

I

Rethinking Anthropology

LET me begin by explaining my arrogant title. Since 1930 British Social Anthropology has embodied a well defined set of ideas and objectives which derive directly from the teaching of Malinowski and Radcliffe-Brown—this unity of aim is summed up in the statement that British social anthropology is *functionalist* and concerned with *the comparative analysis of social structures*. But during the last year or so it has begun to look as if this particular aim had worked itself out. Most of my colleagues are giving up the attempt to make comparative generalizations; instead they have begun to write impeccably detailed historical ethnographies of particular peoples.

I regret this new tendency for I still believe that the findings of anthropologists have general as well as particular implications, but why has the functionalist doctrine ceased to carry conviction? To understand what is happening in social anthropology I believe we need to go right back to the beginning and *rethink* basic issues—really elementary matters such as what we mean by marriage or descent or the unity of sibs, and that is difficult—for basic concepts are basic; the ideas one has about them are deeply entrenched and firmly held.

One of the things we need to recognize is the strength of the empirical bias which Malinowski introduced into social anthropology and which has stayed with us ever since. The essential core of social anthropology is fieldwork—the understanding of the way of life of a single particular people. This fieldwork is an extremely personal traumatic kind of experience and the personal involvement of the anthropologist in his work is reflected in what he produces.

When we read Malinowski we get the impression that he is stating something which is of *general* importance. Yet how can this be? He is simply writing about Trobriand Islanders. Somehow he has so assimilated himself into the Trobriand situation that he is able to make the Trobriands a microcosm of the whole primitive world. And the same is true of his successors; for Firth, *Primitive Man* is a Tikopian, for Fortes, he is a citizen of Ghana. The existence of this prejudice has long been recognized but we have paid inadequate attention to its consequences. The difficulty of achieving comparative generalizations is directly linked with the problem of escaping from ethnocentric bias.

As is appropriate to an occasion when we honour the memory of Bronislaw Malinowski, I am going to be thoroughly egotistical. I shall imply my own merit by condemning the work of my closest friends. But there is method in my malice. My purpose is to distinguish between two rather similar varieties of comparative generalization, both of which turn up from time to time in contemporary British social anthropology. One of these, which I dislike, derives from the work of Radcliffe-Brown; the other, which I admire, derives from the work of Lévi-Strauss. It is important that the differences between these two approaches be properly understood, so I shall draw my illustrations in sharp contrast, all black and all white. In this harsh and exaggerated form Professor Lévi-Strauss might well repudiate the authorship of the ideas which I am trying to convey. Hence my egotism; let the blame be wholly mine.

My problem is simple. How can a modern social anthropologist, with all the work of Malinowski and Radcliffe-Brown and their successors at his elbow, embark upon generalization with any hope of arriving at a satisfying conclusion? My answer is quite simple too; it is this: *By thinking of the organizational ideas that are present in any society as constituting a mathematical pattern.*

The rest of what I have to say is simply an elaboration of this cryptic statement.

First let me emphasize that my concern is with *generalization*, not with *comparison*. Radcliffe-Brown maintained that the objective of social anthropology was the 'comparison of social structures'. In explaining this he asserted that when we distinguish and compare different types of social structure we are doing the same kind of thing as when we distinguish different kinds of sea shell according to their structural type (Radcliffe-Brown, 1953, p. 109). *Generalization* is quite a different kind of mental operation.

Let me illustrate this point.

Any *two* points can be joined by a straight line and you can represent this straight line mathematically by a simple *first* order algebraic equation.

Any *three* points can be joined by a circle and you can represent this circle by a quadratic or *second* order algebraic equation.

It would be a *generalization* to go straight on from there and say: any *n* points in a plane can be joined by a curve which can be represented by an equation of order *n-1*. This would be just a guess, but it would be true, and it is a kind of truth which no amount of comparison can ever reveal.

Comparison and generalization are both forms of scientific activity, but different.

Comparison is a matter of butterfly collecting—of classification, of the arrangement of things according to their types and subtypes. The followers of Radcliffe-Brown are anthropological butterfly collectors and their approach to their data has certain consequences. For example, according to Radcliffe-Brown's principles we ought to think of Trobriand society

as a society of a particular structural type. The classification might proceed thus:

- Main Type: societies composed of unilineal descent groups.
 Sub-type: societies composed of matrilineal descent groups.
 Sub-sub-type: societies composed of matrilineal descent groups in which the married males of the matrilineage live together in one place and apart from the females of the matrilineage,

and so on.

In this procedure each class is a sub-type of the class immediately preceding it in the tabulation.

Now I agree that analysis of this kind has its uses, but it has very serious limitations. One major defect is that it has no logical limits. Ultimately every known society can be discriminated in this way as a sub-type distinct from any other, and since anthropologists are notably vague about just what they mean by 'a society', this will lead them to distinguish more and more societies, almost *ad infinitum*.

This is not just hypothesis. My colleague Dr Goody has gone to great pains to distinguish *as types* two adjacent societies in the Northern Gold Coast which he calls LoWiili and LoDagaba. A careful reader of Dr Goody's works will discover, however, that these two 'societies' are simply the way that Dr Goody has chosen to describe the fact that his field notes from two neighbouring communities show some curious discrepancies. If Dr Goody's methods of analysis were pushed to the limit we should be able to show that every village community throughout the world constitutes a distinct society which is distinguishable as a type from any other (Goody, 1956b).

Another serious objection is that the typology makers never explain why they choose one frame of reference rather than another. Radcliffe-Brown's instructions were simply that 'it is necessary to compare societies with reference to one particular aspect . . . the economic system, the political system, or the kinship system' . . . this is equivalent to saying that you can arrange your butterflies according to their colour, or their size, or the shape of their wings according to the whim of the moment, but no matter what you do this will be science. Well perhaps, in a sense, it is; but you must realize that your prior arrangement creates an initial bias from which it is later extremely difficult to escape (Radcliffe-Brown, 1940, p. xii).

Social anthropology is packed with frustrations of this kind. An obvious example is the category opposition patrilineal/matrilineal. Ever since Morgan began writing of the Iroquois, it has been customary for anthropologists to distinguish unilineal from non-unilineal descent systems, and among the former to distinguish patrilineal societies from matrilineal societies. These categories now seem to us so rudimentary and obvious that it is extremely difficult to break out of the straitjacket of thought which the categories themselves impose.

Yet if our approach is to be genuinely unbiased we must be prepared to consider the possibility that these type categories have no sociological significance whatsoever. It *may* be that to create a class labelled *matrilineal societies* is as irrelevant for our understanding of social structure as the creation of a class *blue butterflies* is irrelevant for the understanding of the anatomical structure of lepidoptera. I don't say it is so, but it may be; it is time that we considered the possibility.

But I warn you, the rethinking of basic category assumptions can be very disconcerting.

Let me cite a case. Dr Audrey Richards's well-known contribution to *African Systems of Kinship and Marriage* is an essay in Radcliffe-Brownian typology making which is rightly regarded as one of the 'musts' of undergraduate reading (Richards, 1950).

In this essay Dr Richards asserts that 'the problem' of matrilineal societies is the difficulty of combining recognition of descent through the woman with the rule of exogamous marriage, and she classifies a variety of matrilineal societies according to the way this 'problem' is solved. In effect her classification turns on the fact that a woman's brother and a woman's husband jointly possess rights in the woman's children but that matrilineal systems differ in the way these rights are allocated between the two men.

What I object to in this is the prior category assumptions. Men have brothers-in-law in all kinds of society, so why should it be assumed from the start that brothers-in-law in matrilineal societies have special 'problems' which are absent in patrilineal or bilateral structures? What has really happened here is that, because Dr Richards's own special knowledge lay with the Bemba, a matrilineal society, she has decided to restrict her comparative observations to matrilineal systems. Then, having selected a group of societies which have nothing in common except that they are matrilineal, she is naturally led to conclude that matrilineal descent is *the* major factor to which all the other items of cultural behaviour which she describes are functionally adjusted.

Her argument I am afraid is a tautology; her system of classification already implies the truth of what she claims to be demonstrating.

This illustrates how Radcliffe-Brown's taxonomic assumptions fit in with the ethnocentric bias which I mentioned earlier. Because the type-finding social anthropologist conducts his whole argument in terms of particular instances rather than of generalized patterns, he is constantly tempted to attach exaggerated significance to those features of social organization which happen to be prominent in the societies of which he himself has first hand experience.

The case of Professor Fortes illustrates this same point in rather a different way. His quest is not so much for types as for prototypes. It so happens that the two societies of which he has made a close study have certain similarities of structural pattern for, while the Tallensi are patri-

lineal and the Ashanti matrilineal, both Tallensi and Ashanti come unusually close to having a system of double unilineal descent.

Professor Fortes has devised a special concept, 'complementary filiation', which helps him to describe this double unilineal element in the Tallensi/Ashanti pattern while rejecting the notion that these societies actually possess double unilineal systems (Fortes, 1953, p. 33; 1959b).

It is interesting to note the circumstances which led to the development of this concept. From one point of view 'complementary filiation' is simply an inverse form of Malinowski's notion of 'sociological paternity', as applied in the matrilineal context of Trobriand society. But Fortes has done more than invent a new name for an old idea; he has made it the corner stone of a substantial body of theory and this theory arises logically from the special circumstances of his own field experience.

In his earlier writings the Tallensi are often represented as having a somewhat extreme form of patrilineal ideology. Later, in contrast to Rattray, Fortes placed an unambiguously matrilineal label upon the Ashanti. The merit of 'complementary filiation', from Fortes's point of view, is that it is a concept which applies equally well to both of these contrasted societies but does not conflict with his thesis that both the Tallensi and the Ashanti have systems of unilineal descent. The concept became necessary to him precisely because he had decided at the start that the more familiar and more obvious notion of double unilineal descent was inappropriate. In retrospect Fortes seems to have decided that double unilineal descent is a special development of 'complementary filiation', the latter being a feature of all unilineal descent structures. That such category distinctions are contrived rather than natural is evident from Goody's additional discrimination. Goody asserts that the LoWiili have 'complementary descent rather than a dual descent system'. Since the concept of 'complementary filiation' was first introduced so as to help in the distinction between 'filiation' and 'descent' and since the adjective 'complementary' cannot here be given meaning except by reference to the word 'descent', the total argument is clearly tautologous (Fortes, 1945, pp. 134, 200f; 1950, p. 287; 1953, p. 34; 1959; Goody, 1956b, p. 77).

Now I do not claim that Professor Fortes is mistaken, but I think he is misled by his prior suppositions. If we are to escape both from typology making and from ethnocentric bias we must turn to a different kind of science. Instead of comparison let us have generalization; instead of butterfly collecting let us have inspired guesswork.

Let me repeat. Generalization is inductive; it consists in perceiving possible general laws in the circumstances of special cases; it is guesswork, a gamble, you may be wrong or you may be right, but if you happen to be right you have learnt something altogether new.

In contrast, arranging butterflies according to their types and sub-types is tautology. It merely reasserts something you know already in a slightly different form.

But if you are going to start guessing, you need to know *how* to guess. And this is what I am getting at when I say that the form of thinking should be mathematical.

Functionalism *in a mathematical* sense is *not* concerned with the interconnections between parts of a whole but with the principles of operation of partial systems.

There is a direct conflict here with the dogmas of Malinowski and Radcliffe-Brown. Malinowski's functionalism required us to think of each Society (or Culture, as Malinowski would have put it) as a totality made up of a number of discrete empirical 'things', of rather diverse kinds—e.g. groups of people, 'institutions', customs. These 'things' are functionally interconnected to form a delicately balanced mechanism rather like the various parts of a wrist watch. The functionalism of Radcliffe-Brown was equally mechanical though the focus of interest was different.

Radcliffe-Brown was concerned, as it were, to distinguish wrist watches from grandfather clocks, whereas Malinowski was interested in the general attributes of clockwork. But *both* masters took as their starting point the notion that a culture or a society is an empirical whole made up of a limited number of readily identifiable parts and that when we compare two societies we are concerned to see whether or not the same kinds of parts are present in both cases.

This approach is appropriate for a zoologist or for a botanist or for a mechanic but it is *not* the approach of a mathematician nor of an engineer and, in my view, the anthropologist has much in common with the engineer. But that is *my* private bias. I was originally trained as an engineer.

The entities which we call societies are not naturally existing species, neither are they man-made mechanisms. But the analogy of a mechanism has quite as much relevance as the analogy of an organism.

This is not the place to discuss the history of the organic analogy as a model for Society, but its arbitrariness is often forgotten. Hobbes, who developed his notion of a social organism in a very systematic way, discusses in his preface whether a mechanical or an organic analogy might be the more appropriate for his purpose. He opts for an organism only because he wants to include in his model a metaphysical prime mover (i.e. God—Life Force) (Hobbes, 1957, p. 5). In contrast Radcliffe-Brown employed the organic analogy as a matter of dogma rather than of choice (e.g. Radcliffe-Brown, 1957, pp. 82–86; 1940a, pp. 3, 10) and his butterfly collecting followers have accepted the appropriateness of the phrase 'social organism' without serious discussion. Against this complacency I must protest. It is certainly the case that social scientists must often resort to analogy but we are not committed to one type of model making for all eternity.

Our task is to understand and explain what goes on in society, how societies work. If an engineer tries to explain to you how a digital computer

works he doesn't spend his time classifying different kinds of nuts and bolts. He concerns himself with principles, not with things. He writes out his argument as a mathematical equation of the utmost simplicity, somewhat on the lines of: $0 + 1 = 1$; $1 + 1 = 10$.

No doubt this example is frivolous; such computers embody their information in a code which is transmitted in positive and negative impulses denoted by the digital symbols 0 and 1. The essential point is that although the information which can be embodied in such codes may be enormously complex, the basic principles on which the computing machines work is very simple. Likewise I would maintain that quite simple mechanical models can have relevance for social anthropology despite the acknowledged fact that the detailed empirical facts of social life display the utmost complexity.

I don't want to turn anthropology into a branch of mathematics but I believe we can learn a lot by starting to think about society in a mathematical way.

Considered mathematically society is not an assemblage of things but an assemblage of variables. A good analogy would be with that branch of mathematics known as topology, which may crudely be described as the geometry of elastic rubber sheeting.

If I have a piece of rubber sheet and draw a series of lines on it to symbolize the functional interconnections of some set of social phenomena and I then start stretching the rubber about, I can change the manifest shape of my original geometrical figure out of all recognition and yet clearly there is a sense in which it is the *same* figure all the time. The constancy of pattern is not manifest as an objective empirical fact but it is there as a mathematical generalization. By analogy, generalized structural patterns in anthropology are not restricted to societies of any one manifest structural type.

Now I know that a lot of you will tell me that topology is one of those alarming scientific mysteries which mere sociologists had best avoid, but I am not in fact proposing anything original. A very good simple account of the nature of *topology* appears in an article under that title in the current edition of the *Encyclopaedia Britannica*. The author himself makes the point that because topology is a non-metrical form of mathematics it deserves especial attention from social scientists.

The fundamental variable in topology is the degree of connectedness. Any closed curve is 'the same as' any other regardless of its shape; the arc of a circle is 'the same as' a straight line because each is open ended. Contrariwise, a closed curve has a greater degree of connectedness than an arc. If we apply these ideas to sociology we cease to be interested in particular relationships and concern ourselves instead with the regularities of pattern among neighbouring relationships. In the simplest possible case if there be a relationship p which is intimately associated with another relationship q then in a topological study we shall not concern ourselves