledge or with increased sensitivity of value to him in handling the personal predicaments of sick people will no doubt regard social anthropology as presented here as likely to be of little use. Those who wish to increase the awareness of doctors both of the dangers of cultural relativity in the judgment of human behaviour and of the possibilities of scientific study of human social relationships may feel that social anthropology is one of the subjects most likely to be of value. Whether the amount of social anthropology it would be possible to insert into an already crowded curriculum would really help anyone is another question.

When we come to consider the contribution which social anthropology might make to the training of psychiatrists we are faced with the tricky problem of what, in a large and increasingly complex subject, is likely to be of the greatest practical use and theoretical relevance. For if it is felt that it is simply a good thing that those dealing with mental illness should know something of the general approach of social anthropology the problem of special relevance does not arise. Whereas selecting those aspects of the subject which are most relevant raises the further problem about how much sense can be made of some portions of a highly integrated subject when they are torn from their context. Some of these issues can be faced quite easily if one imagines a psychiatrist asking for the names of half a dozen monographs which he could use to find out about the central interests and key methods of social anthropology, which might be thought to have something to say to him about his work, and which could also be read with a certain amount of interest and pleasure for their own sake. One selection would be the following.

Firth's *We, the Tikopia* (1936) is generally accepted as the first full-scale study of the kinship system of an unambiguously primitive society, which is also a particularly detailed and rich account of the events of ordinary everyday life among the inhabitants of a tiny and isolated island in the Pacific. Evans-Pritchard (1937) gives an account of beliefs in witchcraft and magic among a Central African people and of the social processes by which the internal logic of a whole system of knowledge and belief is maintained; it is also a most subtle and intricate analysis of how individual actors in particular social situations seek answers to the question 'why' in respect of personal misfortunes, including illness and death. Fortes (1959) shows how beliefs other than beliefs in witchcraft may

equally well be used to explain misfortune in a West African society where the life-experiences and fortunes, good and bad, of the individual are ascribed in part to the notion of personal destiny and partly to the influence of ancestral spirits who reflect crucial aspects of the structure of the society. Malinowski (1922) provides an account of the exchange of objects of no practical value but great ritual value among the Trobriand Islanders of the New Guinea archipelago. In a book which has become a classic of descriptive ethnography, Malinowski carried out the first functional analysis of gift exchange which has great indirect importance for anyone interested in the symbolic exchanges involved in the psychiatric interview. Colson (1953), in an undeservedly neglected study of a small North American Indian tribe, gives a beautifully straightforward account of how gossip and scandal, seemingly haphazard and trivial aspects of daily life, are forms of customary behaviour with their own rules through which membership of the group and status within it are defined and controlled. Turner's *The Drums of Affliction* (1968) is an analysis of ritual healing, of the meaning of symbols and of how cultural categories sustain a particular social system among the Ndembu of Zambia. While it deals with healers and patients, it is principally a book about the relation between a system of thought-categories and the social life of the people who employ them, not only in the dramatic ritual performances described in detail but also in their ordinary daily affairs.

One might hope that, after a comparable reading list in psychiatry for social anthropologists had been prepared, a fresh start could be made on establishing links between two subjects which, unlike psychiatry and genetics, seem at first sight to be made for each other. A major source of difficulty in the past has been confused short-term exploration of each other's territory without an up-to-date map and in the search for common ground. Such ground is too often peripheral to the central interests of each side. However daunting at first, concentration by members of each discipline on the central interests of the other is likely to be less frustrating and more fruitful in the long run.

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