ANTHROPOLOGY AND SOCIAL CHANGE

LUCY MAIR

ROUTLEDGE

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CONTENTS

Introduction

This book includes some essays that were earlier published with the title *Studies in Applied Anthropology*. The substitution of the phrase 'social change' for the label by which my teaching post at the London School of Economics was described is no accident, but rather an admission. Anyone who claims to apply a theoretical discipline is offering to do something with it. All I have ever been able to do is trace out the changes that European technical knowledge, particularly under colonial rule, has brought to the societies which at different times have been called primitive, backward, underdeveloped, developing, and are at the moment officially, it seems, known as less developed.

DO WE APPLY ANTHROPOLOGY?

Anthropologists have always had to claim that this kind of study is useful, because it is expensive, and they are in competition with other specialists who put forward similar claims, perhaps more convincingly. Useful to whom, for what? Are we offering to tell people what to do, or how to do it?

Or can it be that what we actually offer is really neither? We have in practice sometimes been allowed to impart something of what we know to people whose aim it is to promote social change in less developed countries. Englishmen training as colonial administrators used to learn a little anthropology, Frenchmen and Belgians rather more. In Britain today teachers, health educationists, community development and other social welfare workers, sometimes even architects, are 'exposed' to social anthropology for shorter or longer periods. Once this would have meant ethnography: descriptions of the traditional customs of the peoples among whom the men and women taking the course might be expected to find themselves. There are no longer any colonial administrators. The various specialists I have mentioned learn, if they listen and are given enough time, what are the implications of

the changes they are seeking to produce, and what has been the general effect on societies of simple technology of the stream of changes to which each of them is making his contribution. But this is not applying anthropology; it is applying one or other specialist technique with a little background knowledge of the human reactions to be expected.

Rather more was claimed in the heyday of 'applied anthropology', when Malinowski was writing his articles in *Africa*, the organ of a new institution (then) which could be said in a sense to have been founded to promote the subject. The founders of the International African Institute did not think of it as a learned society, but as an organization which should seek knowledge as the basis of enlightened policies. In 1932 they published a research programme the object of which was defined as 'bringing about a better understanding of the factors of social cohesion in original African society, the ways in which these are being affected by the new influences, tendencies towards new groupings and the formation of new social bonds, and forms of co-operation between African societies and western civilization'. This knowledge, it was argued, would enable those in authority 'to foster the growth of a healthy, progressive, organized society', and would assist all those with practical aims in Africa 'in determining the right relations between the institutions of African Society and alien systems of government, education and religion, in preserving what is vital in the former and in eliminating unnecessary conflict between the latter and African tradition, custom and mentality'. The emphasis was on the integration of innovation into an existing framework, with the implication that tradition should be preserved wherever possible and the rate of change be not deliberately accelerated. This was a tenable attitude in a period of economic stagnation and an age when the indefinite continuance of colonial rule was taken for granted. There was room for argument about what innovations were necessary, and here professional anthropologists, with their greater tolerance of customs 'repugnant to humanity and natural justice', were more conservative than the men with practical aims whom their research was expected to assist. In these circumstances there could be no application of anthropology in any meaningful sense; anthropologists were asserting principles that the 'practical man' was committed to reject.

But the African Institute Fellows were pioneers in the kind of

anthropology that is directed towards the study of change rather than the reconstruction of a hypothetical untouched society; and if applied anthropology is indeed the study of social change, they can be said to have introduced it.

What else is it or can it be? Ought we to speak of applied anthropology when what we mean is indicating the social context in which knowledge from some other field is to be applied? Very occasionally an anthropologist has the opportunity of saying 'what will happen if . . .'. An example is Elizabeth Colson's (1950) excellent discussion of the likely consequences of recognizing the right to make wills in a matrilineal society where men who had property – and their sons – were beginning to resent the right of sisters' sons to inherit. But there are not many such; and the government of Zambia still does not recognize wills.

So while I think the study of social change is inseparable from any anthropological work at the present time, I regret that 'Applied Anthropology' should have come to be thought of as an independent subject. There is little harm in making it an examination option – except when examiners demand that the questions set should *not* include any on social change. But it is dangerously misleading to let people think they can study 'applied anthropology' as a short cut to some kind of welfare activity without going through the grind of mastering the principles of social structure.

APPROACHES TO THE STUDY OF SOCIAL CHANGE

To any British anthropologist social change is structural change. We have not found it profitable to see the process in terms of the acceptance or rejection of cultural traits, nor yet of the differential adaptability of different social systems, an approach which has something in common with theories about factors favourable or otherwise to acculturation. American political scientists have sought to predict the type of political system likely to develop in different African states from the characteristics of traditional systems, an exercise that might be more fruitful if any of the new states discussed comprised only one traditional system. Apter (1961) has divided traditional systems into those with instrumental and those with consummatory values. The instrumental are what Max Weber would have called rational, and it seems that Apter postulates a normal evolution from the consummatory to the instrumental, which accompanies the establishment of strong

central power. But what was important on the threshold of independence, according to his theory, was what stage a given people had reached. From this one should be able to calculate where to expect parliamentary government in a federal system, where 'cabinet dictatorship', and so on. Alas, the recent history of most African states has demonstrated that their first preoccupation is not with details of constitutions.

If, however, discussion is limited to the narrower field of the fate of traditional authority in a situation where all social relationships are rapidly changing, Apter's comparison of Buganda with Ashanti could be more fruitful. But a French anthropologist, Jacques Lombard (1967), who has relied largely on Apter's detailed histories for information about chiefs in English-speaking territories, nevertheless rejects his interpretation of the events in terms of the traditional value systems. To Lombard other factors are equally or more important: in particular the question whether the new leaders come from within or outside the traditional *élite*. In Buganda, as in Northern Nigeria, they were members of it; but in Buganda, as Apter himself recognizes, the ruling class was a bureaucracy recruited on principles which owed nothing to tradition. Lombard remarks too that the 'new men' in whose hands the fate of the traditional authorities lay did not necessarily come from the same ethnic group or from groups with similar values, and their attitudes were formed by influences extraneous to any African system, notably in the case of the Francophone leaders who were brought together in Paris because of the French idea of what the advance towards self-government should be.

British anthropologists would find the latter type of interpretation more congenial. It is perhaps our tradition of intensive fieldwork that leads us to see social change as the cumulative effect of individual responses to new situations, where Herskovits saw cultures rejecting alien instrusions and Apter sees value systems endowing whole societies with rigidity or flexibility. We look rather at the new opportunities that present themselves to *individuals*, and ask what choice they make and why, and we tend to find that it is the existing situation of the individual, not of the society or the culture, that makes him choose one way or another; and that all do not make the same choice.

As Malinowski's injunction to 'weigh, measure and count' has been followed by fieldworkers with a mastery of statistical

techniques, we have been able quantitatively to document significant aspects of social change. This has been done sometimes by the use of records, as with changes in the rates of divorce or polygyny; occasionally by a 'replication study' such as that which Garbett (1960) made of five Shona villages, taking up an earlier analysis and showing that over ten years the rate of migration to labour centres had doubled without any corresponding change in the social structure as measured by the proportion of kinsmen of different kinds; or by the return of an anthropologist to the field he visited a generation earlier. Not many anthropologists have had, or sought, an opportunity to measure social change in this way, but Firth (1959) on his second visit to Tikopia was able to document social and economic change in detail by comparison with the data recorded on his first field trip.

In 1929 Tikopia had virtually no contact with the outside world apart from missionary activity. In 1948 the regular recruiting of labour for work on other islands had begun, and already by 1952 this had had marked consequences. Population had grown and resources diminished, even without the dramatic effects of hurricanes shortly before Firth's arrival. His tables document changes in the distribution of economic resources, in exchange rates for different commodities, in the area of land cultivated and type of crop, in the siting and naming of houses; and change – or rather continuity – in lineage structure.

When we seek to explain the direction of change, most of us make assumptions derived from 'naïve introspection': that most men aspire to some level of material wealth and comfort, that all care for the esteem of their fellows, that many compete for power or prestige. Men have interpreted their own actions and those of others in this way for as long as we have documents to testify to it, and in the practice of our other major principle, 'participant observation', we can learn what reasons our acquaintances give for the choices they make and what motives they ascribe to their neighbours. To psychologists this is no doubt an amateurish way of proceeding, but no other way is open to an anthropologist who has not also studied this other discipline; and while there is no reason why any individual should not combine the study of psychology with that of anthropology, as others have profitably combined economics or law, there is equally no obligation to make this everyone's supplementary subject. We have to do our

best with the tools of our craft, extending our knowledge outside it in the direction in which we feel the most need. In this particular context a psychologist could no doubt teach us to refine our methods if he wished to, but I am not aware that any of them has made the offer.

Moreover, it sometimes seems that the contribution of psychologists in this field, with its emphasis on early conditioning, makes the explanation of change more difficult than it need be. Of course every society is maintained by pressures towards conformity, but fortunately these are not so all-pervading that they can only be resisted by creating the chaos that was described as preferable by Leach in the Reith Lectures for 1967. Curiously enough, it was an *obiter dictum* of the Reith Lecturer himself, to the effect that in a lineage system descent is fixed but marriage allows choice, that made me think of social change as primarily a matter of extending the range of options. Of course there is nothing original in this. Economists with their gaze fixed on the allocation of resources think of nothing else.

The theory of traditional 'patterns' which somehow determine the form of new institutions can be very misleading, and is so particularly when the leaders of new states seek to demonstrate that their ways of meeting unprecedented problems accord with traditional values. Democracy which does not permit opposition is, they argue, the African kind of democracy. Socialism is the right kind of organization for Africa because property traditionally belonged to descent groups, land nationalisation desirable because 'land always belonged to the chiefs'. These analogies may be pragmatically useful in securing popular consent to political decisions, but they can only obscure the real nature of contemporary problems.

The essays that I wrote during the period of transfer of power in Africa may seem to be concerned, like Apter's political predictions, with problems that history has swept under the carpet. If they are worth preserving, it is because they approach African questions not as matters peculiarly African but as matters essentially human. Of course anthropologists are largely concerned with the study of societies organised very differently from their own, but they fail in their duty if they do not recognize, and make it clear to the world, that the members of these societies are not a different kind of being from themselves. It is their first task

to show that, whereas anyone's calculation of advantage must depend on the social norms which constrain him, people in any society make these calculations in an equally rational manner. If you understand the rules of the game you can see what people are likely to do. This is what Huckleberry Finn did when the horse was lost and, saying to himself 'If I were a horse, I'd . . .', he went out and found it. The use of this phrase to epitomise the ethnocentric man who assumes that every rational person must *act* – not calculate – exactly as he does is a shocking measure of the illiteracy of contemporary anthropologists. Of my political essays the one that still retains significance is that on *Race, Tribe and Nation*, which I hope may do something to counter the view that 'tribalism' is not only a moral defect but a defect peculiar to Africa.

An aspect of social change that attracted some attention at the end of the Second World War was the efflorescence in the South Pacific of millenarian movements to which the name 'cargo cult' became attached. As an African specialist I might never have heard of them but for the accident that I spent twelve months with an organization which was training cadets for service with the military administration of New Guinea; and I might never have thought of attempting to compare them with other instances of millenarism if I had not been drafted into an LSE team committed to produce a series of papers on religion for a meeting of the Association of Social Anthropologists. It was innocently supposed in those days that a collection of papers by the members of a single department could be expected to have a common approach to the subject discussed.

Not much had been written on the subject at that time, but just about that time a number of people began to write independently about it, somehow under the influence of a *Zeitgeist* I suppose. In the colonial field it did seem that resentment against alien rule among populations which either did not understand political methods of organized resistance or were effectively debarred from these found expression in such movements; and the interest of students may have been stimulated by the revulsion against maintenance of colonial rule that was so strongly felt in the immediate post-war period. The movements described by Balandier (1955) in the (then) French and Belgian Congo dated from long before the war, as did others in Uganda; the latter were not described in any detail until Welbourn wrote about them some years later.

Worsley (1957) sought to link all the movements in the Pacific by a Marxist explanation, but accounts of them were coming out in such numbers that even his diligence did not keep up with them all. More recently Lawrence (1964), Burridge (1960), and Schwartz (1962) have published detailed accounts of particular areas.

At the same time historians and others have been examining comparable phenomena in the past. Their interest has not always been the same as the anthropologists'. Cohn's illuminating *Pursuit of the Millenium* (1957) looks at mediaeval messianism as the forerunner of modern totalitarianism, but it also offers for anthropologists data on the social background of messianism which they could not have obtained from their own researches. From Germany we have a compendium of millenary beliefs from all continents with an interpretation by Mühlmann (1961) from the social psychologist's point of view. His remark that for a millenary prophet to be accepted there must be a predisposing psychological condition among his hearers parallels in an interesting way the reports from New Guinea of populations in a state of expectancy, waiting for a message, experiencing the trembling fits associated with the cargo movements at their height, or asking, as someone asked Firth, whether he had 'brought the things' – the phenomena that Firth (1955) has described as 'a cargo cult type of behaviour without a cargo cult'.

THEORETICAL QUESTIONS

Malinowski sent me to study social change because, he said, I didn't know enough anthropology for fieldwork of the standard type. Nobody today regards the study of social change as an occupation for the half-baked. But this is rather the status of applied anthropology. Students who have not been interested in generalisations about the nature of society, or have found these hard to grasp, are apt to be recommended by their teachers as 'better on the applied side'.

Nevertheless, if one is associated with the subject for long enough, theoretical interests keep breaking in, and I have included in this collection two essays which can hardly be said to have any practical application. That on clientship is another contribution to an ASA discussion, this time on social stratification. Again I was drafted and then found only one subject in my mind, which had come there, largely inspired by Jean Buxton's (1958) study of

clientship in the tiny Mandari chiefdoms, when I was trying to picture how individuals in segmentary societies could build themselves up into chiefs. Clientship has also been discussed in West African political systems by Nadel (1942) and M. G. Smith (1960), and in more complex societies in Campbell's (1964) work on the Sarakatsani, Boissevain's (1966) on Sicily, and Bailey's (1963) on the organization of voting in Orissa.

The paper on witchcraft was prepared when I was asked to address anthropologists in Paris on recent work in this country, and does not purport to give anything more than a survey of current theory. Before this volume is published it will have been supplemented and corrected by the contributions to the ASA meeting in honour of Evans-Pritchard. This is a subject of perennial interest, constantly being reinforced by data from new areas such as that of Gelfand (1967) and Crawford (1967) on Rhodesia, and now beginning to profit from the interest of sociologically minded historians in classical and early European records.

I

Applied Anthropology and Development Policies[1]

In the last twenty or thirty years, gallons of ink have been spilt in discussion of the appropriate, or the inevitable, relation between scientific research and social needs, or, in more old-fashioned terms, between 'pure' and 'applied' science. At one extreme, 'pure' science is conceived as the disinterested pursuit of knowledge for its own sake. At the other, Marxist theory asserts that advances in scientific theory occur only in response to the demand for solutions to practical problems, though this does not exclude the value of 'fundamental' research, the bearing of which on practical problems is not immediately obvious to the layman. In the field which this discussion generally covers there is no doubt as to what is meant by applied science. It is the application of principles experimentally established to the production of specific results. In many cases the techniques based on these principles are so well developed that they can be practised by people with no more than an elementary understanding of the principles themselves, and indeed some of them are practised by all of us in everyday life. In others a scientist may be asked to solve a problem which falls in his field, but yet cannot be dealt with by the application of any principle already established; in these cases the functions of the pure and the applied scientist are combined.

Though some would claim to speak in an analogous way of the application of the principles of social anthropology, it must be admitted that the analogy is not a very close one. Indeed, in many quarters it is questioned whether anthropology is a science at all, and among anthropologists themselves there are some who hold that it is not and should not be. It is not, of course, an experimental science; it shares this disability with the other social sciences, apart from psychology, which can be studied experimentally to a

[1] Reprinted from *The British Journal of Sociology*, vol. 7, 1956.

limited extent. It can, however, claim to be something more than that study of the unique product of a particular series of events which Evans-Pritchard appears to have had in mind when he described anthropology as essentially akin to history. It does look for regularities in social behaviour extending beyond the limits of a single society, even if these must be of the 'natural history' type and not of the kind that the physicist can express in an equation. It has investigated the validity of theories of 'primitive communism', 'primitive pacifism' and a specifically 'primitive' type of mentality, and has rejected them in favour of interpretations which ascribe the special characteristics of the simpler societies, not to peculiarities in the nature of the people who compose them, but to the limitations of the techniques and resources at their disposal. This is a general proposition verified by observation, though in the nature of the case not by experiment, and it is of the first importance to anyone whose interest in the organisation of these societies arises from a desire to change it. It is the first step away from the assumption that any resistance to changes which the wise Westerner sees as desirable can be due only to laziness, stupidity, superstition or some other defect of character. The next, and indeed the heart of the matter, is the explanation of the complex of social pressures, of recognized claims and obligations, of values inculcated from childhood, within which every individual reacts to the attempts of strangers to improve his character, his way of life or his standard of living.

What anthropologists write when they are trying to interpret the African (as I shall call him for brevity, since I draw my own examples from Africa) to the 'practical man' is quite different from the kind of thing they write when they are analysing and comparing societies for the benefit of other anthropologists. But in the former case, are they acting like applied scientists, or even like the authors of text-books on applied science?

In one very important respect, they are not. Their books are not 'how to do it' manuals, providing formulae for the manipulation of society as the text-books of applied science do for the manipulation of matter. This is not to be explained simply by the relatively undeveloped state of anthropological theory. Indeed, an important achievement of anthropological analysis has been to show how much less easy it is to reshape society by deliberate action than has sometimes been supposed. The phrase 'social engineering',

which some of us used with confidence a generation ago, is now out of favour.

The difference in the nature of the contribution that we can make towards the solution of practical problems is inherent in the nature of our subject-matter. That of the natural scientist is inert or without volition; in Africa he is asked to show how the swollen-shoot virus can be controlled, to find a profitable cash crop for an area of poor soils, or a reasonably cheap fertiliser, a prophylactic against malaria or a source of energy in a region with no coal. Attached to his solution is a large proviso which he is allowed to take for granted; his prescription will work *provided that people will use it.* Where it directly affects the mode of life of individuals, the people in question are the public in general; where it involves large-scale activities like the supply of power or irrigation, they are the controllers of the public purse. *As a scientist* he can work out the answer and say, 'Take it or leave it'; though if he happens to be by nature a politician, he will try to present it in palatable form. It is not, however, his professional research that tells him what is or is not palatable; his views on this subject will be drawn from incursions as an amateur into the social field. If, however, he fails to persuade, he has not failed in his task as a scientist; and he always has to face the possibility that a government may decide that what he recommends is too costly for the available resources.

The anthropologist's field of study is society. He cannot deal with a smaller unit than a number of persons linked by a network of socially recognized relationships, and his subject-matter is, not even the persons as organisms, but the completely immaterial relations – of claim and obligation, right and duty, superiority and subordination – that exist between them. If these can be manipulated, and some anthropologists do use the word, it must be in a very different sense from that in which iron ore is treated to make a steel girder or even malaria parasites killed in a human body.

They can, of course, be changed by external influences – directly by penalizing customary actions and imposing new obligations, indirectly by offering new opportunities. The civilizing mission of Europeans in the tropics, as it used to be called, the diffusion of technical assistance to underdeveloped areas, as it is called today, consists precisely in these processes. In the early period, the emphasis was on the whole more moral, in the latter it is more

technological; though, at any rate in those territories for which the United Kingdom is responsible, we are as much interested today in making people democratic as our grandfathers were in making them Christian.

Some of the anthropologists who have given special attention to the social changes that these influences produce describe their work by the adjective 'applied'. The word recalls the confident 'social engineering' days in which it was born, and the fact that, historically, the founders of the International African Institute, the first body to sponsor studies of social change in Africa, expected the research which it promoted to bear fruit in enlightened policies. Predisposed to a sympathetic interpretation of African institutions and to those policies which sought to build on and develop these institutions rather than abruptly substitute others of European type, they expected that intensive field studies would provide governments with the data they were already looking for. To a large extent, they did so. They greatly increased the range of knowledge about the structure and operations of African political systems and about the nature of African civil law, notably in relation to land rights and marriage.

To administrators, however, the operative words have always been 'build' and 'develop', and it is here that the role of anthropologists becomes more difficult. Trained as they are to approach human institutions in an analytical spirit and to recognize how much all moral judgments are culturally conditioned, they do not necessarily share the administrators' assumptions as to what constitutes progress. On the other hand, they do not follow Westermarck's theory of ethical relativity to its logical conclusion of complete neutrality between different policies. When they have argued that the improvements which administrators have sought to make in the institutions of subject peoples were in fact no improvement, they have not taken their stand on the ground that there would be nothing to choose between the first state and the last, but have defended traditional institutions in terms of values shared by themselves and the administrators. Some have held the view that the colonial status is wrong in itself, and have argued that to integrate tribal political authorities in a colonial administrative system is a way of perpetuating this status and so deserves no assistance from them (cf. Firth 1938, pp. 195–7). Some have combined both attitudes. The second has something in common with