

SOCIAL ANTHROPOLOGY IN THEORY AND PRACTICE*

By J. A. BARNES

I. *Social anthropology in the University of Sydney*

For more than thirty years social anthropology has been taught in this University. As an academic and professional discipline social anthropology is still young and in its short history thirty years is a long time. We of the present generation are the fortunate heirs of those who in 1925 decided to found here a Chair of Anthropology at a time when there was no full-time Chair of Anthropology in the United Kingdom and only one in the British Commonwealth, that occupied by Radcliffe-Brown at the University of Cape Town. We are fortunate in several ways. We have the research work of thirty years on which to base our own enquiries; there is a well-established tradition of teaching to conserve and develop; the climate of informed opinion both within the university and among the public at large recognizes the contribution social anthropology can make to our understanding of the world around us. Under these circumstances there is no need for me to define my subject *ab initio* or to rehearse its historical antecedents and its position among the other arts and sciences. If my subject were a new one recently introduced for the first time into the university curriculum this course might be appropriate but it would be out of place here where the task of pioneering has long ago been completed by my distinguished predecessors. Instead I want to use this opportunity to draw attention to some aspects of social anthropology which seem worthy of scrutiny and which are likely to influence the lines along which the subject will develop in the years ahead. In particular I want to examine how the practice of social anthropology is related to notions of what the subject is or ought to be.

In approaching this topic I am very conscious of my debt to Professor Radcliffe-Brown, the first holder of this Chair and one of my own teachers. He was always concerned to relate theory to practice, shunning alike the mere collection of ethnographic material without reference to theoretical interest and the elaboration of theories and models of society not grounded on ethnographic fact. During the years he spent in this university he succeeded in initiating the first major programme of field research into the ethnography of aboriginal Australia and published that monograph which is still a major landmark in comparative and theoretical anthropology, "The social organization of Australian tribes". He retained a lively interest in problems of aboriginal society throughout his life and at the time of his death, less than two years ago, he was still engaged in spirited controversy about the analysis of

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Australian aboriginal social organization. When Radcliffe-Brown left Sydney in 1931, his lecturer Raymond Firth served as acting-professor for about eighteen months. Throughout his many publications Professor Firth has shown the same concern to marry sociological analysis with ethnographic fact, and by his outstanding study of Tikopia in the British Solomon Islands Protectorate he has enriched our understanding of the family and kinship everywhere. Firth left in 1932, and in the following year Dr. A. P. Elkin was appointed to the Chair, a position he was to occupy for over twenty-two years. Under Elkin's vigorous leadership the Anthropology Department increased in size, student numbers rose, and field work was carried on in aboriginal Australia, Papua, New Guinea and farther afield. Most of this work was done in regions where linguistic information was fragmentary if not entirely absent. Hence the study of Australian and Oceanic linguistics was developed as an essential auxiliary to the studies in social anthropology. Later, the great social changes which were beginning to make themselves felt in the wider Australian society attracted the attention of Elkin and his colleagues. Research was organized on aspects of assimilation, class structure and cognate topics. In all these inquiries an effort was made to relate empirical findings to analytical concepts, so that facts revealed by research became of more than parochial significance. Professor Elkin stressed particularly the moral obligation imposed on the anthropologist by his special knowledge to help in formulating public policy. By his long years of service to the cause of aboriginal welfare he has done much to make Australians of all kinds aware of their obligations towards one another.

Thus my department has a solid basis of thirty years work behind it. Much has been learnt about primitive and industrial societies both in Australia and in the islands of the Pacific. Valuable concepts have been developed, particularly in the field of aboriginal assimilation, and recognition has been gained for the rightful place of social anthropology as part of a liberal education as well as part of the specialist training of all those who have to work among peoples with cultures different from their own.

2. *Ethnographic fieldwork and sociological analysis*

Social anthropology is principally the study of social systems. In this study there are, broadly, two kinds of inquiry. On the one hand information is collected about particular systems, and on the other hand this information is interpreted and collated with data from other systems. The description of the way of life of a people is usually called ethnography and I shall refer to the anthropologist engaged in this task as the ethnographer. The interpretation of ethnographic reports and the comparison of one society with others may be termed sociological analysis. The problem I shall consider is then the relationship of ethnographic practice to sociological theory. Ethnographic reports, or tribal monographs as many of them are called, provide the link between fieldwork and comparative sociological analysis and I shall examine how the construction of these reports reflects changing field techniques and theoretical interests.

Both aspects of anthropological study are now carried on by the same body of professionals, if not by the same individuals. Yet in the nineteenth century anthropologists were rarely ethnographers. Tylor, the principal founder of social anthropology in Britain, did scarcely any fieldwork, and Sir James Frazer, whose monumental work *The Golden Bough* firmly established anthropology in the eyes of the general public, did no fieldwork at all. The advantage of combining the observer and the analyst was first demonstrated in 1888, when the young German ethnologist Franz Boas, who later was to become the greatest of American anthropologists, was sent by the British Association for the Advancement of Science to investigate the tribes of north-western Canada. For British anthropology the first professional expedition was made in 1898, when Haddon and Rivers from the University of Cambridge visited the islands of Torres Strait with their colleagues. Yet compared with what has to come these early fieldworkers stood aloof from the society and culture they endeavoured to observe and record. It was left to Malinowski, of the London School of Economics, who visited eastern Papua in 1914, to carry through the real revolution in fieldwork. To-day it is accepted that before the ethnographer can begin his work in the field he must be trained in anthropology, that his sojourn in the field will last for one or two years, and that his work will be carried on not through interpreters but in the language of the people he studies. Recruitment to the anthropological profession is based on sound theoretical training as well as practical field experience. By insisting on the importance of personal experience in the field social anthropologists distinguish themselves from many other sociologists, and likewise by stressing the necessity of theoretical analysis they differentiate themselves from a wide range of writers who report what they observe. Because of this dual orientation every ethnographer has to undertake two tasks. He must endeavour to understand the society he studies not only in its own terms but also in terms of a set of sociological concepts that will enable it to be compared and contrasted with other societies organized in different ways and employing different concepts.

Every description of the way of life of an exotic people has then to utilize the concepts of the people themselves and the concepts of the ethnographer which he brings, as it were, from outside. As theoretical interests develop and change, so the categories into which he groups his material change. Indeed, it is salutary to compare two tribal monographs written at the same time about similar and neighbouring peoples by ethnographers trained in different schools and with different theoretical or comparative interests. It is usually clear that they deal with similar people, and in so far as they are concerned only to report in the societies' own terms, there is likely to be similarity in their accounts. But the kinds of material they single out for special attention and the terms in which they analyse their material reveal their diverse professional interests.

Apart from differences in theoretical orientation, differences in treatment arise from the special characteristics of the societies themselves. A clear instance of this can be seen in the study of so-called stateless societies, a branch of social anthropology which has developed strikingly during the last twenty years. Stateless

societies are characterized by an absence of centralized authority and governmental machinery. They have no monarchs or chiefs and no institutionalized courts of justice. Public order and social equilibrium are maintained by elaborate systems of cross-cutting alignments so that any group or individual who seeks to depart from the accepted pattern of behaviour finds itself opposed by some other group of approximately equal strength. Deviant behaviour is thus limited, though not entirely prevented. Societies of this kind have been known for a very long time, but early travellers usually assumed that because there was no ruler a state of unbridled lawlessness must prevail; or alternatively, that members of these societies were such slaves to custom that they did not require special machinery to achieve social harmony. It was not until Evans-Pritchard studied the Nuer of the Sudan in the nineteen thirties that the great complexity of stateless societies began to be realized. Since then a growing collection of detailed studies has shown how intricate and varied are the solutions to the common problem of how to achieve an ordered and stable social life without centralized government and the organs of a State. An ethnographer who now goes to study a stateless society knows in part what to expect; at least he knows what kinds of social institutions he will probably find and he knows that if he does not find them he must endeavour to explain how it is that the people he is studying manage to do without these institutions.

Australian aboriginal societies belong to this stateless type. One of the tragedies of the study of Australian ethnography is that while there were still substantial numbers of aborigines living under conditions where public order was maintained by sanctions arising within the group, these problems of social control were not dominant in anthropological thinking and discussion. Now that the sanctions wielded by the European constable or station manager or missionary play such a large part in influencing behaviour, aboriginal communities can no longer be regarded as stateless, and these particular problems can no longer be studied directly. Aboriginal and part-aboriginal societies still call for our attention, but for other reasons. The old way of life has gone and cannot be revived.

In examining ways of life that are no longer accessible to direct observation the anthropologist is in much the same position as the historian. He is limited by the evidence that has fortuitously survived. For instance it would be of great interest to us to know more about the kind of law suits that were heard in Britain during the Roman occupation. It would certainly help us to understand the structure of the society in which our distant forebears lived, out of which our present way of life has developed. It would probably aid us in the comparative study of colonial administrations and in particular in the nature of what is misleadingly called "Indirect Rule". Unfortunately the detailed evidence is not available and never likely to become so. The position is the same for thousands of primitive societies whose mode of existence has been disrupted by Western contact. Even where contemporary documents exist, it is often difficult to reconstruct a vanished way of life. The patient labours of medieval historians demonstrate how slow and tentative must be the process of building up a balanced picture of social life,

particularly with reference to matters of marriage and kinship, even in so well documented a society as medieval Europe. The fragmentary records of the past may often have a suggestive value for the anthropologist, but to test and validate his hypotheses, and to refine his concepts, he must seek his own facts. These are to be found mainly by personal observation of a living society.

Furthermore, not only are the ethnographer's records the records of present activity; they are recorded in terms of the analytical framework of his time. Accounts of the present are likely to be of interest to future generations, though we cannot guess how our descendants will interpret these accounts or how they will draw inferences from them. Where societies are undergoing rapid change we are particularly under an obligation to record for posterity ways of life that will soon disappear. Yet the ethnographer cannot record for the sake of posterity alone. He is faced not with the task of recording thousands of discrete facts, but rather of making systematic observations that will reveal as clearly as possible the way in which the society is constituted and how it operates. He must continually gather fresh information, but it is quite impossible to expect an ethnographer to write down "everything he sees" on the chance that his observations may be of interest to someone at a later date. If he were rash enough to attempt this he might produce a stream-of-consciousness novel but scarcely an intelligible field report. The ethnographer tries always to understand and record what he sees and also to record those facts he sees and does not understand. But he cannot record what he does not realize exists, and in practice he cannot hope to publish more than he considers significant and of general interest. The problem here is not the volume of his records, though that is a problem of its own, but rather that his records can never be complete for all time. There is no complete description of any event, just as there is no complete explanation of it. The field ethnographer has to aim first at investigating fully those aspects of the society which he is competent to tackle, and then to follow his own intellectual curiosity for the rest. It is of course true that chance observations may have a great and unexpected value to posterity. For instance, David Livingstone in his travels through Bechuanaland in the middle of the last century happened to write down in his journal a short account of a settlement he was visiting, with information about the men and their wives. Today this note supplies us with data which would otherwise be quite unobtainable about the historical development of polygyny in that region. We may hope that some of the chance observations of modern ethnographers and travellers may be equally enlightening to posterity. But it would be foolish to adopt as a working rule that observations should be made merely in the hope that someday they may be useful.

The testing ground of present sociological concepts and generalizations must be the societies of the present. Inquiries about the past are of interest for their own sake, but chiefly for the light they throw on the present. Neither the memories of informants nor the records of earlier investigators are adequate substitutes for contemporary observations, though they may be essential aids to interpreting these observations.

Thus we see that the particular topics that attract the attention of the ethnographer arise in part from his training in social anthropology and in part from those specific features of the society that force themselves upon his attention. There is a continual two-way traffic between sociological analysis on the one hand and ethnographic observation on the other. New analytical concepts of social anthropology are developed to deal with field material that will not fit conveniently into existing categories ; new kinds of observations are made in the field to permit analysis in terms of recently-developed concepts. The only sound basis for the development of sociological concepts and generalizations is the detailed study of ethnographic fact, and the only adequate basis for penetrating field research is a solid grasp and lively interest in anthropological theory. It is true that social anthropology would be much the poorer if it had not drawn heavily on the ideas and suggestions of a host of classical and modern writers in philosophy, political science, psychology and other branches of learning. In the same way, ethnography would scarcely have begun its huge task without the help of observations made by historians, travellers, missionaries, administrators, novelists and countless others. But from the point of view of developing an effective body of sociological analysis together with economical yet comprehensive techniques of field research, these must be secondary considerations. Without the discipline of fact imposed on theorizing, the theory of anthropology would easily degenerate into elegant but useless model-building ; without the discipline of theory imposed on fact-finding, fieldwork would become mere kleptomaniacism for esoteric facts.

3. *The ethnographic report*

The ethnographer devotes a year or more of his life to the study of a community, but the only way in which he can make that experience available to the world at large is through his published reports which can be read in a few hours. I want now to consider what kinds of facts are included in these reports.

The main sources of information available to the ethnographer are what he sees and hears himself, what he is told by his informants, and what he reads in documents of one sort or another. To present his report, he combines this information and selects from it to construct a description of the people he has studied. This description consists partly of statements about what is done in general, and partly of accounts of what happened on particular occasions. Sometimes statements are added about the incidence or frequency of certain characteristics or events of a specified type. The different weight given to these types of presentation has varied considerably as social anthropology has developed and is closely related to changing methods of fieldwork and changing theoretical interests.

In their descriptive accounts, all ethnographers make considerable use of general statements, that is, statements about what is generally done. It is in this respect that the difference between social anthropology and history can perhaps be seen most clearly. To make a caricature of the distinction, one might say that whereas historians endeavour to show that events are different, anthropologists endeavour

to show that they are the same. Both are right, and the two viewpoints are not incompatible. On the contrary, both are essential to a full understanding of the flow of events. But in analysis they must be kept distinct. In other words, one phase in anthropological analysis consists of looking at the extremely diverse happenings of everyday life and seeking to classify them as simply as possible. For instance, the many individuals with whom the ethnographer is acquainted, each with his or her own peculiar circumstances of life and with a distinctive personality and character of his own, are reduced to a set of statuses, to fathers, youths, mother's brothers, employers, headmen, and magicians. On the basis of these classifications the ethnographer can speak of the actions of fathers in general, and of employers, and so on. He may observe a number of individuals performing sacrifices. He discovers that all these individuals are heads of households. Parallel enquiries confirm that it is because they are heads of households that they act in this way. The ethnographer can then say that household heads sacrifice at ancestral shrines.

At all stages of ethnography these general statements, as I have called them, form the bulk of tribal monographs. Even the accounts of myths and legends which used to occupy so large a place in the ethnographic literature are mainly statements about what is generally believed. From the earliest times, however, descriptive writing has also contained a chronicle element. By that, I mean that particular incidents have been reported, sometimes because they are held to be typical and thus to amplify and give additional credence to the general statement ; sometimes because the particular incident is thought to be unusual and thus to constitute a qualification or exception to the general statement ; and sometimes because the event reported is unique in the experience of either the people or the observer, so that no general statement is possible.

Early ethnographic accounts were organized in terms of a multiplicity of " customs ", that is, ways of acting and even of believing and feeling that are acquired by members of a society and which are taught with varying success to each generation. As we have seen, statements about customs are made on the basis of either the ethnographer's own observations or the reports of informants. Sometimes informants are asked about what would or should happen in hypothetical instances, at other times they are asked to recount what has happened in past situations in which they have been involved or about which they have heard. Most ethnographers use both approaches. In the past, the ethnographer often assumed that there was a single customary mode of behaviour and that most people followed it most of the time. If variations were observed or if informants disagreed with one another, this was often reported but many of the early writers took the matter no further. Apparent variations in custom were usually related, if to anything, to regional differences or to the changes going on as a result of external contact. Rarely were variations connected with the specific social circumstances of the informant, to his position as the head of a large household or as a senile widower, as a social climber or as one who has already reached the top, or as a person intellectually curious or socially inarticulate.

The study of variation developed further when attention was directed to societies undergoing pronounced social change. The early ethnographers were usually interested in establishing how primitive people lived before they became exposed to Western influence and often ignored recent innovations as not forming part of their study. Thus for instance when they were studied by Malinowski the Trobriand Islanders had already been in contact with the West for several decades. Yet Malinowski almost ignores the effect of the British and Australian administrations on their way of life. However, in later years Malinowski was prominent in the study of cultural change and encouraged his pupils to investigate the diverse ways in which primitive societies were changing in response to White contact. The work of Dr. Hogbin, who for many years has been on the staff of the Anthropology Department of this University, is notable among these studies. In rapidly changing societies, external influences act more sharply on some sections of the populations than others, so that it cannot be said of them that all men behave in some specified way. Sometimes variations are reported in statistical form, at other times a contrast is drawn between communities exposed to varying degrees of contact.

When an ethnographer makes a general statement he mentions no individuals. He reports simply that all members of a society, or all men or all village headmen, whatever the social category may be, behave in a certain way. Individuals make their appearance in ethnographic reports in two roles, as informants or as actors. For instance, the ethnographer may report that according to X a widow is cared for by her dead husband's brother in a leviritic union, while Y states that widows are free to return to their own kin. In this case, X and Y appear in his report as informants. Alternatively, the ethnographer may observe that Z was caring for the widow of one of his dead brothers but had allowed the widow of another dead brother to depart to her own people. In the report, Z appears as an actor whose actions are described rather than as an informant with a stated opinion. These two roles of informant and actor often overlap, and the same individual may appear in both roles, but the distinction between the two is important. In the early monographs, few individuals are mentioned at all and they are usually informants, even though part of their information refers to their own actions. If they do appear as actor, it is in very short episodes and the cast in one episode is not ostensibly related to the cast in any other. This style of reporting arose out of the field methods then used, and in particular from the necessity of working through interpreters. The ethnographer tended to begin his inquiries by eliciting general statements from his informants, and then later checked some of these by direct observation of particular instances or by asking informants to report on instances they had seen or could remember. No ethnographer can hope to observe with his own eyes more than a fraction of the social situations he needs to investigate and he has therefore to make use of the evidence of others. This is as true to-day as it was fifty years ago, but recent field workers tend to make much greater use of the people they study as actors than did their predecessors. This is partly due to the better opportunities the modern fieldworker often has for coming into close and prolonged contact with a small group

of people, a mode of fieldwork which has developed as a result of conscious policy. Instead of trying to survey a large population, or to cover all aspects of culture, the method of study now followed is often, in Evans-Pritchard's words, "to get to know well the persons involved and to see and hear what they do and say"¹ In part therefore the emphasis on actors rather than informants is due to the shift away from the collection of statements about customs and the details of ceremonial behaviour to the study of complex social relationships. The core of the ethnographic report then becomes an account of the way in which specified individuals are seen to behave towards one another in a variety of situations, together with a full analysis of these events elucidating the various general principles which appear to give both regularity and diversity to social behaviour.

In these respects therefore we may expect to find differences in the content of ethnographic reports. I have mentioned already the use made of general statements, of statistical and other indications of variation, the appearance of individuals as actors or informants, and the effects of intensive field methods on the content of these reports. I shall consider shortly shifts in emphasis between what is actually done and what ought to be done.

4. *Spencer and Gillen, Radcliffe-Brown, Malinowski and their pupils*

I wish now to turn from these general observations about ethnographic reporting and to examine briefly a few well-known monographs. I shall discuss one nineteenth century publication before considering the work of Radcliffe-Brown and Malinowski. I shall pass rapidly over the work of their pupils before considering some of the salient features of modern fieldwork.

The different modes of reporting I am trying to distinguish can be well illustrated from Spencer and Gillen's classic work *The native tribes of central Australia* (1899). Both authors were interested in the wider problems of anthropology, but they were essentially ethnographers rather than theoretical anthropologists. They were in touch with anthropologists in Britain who looked to them for fresh ethnographic information. In their book² the authors make a broad distinction between "custom" and "ceremonies". They present their information on "custom" mainly as general statements, with particular instances cited in support or amplification. Thus there is an account of how wives are obtained among the Aranda and their neighbours, including a statement of the circumstances in which a woman will elope with a man and the likely consequences of their action. This is followed by an account of what happened when a certain woman eloped with a certain man. Other features of secular behaviour are reported in similar style. On the other hand, in dealing with various totemic and initiation ceremonies, the authors relate what happened in ceremonies they witnessed and fill in their account of ceremonial life with informants' versions of those ceremonies they did not see themselves. They comment on

¹ Page x of Introduction by Professor Evans-Pritchard in J. A. Pitt-Rivers, *The People of the Sierra* (1954).

² W. B. Spencer and F. J. Gillen, *The Native Tribes of Central Australia*, (1899), pp. 556-559.

differences between the ceremonies they saw and similar ceremonies reported by other writers. Physical measurements of Aranda individuals are given with considerable precision, but there is very little statistical information about social life. It seems as if Spencer and Gillen were well aware that physical characteristics like the size of the head and the length of the arm were quantifiable, but did not see the need or perhaps even the possibility of attempting quantified statements about non-physical attributes. Thus they observe that the most usual way of obtaining a wife is by agreement between the fathers of the couple, and not by elopement, but they do not indicate precisely how frequently couples elope or what categories of men are most likely to seek wives in this way.

The great attention given to the details of ceremonial is characteristic of much writing of this early period. Spencer and Gillen in Australia co-operated closely with Tylor and Frazer in Britain when preparing their book for publication, and it is therefore understandable that their book should reflect the theoretical interests of their time. Comparatively great stress is placed on the details of symbolism, particularly in relation to totemism and initiation; there is a substantial chapter on what we would now call material culture and technology; there is a long account of magical procedures. Most of this material is presented as forming part of a common pool of knowledge and belief, or as part of a body of customs. In describing customary behaviour, the authors pay more attention to what is done than to the identity of the people who do it. There are some exceptions to this, and a striking indication of the extent to which kinship relations dominate aboriginal society is that so frequently Spencer and Gillen report that such-and-such an action must be performed by someone standing in a specified kin relationship to the central figure in the situation. Even so, the evidence that now enables us to piece together the content of the relationship between a man and certain specified kinsmen is scattered throughout the book and not drawn together in a chapter dealing with the kinship system as a whole.

Spencer and Gillen belong to the era when the comparative anthropologist at home relied solely on the reports of others for his ethnographic information. All the other monographs I shall consider belong to what we may call the present era, when social anthropologists conduct their own fieldwork. I shall begin by examining the ethnographic work of Radcliffe-Brown and Malinowski. The choice of these two anthropologists is almost inevitable. They exercised a paramount influence on the development of British anthropology, and their books are well known to a wide audience, lay as well as professional. Their work has a special significance for us on account of their connexion either with this university or with the ethnography of this region.

One of the earliest monographs of the present era is *The Andaman Islanders* by Radcliffe-Brown. This book has had a profound effect on social anthropology for its analysis of ritual and symbolism, and with it Radcliffe-Brown established himself as the interpreter of Durkheim's sociological theories to the English-speaking world. The fieldwork for the study was carried out from 1906 to 1908 and the report finished in 1914. Looking back over the years this classic work can be seen clearly occupying

a position intermediate between the earlier work of Haddon and Rivers on the Torres Strait expedition and the later studies that were to follow based on methods of fieldwork inspired by Malinowski. Most of the descriptive portion of Radcliffe-Brown's book consists of statements of what the Andamanese do in general, with frequent references to regional variations. The influence of Haddon, Radcliffe-Brown's first teacher, can be seen plainly. Much of what Radcliffe-Brown reports he must have seen for himself, and at several points in the book he makes this explicit. Thus his description of dancing is far too detailed and perspicacious to be based on anything other than direct observation, and we may assume therefore that on several evenings Radcliffe-Brown watched dances in the middle of Andaman settlements. In one instance he reports that he was unable to witness a certain ceremony and reproduces the report of an earlier investigator. Occasionally he mentions a discrepancy between what it is said should be done and what is actually done, as for instance when he observes that a certain kind of fig tree is associated with the unborn souls of babies and must not be cut or damaged. He adds³ that the tree is nevertheless cut to obtain bark for ornaments. In describing myths and legends he refers frequently to the divergent accounts of informants from different localities. But throughout the book there are almost no persons mentioned by name, and specific individuals make their appearance as informants rather than as actors. The few individuals mentioned each appear in one context only.

In almost all primitive societies, every individual depends for help and assistance on his kinsmen. The study of kinship has accordingly become one of the major branches of social anthropology. Rivers, one of Radcliffe-Brown's teachers, showed how the investigation of kinship could be facilitated by the use of what he called the "genealogical method". His method was to ask an informant the names of his parents, cousins and other relatives, and then to inquire of him what he called a specified relative and how he behaved towards him. Radcliffe-Brown discusses Andamanese kinship at some length, but surprisingly includes no genealogical information in his book and states⁴ that his investigations on this topic failed. I shall show later how important genealogies have become in recent ethnography.

Radcliffe-Brown's studies of Australian aboriginal society and culture, carried out at various times between 1910 and 1931, are reported in much the same form. He is concerned to discover what are the beliefs generally held about the cosmos, what are the rules of marriage and descent, and what variations there are in these matters from one group to another. There are few suggestions that individuals may differ in their beliefs from the rest of their group, and little concern with the extent to which the rules of proper and alternate marriage are followed in practice.

No investigator can hope to publish in full all the detailed observations on which his eventual conclusions are based. Indeed it would be an impossible task to try to make oneself acquainted with all the details of the work of each of one's colleagues. Everyone has to support his published conclusions with sufficient empirical evidence

³ A. R. Radcliffe-Brown, *The Andaman Islanders* (1933), p. 91.

⁴ *Ibid.*, p. 72, fn. 1.

to convince the world at large without deterring others by the mass of evidence in which the conclusions lie buried. Radcliffe-Brown may have collected large numbers of accounts of actual instances and on the basis of this evidence published his statements that in general men in the Andamans or in aboriginal Australia behave in such-and-such a way. It is clear that evidence of this kind does lie behind many of the descriptive passages in *The Andaman Islanders*. At other times the reader is left wondering how far the picture presented is a generalized account based on informants' statements of what should or has recently happened and how far it is derived from observations made by the ethnographer himself. I think it is an indication of Radcliffe-Brown's greatness that this book, which it is now so easy for us to criticize, should have had so profound an effect on social anthropology. Our ability to make these criticisms depends in good measure on Radcliffe-Brown's own teaching and his interest in problems of method. Radcliffe-Brown's achievement lies not only in his publications but in the extent to which he was able to inspire his pupils to examine in detail the problems he had sketched in outline.

It has often been argued that although Radcliffe-Brown supplied social anthropology in Britain with its initial set of theoretical concepts he did little to influence its methods of fieldwork. These were derived from the teachings of Malinowski, who worked in the Trobriand Islands of eastern Papua between 1915 and 1918. How far does Malinowski differ from Radcliffe-Brown in the kind of ethnographic information he collected and published?

At first glance the difference is considerable. Malinowski went out of his way to stress the importance of close and sustained contact with the society being studied. Whereas Radcliffe-Brown is often silent on his methods of fieldwork, Malinowski begins his first book on the Trobriand Islanders with a discussion of fieldwork methods. The first plate in the book shows his tent pitched alongside some native dwellings and the caption tells plainly that this is precisely what the photograph is intended to show. Radcliffe-Brown was handicapped in the Andaman Islands by language difficulties and never became expert in the vernacular,⁵ whereas Malinowski, who spent a much longer period in the field, was able to become fluent. He was thus free of many of the limitations that restricted the information available to Radcliffe-Brown. His books abound with a wealth of detail that leaves the reader in no doubt that Malinowski reports what he saw with his own eyes and relies comparatively little on the recollections of informants. Indeed, where Malinowski gives several accounts of similar events, it is usually because he has seen the various ceremonies or expeditions, or heard the magical formulæ recited, several times. A few Trobriand Islanders are mentioned by name in Malinowski's writings and some of them begin to emerge as distinctive personalities, as for instance the chief To'uluwa of Kirawina district and his magician nephew and heir Bagido'u. Yet although it is clear that Malinowski has information about a large number of individuals, he presents his information in general form when he is not relating events in which he participated.

⁵ *Ibid.*, p. 69, fn. 1.

Indeed, he sometimes says in rather half-hearted fashion "Let us take a concrete example, that of *an average man*"⁶ and proceeds to outline a hypothetical situation, though no doubt a typical one. In the last of his Trobriand studies, *Coral gardens and their magic*, he changes his mode of presentation somewhat and gives details of particular gardens and particular gift transactions. The people who appear in Malinowski's writings flit in and out of our ken in tantalizing fashion and we seem never to be able to bring them into sharp focus. One reason for this is that, despite his affirmation that social organization underlies the whole of social life, Malinowski never published his long-awaited account of Trobriand kinship, and he does not seem even to have contemplated an account of Trobriand political life.

In order to build up a systematic picture of Trobriand society we have to work through Malinowski's numerous volumes and articles and piece together the structure from the wealth of generalized detail which he records. This gap in Malinowski's publications is all the more unfortunate since he clearly collected the necessary information and he stressed⁷ in general terms the value of concrete genealogical facts in directing field inquiries.

Despite the richness of Malinowski's work and the wide range of his inquiries, his account of Trobriand life remains largely at the level of general statements about behaviour postulated as typical, with a detailed chronicle of certain ceremonial trading voyages in which he took part. Although in his first volume he promises⁸ the reader "*statistic documentation by concrete evidence*" it is not until his final study published thirteen years later that he begins to provide what we should now regard as statistical evidence.

I shall comment only very briefly on the work of the pupils of Radcliffe-Brown and Malinowski. Fieldwork carried out between the wars by ethnographers trained by them shows the direction along which the subject began to develop. General description still forms the greater part of their writings but many more individual instances are reported and there are the beginnings of a statistical treatment of individual variation, particularly in marriage relationships and economic activity. The individuals whose names are mentioned often appear as actors in many different contexts so that it is possible to gain some idea of how they stand out from their fellows. The reader can to some extent rearrange the ethnographic information provided in terms of his own analytical scheme which may be at variance with the author's. For instance, Firth's study of kinship in Tikopia contains not only a full description of what the Tikopia do in general, but also a great number of accounts of what named people did on occasions when the ethnographer was present. Similarly, Fortes' books on the Tallensi of West Africa are studded with proper names. Data of this kind are published by the ethnographer in order to support his generalizations about customary or usual behaviour, and it is a tribute to the thoroughness with

⁶ B. Malinowski, *Argonauts of the Western Pacific: An account of native enterprise and adventure in the archipelagoes of Melanesian New Guinea* (1922), p. 92. [His italics.]

⁷ *Ibid.*, p. 14 f.

⁸ *Ibid.*, p. 17. [His italics.]

which he does this that the material can later be used by some other writer to illustrate different points. Both Firth's and Fortes' studies have been utilized in this way.⁹

Adequate documentation is clearly an advantage, for it makes the author's own argument and assertions more convincing and permits alternative analyses of the material. But if carried too far it defeats its own purpose, for the argument is concealed by the forest of data. One solution to this problem of presentation is that followed by Evans-Pritchard in his studies of the Nuer. His practice has been first to publish considerable quantities of empirical material culled from his field notebooks and at a later date to publish separately an account dealing with the same topics at a more abstract level.

The same awareness of the need for both general statements and particular chronicles can be seen in the study of the Nupe of Northern Nigeria by Nadel, whose sudden death last year was such a severe blow to anthropology in Australia. Despite the great size and complexity of the society he describes, he manages to include many references to individual instances and indeed is almost forced to do so by the impossibility of making valid generalizations in situations of great social and cultural heterogeneity.

5. *Comparative studies*

So far I have considered only what are sometimes called descriptive accounts, in which an ethnographer presents a report on a people he has studied and draws only incidentally upon information about other societies. Every good description is also an analysis and has relevance for studies of other societies. Sometimes the analysis is made in terms of well-tested categories developed earlier, in which case the account confirms the utility and applicability of the categories or it draws attention to their limitations. At other times new categories of sociological analysis are developed to handle unprecedented field data and in this case subsequent testing in other societies is invited. A quite different kind of study which has had an influence on methods of fieldwork and categories of analysis is the purely comparative investigation in which a writer takes a number of ethnographic reports and attempts to make comparisons between them, or to establish some correlation valid for them all. In these studies, anthropologists attempt to make statements about human society in general and not merely about one society. Every ethnographer tends naturally to be more impressed by what he has seen with his own eyes and experienced directly than by what he has read in the reports of other writers, and hence is tempted to pontificate about all human society on the basis of his own restricted fieldwork. For instance, it is often pointed out that when Malinowski talks of primitive people in general, he is really talking only about the Trobriand Islanders; but Malinowski is not alone in this fault. Comparative studies provide a simple check against this temptation.

⁹ Cf. G. C. Homans, *The Human Group* (1951), Chapter IX; P. M. Worsley, "The Kinship System of the Tallensi: a Reevaluation", *J. Roy. Anthropol. Inst.*, Vol. 86 (1956), pp. 37-75.

One of the earliest of these comparative analyses is a paper by Tylor published in 1889. Some features of Tylor's methods are still relevant. In his paper Tylor endeavours to correlate the presence or absence in about three hundred and fifty societies of some nine customs relating to marriage and descent. He compares not the customs themselves, for these are assumed to be uniform, but the societies in which they occur. Each tribe for which there is adequate information is classified as having or not having one or other variety of the custom being considered. This procedure may seem simple enough, but it invites a query. What is meant by saying that a certain tribe *has* the custom of, say, a married couple residing with the kinsfolk of the bride? Some writers assume that this is merely a short-hand way of saying that each married man in that society lives with his bride's kin. Other writers adopt a less draconian standpoint and take the statement to mean that most men live in this fashion. Yet others assume that the society has the rule of residence with the bride's kin and that even if some men live elsewhere, everyone in that society is agreed that a man ought to live with his bride's kin, and there may be penalties for not doing so. In other words, some writers imply that all members of the group behave in some specified way; others imply that at least the majority behave thus; and yet others imply that the group has a rule about how people should behave. To writers of that last category it is often of only secondary importance how often the rule is followed or broken. These different approaches have been followed by various ethnographers as well as comparative writers, and it is not always clear which standpoint has been chosen.

Although there have been many cross-currents in the development of social anthropology since Tylor published his paper nearly seventy years ago, I think we may say that one line of progression has been from the first standpoint through the second to the third. Initially, as it were, anthropologists tended to assume that, at least on the whole, people were bound by custom, even if careful analysis was necessary to discover and define these customs so that they could be said to be followed. Then came a growing realization that in many respects the behaviour of men and women in other societies, however simple these might be, was as varied as in our own complex and heterogeneous society. Much effort was expended in describing and measuring the range of variation. Consequently it became harder to make generalizations about particular societies and very difficult to compare one society with another. In the third phase, which is still with us, a rather drastic attempt has been made to simplify this overcomplicated picture by shifting attention to the rules of the society, the "ideal structure" as it is sometimes called. Instead of studying what actually happens, attention is directed to what people think should happen or might happen. In this third phase it is usually tacitly assumed that although people may differ considerably in what they do, they agree about what ideally should be done. In terms of our example, the men of a tribe may vary considerably in where they live; one man may live with his wife's father, a second with his wife's mother's brother and a third with a distant kinsman of his wife, a fourth may have lived for a while with his wife's people and then returned to his

own kin, a fifth may live permanently with his own father and brothers, a sixth may marry a close neighbour and thus live in close proximity to both his kin and his wife's kin, and a seventh may take his wife to live where neither has kin; yet all seven, it is postulated, agree that a man ought to live with his wife's kin, for this is the "rule" in that society. To me, this assumption appears often unjustified. Recent research¹⁰ has shown that in a complex industrialized society there is sometimes as much variation in what is believed should ideally be done as there is in what is actually done. Anthropologists have long been aware that in another department of belief, that of myths and legends, and even of genealogies, members of the same society believe differently, and it has been shown how these differences are related to the divergent interests of the people concerned. But curiously in the field of customary behaviour this likely lack of consensus has generally been overlooked or attributed to differential contact with outside influences and agencies.

At present, purely comparative studies are beginning to appear again after a gap of several decades. The value of the substantive conclusions reached by these inquiries is often disputed but it seems certain that they will at least have a suggestive value for further field research. I shall consider the aims of these comparative studies at the end of this lecture.

6. *Recent ethnography*

We of the present generation of ethnographers have the advantage of the pioneering efforts of those who went before us. If what we try to do appears different from that aimed at by our elders, it is because our elders explored so thoroughly the problems from some of which we appear to be turning away. The present trend in ethnographic fieldwork is towards intensive, rather than extensive, field enquiries. In several recent reports an attempt is made to deal not only with that regularity between societies which interested Tylor and Radcliffe-Brown but also with variation within societies, a topic which first became prominent in studies of culture change under Malinowski's aegis. At the same time an effort is made to relate what actually happens with what people think ought to happen. This synthesis is attempted by a detailed examination of the various relationships that link together the individuals in a community and of the manner in which these conflict with and assist each other. Another line of inquiry is into the constitution of the various groups and networks of relationships in the community. A dynamic element is introduced by examining how groups break up and coalesce, how one relationship yields dominance to another. The individuals who featured in the earlier reports as informants are largely replaced in these most recent studies by individuals who are actors, who are presented to the reader not for what they believe or have remembered, nor even for what they do on their own, but rather for the way in which they behave towards other specified individuals. One index of this change of treatment is shown in the use made of genealogical tables. In most of the earlier monographs genealogical charts are

¹⁰ E. Bott, "Urban Families: The Norms of Conjugal Roles", *Hum. Relat.*, Vol. 9 (1956), pp. 325-342.

merely convenient diagrams for indicating kinship terms. In recent studies there is a return to the pioneer methods of Rivers. Quite complex genealogical charts are used to set out the kinship connexion between the various individuals whose actions are reported and analyzed.

In the early accounts, ceremonies, rites, legends and myths were reported at great length, while the groups and relationships characteristic of the society were outlined in static terms. By contrast many modern monographs contain only highly selected accounts of ceremonial behaviour but include long and complex case histories involving a large number of individuals whose relationships to each other are of consequence for understanding their actions. In other words, a shift has been made from describing the constitution of a society to showing how it works. The difference between the two periods can be seen by reading an early and a recent monograph in succession. Whereas an early report such as Spencer and Gillen's classic mentioned earlier may remind us of a church service, with elaborate symbolism and ceremonial but few distinctive social roles, these later works suggest instead a Russian novel, with a host of characters whose changing positions relative to each other and to the external world constitute the plot. Mitchell's recently published study of villages among the Yao of Nyasaland provides a good illustration of this contrast. To illuminate one particular point of his analysis Mitchell presents a summary account of events covering a period of eight years and involving more than thirty-five individuals whose relationships to one another are shown¹¹ on an accompanying genealogy. By means of this episode he demonstrates how a group of matrilineally-related kinsfolk living together in a village breaks up, so that one section of the group goes off to live elsewhere. Though only a summary of the episode is presented, it is full enough to show the complicated way in which brothers compete with one another for the allegiance of their sisters and cousins, how one generation endeavours to maintain its leadership when challenged by the next, how charges of sorcery are made as a way of bringing to a head long-standing antagonisms and grievances, how ill-health and death are given a moral value, how quarrels are settled sometimes within the group and sometimes by appeal to outside authority. Like a Russian novel, an account of this kind is sometimes hard to follow, particularly for a reader without practice in tracing genealogical connexions, but it makes possible a much greater understanding of the mechanisms by which these matrilineal groups usually hold together but sooner or later break up.

Presentation of material in this way is essentially exposition by case histories. The ethnographer reports an incident or episode and uses it as a text, as it were, on which to base his analysis. He also supports his analysis by material drawn from a wider range, in particular by a statistical analysis of many instances, so that the range of variation and the most frequent occurrences can be indicated. The statistical analysis provides him with the skeleton of the analysis; the detailed case histories give it flesh and colour, as well as suggesting further lines of research. This attention

¹¹ J. C. Mitchell, *The Yao Village: A Study in the Social Structure of a Nyasaland Tribe* (1956), pp. 165-174.

to the details of social life, of not merely aiming at making as wide and abstract a generalization as possible, is perhaps characteristic of a phase in the development of our subject when we are looking for new phenomena in social life and not merely trying to see into which previously-established pigeonhole our data will fit. The need to document fully in order not only to demonstrate the complexity of actual social life but also to isolate social processes which have hitherto been ignored can be seen in Gluckman's recent study of Barotse law.¹² Whereas accounts of primitive legal systems have dwelt mainly on the body of substantive law, Gluckman's concern is with the processes and concepts used by judges in arriving at their decisions. He supports his analysis with some sixty court cases reported in considerable detail, with relevant material on the social position of the litigants and sometimes of the judges as well. This pioneering study relies not only on the force of its analysis but also on the weight of its documentation.

The same concentration on the social relationships of a small number of persons well known to the ethnographer is seen in a recent study of middle-class married couples in London. In this enquiry, conducted by an interdisciplinary team, only twenty couples were studied, and no special effort was made to determine whether or not they could be regarded as typical or average. Elizabeth Bott, the social anthropologist in the team, has shown that one of the factors which influences the behaviour of husband and wife to each other is the extent to which their various friends are in touch with one another. Roughly speaking, where many of the friends of the couple are friends of each other, we find that husband and wife have different leisure pursuits and there is a clear division of labour between them in the household. On the other hand, where the friends of the couple are mostly strangers to one another, the couple carry out many activities together, spend much of their leisure time together, and exchange many household tasks as occasion allows.¹³ An observation of this kind is significant for its own sake, but also because it is the sort of observation that can be made only as the result of detailed and prolonged study of the behaviour of persons well known to the observer. Again it reveals an interest in the people studied not primarily as informants who might be asked about typical middle-class behaviour, as they saw it, but rather as actors in a social field whose configuration has to be charted and studied.

This approach has been carried a stage further in a recent study of Ndembu villages by Turner.¹⁴ He has been able to collect detailed information on a number of crises in the social life of the villages he studied, and has presented his material on them in a series of what he calls "social dramas". Not all life is crisis, but in critical situations tensions in the social fabric which are normally invisible or latent burst into the open. In much the same way as the study of the mind has been

¹² H. M. Gluckman, *The Judicial Process Among the Barotse of Northern Rhodesia* (1955), pp. xxiii + 386.

¹³ E. Bott, "Urban Families: Conjugal Roles and Social Networks", *Hum. Relat.*, Vol. 8 (1955), pp. 345-384.

¹⁴ V. W. Turner, *Schism and Continuity in an African Society: A Study of Ndembu Village Life* (1957), pp. xxiii + 335.

advanced by investigations of mental process under conditions of extreme stress, so our knowledge of the internal stability and external pressures of these village communities has been increased by Turner's analysis of the complicated currents of thought and action set in motion by some long-awaited crisis. To continue our metaphor, the Russian novel is here replaced by a Greek tragedy, in which eternal conflicts suddenly take on dramatic form. Turner's brilliant study can scarcely be a model for all ethnographic reports, for the ethnographer must take his people as he finds them, and cannot induce crises for his own edification. But Turner's work reminds us of the diagnostic value of critical situations when latent stresses and strains become manifest.

A crisis is not only an emergency in which social reserves are thrown into the fray; it is also a situation whose outcome is uncertain. Turner shows how there is a periodic movement from one crisis to the next, and how at one level of analysis this gives a regularity to Ndembu village life. But the outcome of any particular crisis is never completely predictable. His actors are creatures of their culture, bound by their ties to one another, and yet are free agents able to make decisions. They are in fact continually forced to choose between many possible courses of action. He shows us the actual consequences of the decisions that were taken, and also the probable consequences of the decisions that were rejected. Above all, his study reminds us how circumscribed must be our attempts to forecast what will happen in some future hypothetical situation.

7. *Social laws*

This brings me to my last point. It is sometimes said that the task of social anthropology is to establish social laws. There has been dispute on this issue for a considerable time, not only about whether or not these laws or correlations exist, but also about the nature of these postulated laws. This controversy is likely to continue. One of the first attempts to establish general correlations, using a large body of controlled ethnographic data, was made by Tylor in the article already mentioned. In it, among other things, he tried to show that the practice of naming the parent from the child (teknonymy), ceremonial avoidance of a wife's relatives, and marriage residence with the relatives of the bride (uxorilocality) were all due to a common cause, namely, matrilineal descent. His evidence showed, he asserted, that the odds were six to one in favour of a common cause. He added,¹⁵ characteristically: "Many of the firmest beliefs of mankind rest, I fear, on a less solid basis." Tylor's methods have been followed by several investigators including Homans and Schneider of Harvard in a notable recent inquiry (1955). But despite the claims of many social anthropologists the search for social laws that would have a universal validity analogous to that claimed for the laws of thermodynamics and quantum mechanics has not been very rewarding. I still remember the confusion with which, as an undergraduate trained in mathematics, I discovered that although the stated aim

¹⁵ E. B. Tylor, "On a Method of Investigating the Development of Institutions: applied to Laws of Marriage and Descent", *J. Anthropol. Inst.*, Vol. 18 (1889), p. 249.

of anthropology was the elucidation of social laws, no compendium of these laws, set out like propositions in geometry, was yet available. These laws still elude us, at least in that form. I think that in part the trouble has been that we have been dazzled by the achievements of physics and have tended to think that if anthropology is to be more than a purely descriptive art it must therefore be a science like physics. Had we modelled our scientific procedures on some discipline such as meteorology which, like anthropology, has to deal with successions of events that are perceived as similar and yet which cannot be treated as identical, we might have made better progress. Both deal with real life, and cannot conduct their researches in the laboratory. Neither can rely on planned experiments. In both disciplines, some of the significant variables in the situation being analysed are known to the observer, but not all. A complicating factor in social anthropology that is absent from meteorology is that the presence of the observer and, sometimes, the mere existence of his publications, affects the community being studied; but this complication too has analogies in other sciences. Social anthropology may differ widely from physics, but in my view it is a science like meteorology just as much as it is an art like history.

Indeed, my thesis is that social anthropology must have this dual aim. The postulated regularity of social events can be either an article of faith or an heuristic device or else it can be empirically, and only partially, demonstrated. So long as the evidence utilized in comparative study was made up of general statements about normal expected typical behaviour, perhaps all that could be done was to seek for abstract universal correlations. Other lines of development are open to us if we can utilize in comparative studies the wealth of detailed observation of concrete instances which is now becoming available. One approach would lie in the comparison not of societies possessing or lacking customs, as Tylor attempted, but of the customs or processes themselves as operating in diverse societies. Whatever line we follow I do not think we shall stumble easily on universal laws. As Postan said¹⁶ nearly twenty years ago

“ . . . the penalty of being sufficiently concrete to be real is the impossibility of being sufficiently abstract to be exact. And laws that are not exact, predictions which are not certain, generalizations which are not general, are truer when shown in a concrete instance or in one of their unique manifestations than they are when expressed in quasi-universal terms ”

In other words we have to steer a middle course between the Scylla of a mechanistic and unreliable sociological determinism and the Charybdis of ethnographic particularism. Such a course is possible and is, I think, the only one open to us. The detailed study of social process has been called “ social histology ”, a name which indicates clearly the way in which the field worker comes to grips with the social ties, the allegiances and divergencies, that link and divide the men and women he

¹⁶ M. M. Postan, *The Historical Method in Social Science: An Inaugural Lecture* (1939), pp. 38.

has chosen to work with. It calls for observations made over a period of time by a field worker who has made himself fully conversant with the general features of the larger society in which his microcosm is embedded. He cannot completely identify himself with his informants, but likewise he cannot stand aloof from them. He has to analyse not only his influence on their actions, but also their influence on his, for to a limited extent the observer is also an actor. When he comes to present his material to the world, he has to show in what respects the events he records are similar to those reported from other societies and in what other respects his data are apparently unique or distinctive. To illustrate the ways in which field ethnographers are facing these requirements I have selected a few recent studies in which I have had a personal interest, but there are many others which could have served in their place. In a short appraisal of this kind, there has been time to pick out only a few of the many encouraging and stimulating features of contemporary social anthropology.

Proceeding in this way, we can hope to increase our knowledge of what have been called social syndromes, that is to say, concatenations of processes, customs, systems of organization, external conditions and historical antecedents which are found to occur together sufficiently often to warrant at least their being given a name so that we may better be able to detect them when they appear again. The study of society in general is of value, not merely because it enables us sometimes to forecast what we are likely to find in society in particular, but more importantly because it can lead us to discover there new phenomena we might otherwise have passed by unseen. The justification of social anthropology, if justification be needed, is not that by looking at particular societies we can make statements about societies in general; it is rather that by looking at mankind in general we can better understand man in particular, including ourselves.