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Rethinking Anthropology

by

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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 siblings, 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

with the particular characteristics of p and q but with their mutual characteristics, i.e. with the algebraic ratio p/q . But it must be understood that the relationships and sets of relationships which are symbolized in this way cannot properly be given specific numerical values. The reader should bear this point in mind when he encounters the specimens of pseudo-mathematics which occur later in this paper.

All propositions in topology can also be expressed as propositions in symbolic logic (see Carnap, 1958, chapter G) and it was probably a consideration of this fact which led Nadel to introduce symbolic logic into his last book (Nadel, 1957). My own view is that while the consideration of mathematical and logical models may help the anthropologist to order his theoretical arguments in an intelligent way, his actual procedure should be non-mathematical.

The relevance of all this to my main theme is that the *same* structural pattern may turn up in *any* kind of society—a mathematical approach makes no prior assumption that unilineal systems are basically different from non-unilineal systems or patrilineal structures from matrilineal structures. On the contrary, the principle of parity leads us to discount all rigid category distinctions of this kind.

Let me try to illustrate my point with an example. To be appropriate for the occasion I shall take my example from Malinowski.

Most of you will know that Malinowski reported, as a fact of empirical ethnography, that the Trobrianders profess ignorance of the connection between copulation and pregnancy and that this ignorance serves as a rational justification for their system of matrilineal descent. From the Trobriand point of view 'my father' (*tama*) is not a blood relative at all but a kind of affine, 'my mother's husband' (Malinowski, 1932a, p. 5).

However, alongside their dogmatic ignorance of the facts of life, Trobrianders also maintain that every child should resemble its mother's husband (i.e. its father) but that no child could ever resemble a member of its own matrilineal kin.

Malinowski seems to have thought it paradoxical that Trobrianders should hold both these doctrines at the same time. He was apparently bemused by the same kind of ethnocentric assumptions as later led a Tallensi informant to tell Professor Fortes that 'both parents transmit their blood to their offspring, *as can be seen from the fact* that Tallensi children may resemble either parent in looks' (Fortes, 1949, p. 35; my italics). This is mixing up sociology and genetics. We *know*, and apparently the Tallensi assume, that physical appearance is genetically based, but there is no reason why primitive people in general should associate ideas of genetic inheritance with ideas about physical resemblance between persons. The explanation which the Trobrianders gave to Malinowski was that a father impresses his appearance on his son by cohabiting repeatedly with the mother and thereby 'moulding' (*kuli*) the child in her womb (Malinowski, 1932a, p. 176) which is reminiscent of the Ashanti

view that the father shapes the body of his child as might a potter (Rattray, 1929, p. 9). This Trobriand theory is quite consistent with the view that the father is related to the son only as mother's husband—that is, an affine and not as a kinsman.

There are other Trobriand doctrines which fall into line with this. The father's sister is 'the prototype of the lawful woman' (Malinowski, 1932a, p. 450) which seems to be more or less the equivalent of saying that the father (*tama*) is much the same sort of relation as a brother-in-law. Again, although, as Powell has shown (Powell, 1956, p. 314), marriage with the father's sister's daughter is rare, the Trobrianders constantly assured Malinowski that this was a very right and proper marriage. Evidently in their view the category *tama* (which includes both father and father's sister's son) is very close to that of *lubou* (brother-in-law) (Malinowski, 1932a, pp. 86, 451). The similarity is asserted not only in verbal expression but also in the pattern of economic obligation, for the harvest gift (*urigubu*) paid by a married man is due *both* to his mother's husband (*tama*) and to his sister's husband (*lubou*) (Malinowski, 1935, I, pp. 386, 413-18).

From my point of view this cluster of Trobriand beliefs and attitudes is a 'pattern of organizational ideas'—it specifies a series of categories, and places them in a particular relationship with one another as in an algebraic equation. But Malinowski was biased by his down to earth empiricism, by European prejudices and by his interest in psycho-analysis, and he refused to accept the Trobriand doctrine at its face value. Instead he refurbished his concept of 'sociological paternity' which he had originally devised to fit a quite different context, that of patrilineal organization among the Australian Aborigines (Malinowski, 1913, p. 170-83).

On this earlier occasion Malinowski had used 'sociological paternity' to show how relations between parents and children and between spouses derive from customary rules and not from any universal facts of biology or psychology, but in the later application of these ideas to Trobriand circumstances he shifts his ground and the argument becomes confused by the introduction of naive psychological considerations.

On the face of it 'sociological paternity', as used in *The Sexual Life of Savages*, seems to mean that even in a society which, like the Trobriands, denies the facts of 'biological paternity', sociological attitudes which pertain to paternity, as *we* understand it, may still be found. So far, so good. But Malinowski goes further than this. Instead of arguing, as in the Australian case, that kinship attitudes have a purely social origin, he now insists that social attitudes to kinship are rooted in universal psychological facts. The paternal relationship contains elements which are necessarily present in the father/child relationship of *all* societies, no matter what the circumstances of custom and social structure may be. This is all very confusing. On the one hand the reader is told quite plainly that the Trobriand child is taught to think of his father as a non-relative, as an

individual with the special non-kinship status of mother's husband. But on the other hand the reader is forced to conclude that this Trobriand mother's husband is related to the mother's child 'as a sociological father', that is to say by ties of kinship as well as by ties of affinity. The argument, as a whole, is self-contradictory.

You may well think that this is a very hairsplitting point to make a fuss about. How can it possibly make any difference whether I think of a particular male as my father or as my mother's husband?

Well, all I can say is that anthropologists do worry about such things. Professor Fortes, Dr Goody and Dr Kathleen Gough are so disturbed by my heretical views on this subject that each of them has recently taken time off to try to bruise my head with their private recensions of Malinowski's argument (Fortes, 1959b; Goody, 1959, pp. 83, 86; Gough, 1959).

The heart of the controversy may be stated thus. To Englishmen it seems obvious that the relation between brothers-in-law is radically different from the relation between father and son. By that we mean that the rights and duties involved in the two cases are quite different *in kind*. The first relation is one of affinity; the second is one of filiation.

It also seems obvious to us that the relation between mother and son, though different from the relation between father and son, is nevertheless of the same general kind—it is again a relation of filiation. Now Fortes and his followers maintain that this is universally the case—that the relations between a child and *either* of its parents are of the same basic kind, relations of filiation. Fortes asserts that it is necessary to maintain this because any other view 'would make the incest taboo nonsensical'. Thus, like Malinowski, he is prepared on dogmatic psychological grounds to repudiate the Trobrianders' views of their own social system (Fortes, 1959b, p. 194).

The contrary approach, which is my heresy, is that we must take each case as it comes. If the Trobrianders say—as they do say both in word and deed—that the relation between a father and his son is much the same as the relation between male cross-cousins and as the relation between brothers-in-law, but absolutely different from the relation between a mother and her child, then we must accept the fact that this is so. And in that case we only delude ourselves and everyone else if we call such a relationship *filiation*.

My disagreement with Professor Fortes on this matter turns on this point. It seems to me that in his use of the term 'complementary filiation' he is trying to establish as a universal a special ethnographic phenomenon which he happens to have observed among the Tallensi and the Ashanti.

For my part I have no anxiety to demonstrate anything. I am interested only in discerning *possible* general patterns in the peculiar facts of particular ethnographies.

Let us see if we can examine this issue, not as a problem of comparative

social structure, nor of verbal polemic, but as a case of generalized (mathematical) structural pattern.

The cardinal principle of Malinowski's anthropological method was that we should view the system as a whole and examine the interconnections between the parts. Thus, in his view, all the following Trobriand facts are closely interconnected:

1. A father is deemed to have no biological connection with his child.
2. A child shares the blood of its mother and her siblings; a father is related to his child as 'mother's husband'.
3. Marriage is virilocal; a boy at marriage sets up house in the hamlet of his mother's brother and his wife joins him there. After marriage brothers and sisters live in different hamlets. They must avoid one another.
4. An individual's own 'blood relatives'—his matrilineal kin—are never suspected of sorcery or witchcraft; affinal relatives, including wives and children, are often so suspected.
5. Children are thought to resemble their fathers but not their mothers.
6. During a man's lifetime his wife's brother gives him an annual gift of food.
7. At his death his lineage kinsmen make large payments to his wife's lineage. All activities connected with the disposal of the corpse are carried out by members of the wife's lineage.

The list of relevant interconnected facts could be extended indefinitely, but these are the items to which Malinowski himself seems to have attached most weight (see Fig. 1).

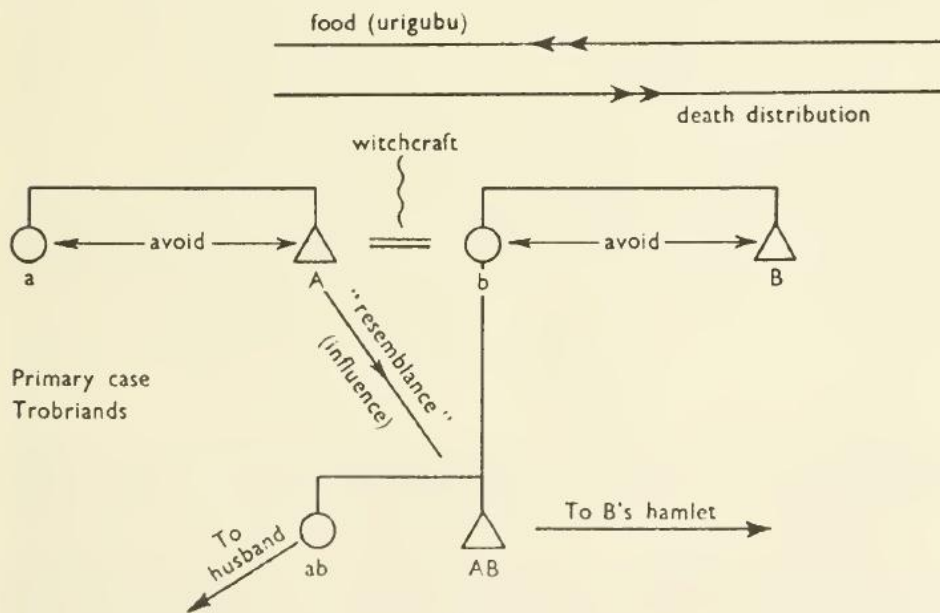


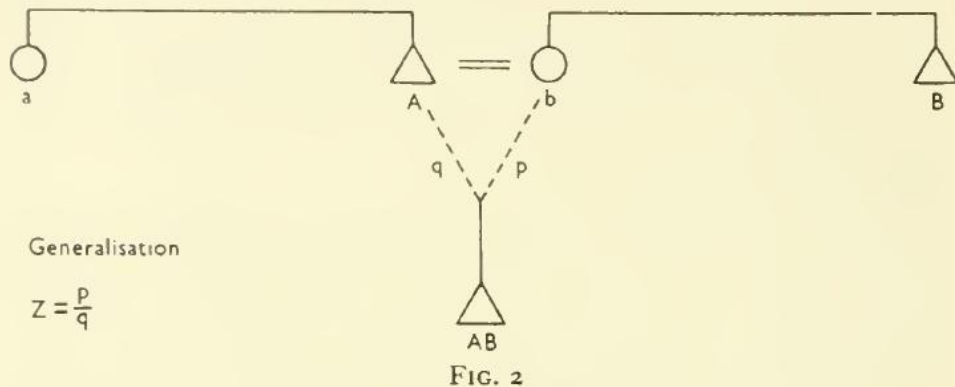
FIG. 1

All of us now accept this principle of the functional interconnection of items of cultural behaviour, but *generalization* calls for an exactly opposite treatment of the data. [If we are to generalize, a small cluster of inter-

connected facts must be treated as an isolate expressing a particular principle of social mechanism.)

Now consider Fig. 2 and regard it as a generalized version of the centre of Fig. 1. I want to consider the relations of filiation *not* in relation to the system as a whole but in relation to one another.

In talking about 'function' in a *generalized* way it is not sufficient to specify relationship between particular empirical facts; we must give a genuine mathematical sense to our concept of function and start thinking in terms of ratios and the variations of ratios.



So now please forget about my list of cultural characteristics and turn your attention to the diagram (Fig. 2). Try to think of this as a mathematical expression and forget for the moment that it was originally derived from Trobriand ethnography. I want to 'generalize' this pattern. Instead of using a value loaded term like *filiation* we will use algebra. Filiation with the father = 'q', filiation with the mother = 'p'.

The ratio p/q is a mathematical function which varies along with variations of 'p' and 'q'. As indicated above I want to think of these items as topological variables rather than as measurable quantities.

If we call this function z it is clear that z has an infinite number of values between 0 and infinity. The Trobriand case evidently represents one extreme:

$$q = 0; p = 1; z = \text{infinity}$$

The opposite extreme would be:

$$p = 0; q = 1; z = 0$$

And there is also an interesting special case somewhere in the middle:

$$q = p; z = 1$$

In the great majority of cases we must expect both 'p' and 'q' to have values but the exceptional cases where either 'p' or 'q' is zero are clearly of great interest.

I am *not* trying to argue that we can use mathematics to solve anthropological problems. What I do claim is that the abstraction of mathematical statement has great virtues in itself. By translating anthropological facts

into mathematical language, however crude, we can get away from excessive entanglement in empirical facts and value loaded concepts.

When mathematicians write down equations it doesn't worry them overmuch whether any particular instance is going to turn out 'real' or 'imaginary', but I am prepared to admit that the only kinds of structural pattern which interest anthropologists are those which actually occur.

Well, do my equations represent real or imaginary situations?

For example, what about $x=0$; $q=1$; $p=0$? Obviously an impossible case, for this would imply a society in which a child is not related to its mother, which is absurd. But wait a minute. Why is it absurd? Why is it more absurd than Malinowski's case where a child is unrelated to its father? Mathematically speaking the two cases are precisely on a par; the virtue of mathematical statement is that it allows us to see at once the similarities of pattern in this sense.

Now the Malinowski of *The Family among the Australian Aborigines* would have accepted this equivalence for he argued quite explicitly that maternity as well as paternity is sociologically determined (Malinowski, 1913, p. 179). But to the later Malinowski, who ridiculed Briffault for his notion of group motherhood (Malinowski, 1930, pp. 134-7), it would certainly have seemed absurd to talk about 'children who are not related to their mothers'. In all his Trobriand writings Malinowski was confused by a bias derived from Freudian psychology which made it impossible for him fully to distinguish relationships of a biological and psychological kind from purely sociological relationships; Malinowski's successors—notably Professor Fortes—have, I believe, been hampered by precisely this same excessive involvement in the empirical facts of the case.

Of what sort of society could we say that a child is unrelated to its mother—in the sense that there is no bond of social filiation between mother and child? Clearly the converse of the Trobriand argument applies. If there is a society in which the relation between a child and its mother is utterly unlike that between a child and its father but has much in common with the relations between cross-cousins and between brothers-in-law, then this mother/child relationship is not sensibly described as one of filiation. It is rather a relationship of affinity traced through the father.

There are many forms of ideology which might form the basis for such a pattern of ideas. The essential requirement is that the 'p' and 'q' relationships should be symbolized as different not only in quality but in kind. The Tikopia are a case in point. They say that the substance of the child originates in the father's semen and derives nothing from the body of the mother. Nevertheless the limbs of the child are fashioned by the Female Deity—a being who seems to be a mystical aspect not only of the mother herself but of her whole patrilineage (Firth, 1936, p. 481).

An analogous contrast is provided by the common Asiatic belief that the bony structure of the child's body derives from the father's semen

while the soft fleshy parts are made of the blood and the milk of the mother (Lévi-Strauss, 1949, ch. xxiv). The North Burma Kachins supplement this with a metaphysical argument. They say that the child acquires its soul (*minla*) only at the moment of birth when it begins to breathe so that this soul is not in any sense derivative from the mother. For that matter the *minla* is not properly speaking hereditary at all; the child acquires this soul from its immediate environment and it is therefore important that a child be born in its father's house (Gilhodes, 1922, pp. 134, 175). Consequently a localized patrilineage is known as a *dap* (hearth), i.e. the persons born and raised in one section of one house.

In the same Assam/Burma societies which emphasize in this way the substantial unity of the child with its father's body and with its father's house, we find that the language of kinship contains a special general category which might be translated as 'affinal relatives on the wife's side'. This category includes not only all the men rated as 'wife's brother' and 'father-in-law' but also all those classed as 'mother's brother' as well as all the women classed as 'mother'. (Examples of such broad affinal categories are the Jinghpaw term *mayu* and the Lakher term *patong*—see chapters 2, 3, and 5 below.)

All these are different ways of asserting both that the 'p' and the 'q' relationships are radically different, and that the maternal relationship approximates to affinity, but this is not enough. Something more than metaphor and metaphysics is necessary if I am to convince you that in these societies the mother/child relationship is in sociological terms one of affinity rather than of filiation.

Fortunately, from my point of view, we possess an extremely detailed ethnography of one of these groups . . . the Lakher (Parry, 1932). Unlike some of their neighbours the Lakher recognize divorce and divorce is frequent. They consider however that the child of a properly married man is exclusively his and that his divorced wife has no rights in the child whatsoever. It follows that if a woman has a son and a daughter by two different husbands the children are deemed to be unrelated to one another. Therefore they may marry without restraint. In contrast, the son and the daughter of one man by two different mothers stand in an incestuous relationship to one another (Parry, 1932, p. 293).

This surely is the case we are looking for. Just as the Trobriands are an extreme case in the sense that the father has no consanguineous ties with his wife's children but is bound only to their mother as an affine, so also the Lakher are an extreme case in the sense that the mother has no kinship ties with her husband's children but is bound only to their father as an affine.

It would of course finally clinch the argument if I could show that the rules allow a Lakher male to marry his own divorced mother, but I am afraid that neither the Lakher nor their ethnographer seem to have considered this bizarre possibility!

However there are a variety of other Lakher customs which support my thesis. For example, the death due (*ru*) (*op. cit.* pp. 418-19) is paid on behalf of a deceased *male* by his eldest son (or other male heir) to his *pupa*, that is, to a male of the deceased's mother's patrilineage. But in the case of a deceased *female* it is paid by her husband to a male of the deceased's own patrilineage. Should her husband be dead it is payable by her youngest son. If we assume that a common logic pervades these substitutions it is evident that the payment is made from males of the husband's lineage (*ngazua*) to males of the wife's lineage (*patong*) and that the payment reasserts the survival of an affinal tie temporally severed by death. But it will be noted that in these transactions a deceased woman's son can act as deputy for her husband, that is to say, the son is deemed to be related to the mother as an affine (*ngazua*).

No *ru* is payable for unmarried persons but a different death due called *chhongchhireu* is, in this case, paid by the father of the deceased to the mother's brother of the deceased; again an indication that the mother's brother is thought of as an affinal relative (*op. cit.* p. 428). Among some Lakher groups still another death due called *chachhai* is payable by the heir of a deceased male to the deceased's wife's brother. The Lakher explained this last institution by saying that 'a man by dying has abandoned his wife so his heir must pay a fine to the dead man's relations as compensation for the inconsiderate conduct in leaving his wife without a protector'. Here again then the obligation is viewed as an aspect of affinity, and not of uterine kinship; the fact that the 'heir' in question would ordinarily be the wife's son is not considered.

Although I cannot demonstrate that the Lakher would tolerate sex relations between a man and his own mother, it is the case that among the very similar Kachin (where divorce is impossible) such relations would be treated as adultery (*shut hpyit*) rather than as incest (*jaiwawng*) (Leach, 1954, p. 137; cf. Goody, 1956a). Also in the contrary case, a Trobriand man may cohabit regularly with his own daughter or step-daughter without committing the sin of incest (*suvasova*) even though such relations are considered morally objectionable on other grounds (Malinowski, 1932a, pp. 445-9). Malinowski says that such relations could never be legitimized as marriage but it is not clear what he means by this. A Trobriand marriage is legitimate when the wife's matrilineal kinsmen pay *urigubu* harvest gifts to the husband (cf. Powell, 1956, p. 349). In the case of a man cohabiting with his own daughter this requirement is fulfilled in any case. The Trobriand moral objection is in fact precisely on these grounds. Since the husband is already receiving *urigubu* payments on account of his wife he cannot expect to have sexual access to the daughter as well (Malinowski, 1932a, p. 446).

We should note that in both the 'extreme' cases the affinal alliance between the lineage of the father and the lineage of the mother is expressed in enduring and elaborately defined economic obligations. The require-

ment that a married Trobriand son should contribute *urigubu* harvest gifts to his father has its counterpart in the payment due from a Lakher male to his mother's brother and his mother's brother's sons. Both sets of payments have their basis in a contract of marriage and are in no way connected with any recognition of common bodily substance (Parry, 1932, p. 244).

That at any rate is my reading of the evidence, though those who disagree with me can doubtless turn the matter back to front. Parry himself, under the influence of Hutton, assumed that the peculiarities of the mother's brother/sister's son relationship, which he recorded for the Lakher, demonstrated 'traces of a very recent matrilineal system' (*op. cit.* p. xiii). Although this evolutionist doctrine seems to me totally mistaken, it is only marginally different from the views currently advocated by Fortes (1959b) and Goody (1959). The latter (pp. 82-3) argues that, in a patrilineal system in which property is transmitted between male agnates, the 'children of the residual siblings' (i.e. the children of the sisters) are, as it were, second class members of the patrilineage—hence the sister's son has 'a shadowy claim' upon the property of the mother's brother by virtue of his mother's position in his mother's brother's patrilineage. There may well be societies where this is so but it seems to me to be going right against the evidence to suggest that the Lakher is one of them.

I maintain on the contrary that the evidence shows unambiguously that the obligations which link a Lakher man to his *pupa* (mother's brother or mother's brother's son) and also to the *pupa* of his mother are part of a complex of economic obligations established by marriage. They are obligations between males of patrilineages linked by marriage alliance and they do *not* have their roots in notions of filiation between mother and son.

The patrilineal Lakher case is not unique of its kind. Long ago Philo reported of the Spartans that a man might marry his mother's daughter by a different father. McLennan, in noting this fact, deemed it incredible and brushed it aside as an obvious ethnographic error (McLennan, 1876, p. 177). Nevertheless McLennan's comments deserve quotation for they show that he fully appreciated the significance of such a case. His text has: '. . . the report of Philo, that the Spartans allowed a man to marry his sister-uterine, but not his sister-german, or by the same father . . . we hold it to be incredible—as discordant with old law as with the habits of the Lacedaemonians'. But to this he adds a footnote: 'The reader may suspect that this is a relic of strict agnatic law. But for the reasons stated in the text, we hold that view to be excluded. *The system of relationship through males only has never, in any well authenticated case, been developed into such a rule as this*' (my italics).

There is also the case of the Tikopia who seem to treat cohabitation between half-siblings of the same father as incestuous, whereas the marriage of half-siblings of the same mother is merely odd (Firth, 1936, p. 330). The facts here are that in Tikopia divorce and widow remarriage are both

uncommon and there is a general dislike of marriage between very close kin so that the possibility of half-sibling domestic unions does not often arise. Firth reports on two cases only. Cohabitation between half-siblings of the same father was tolerated but the union was sterile and strongly disapproved. In contrast, a domestic union of half-siblings of the same mother had produced a large and flourishing family which suffered no stigma.

No doubt the majority of human societies fall somewhere between my two extremes. Usually a child is related to both its parents because of direct ties of filiation and not simply because its parents happen to be married. I agree too that, for a substantial proportion of these intermediate cases, Fortes's concept of 'complementary filiation' may have analytical utility, but the general pattern must include the limiting extremes, so I prefer my algebraic formulation.

In a way this is all very elementary. Those of you who teach social anthropology may protest that, leaving out the algebra, this is the sort of thing we talk about to first year students in their first term. And I agree; but *because* you leave out the algebra, you have to talk about descent and filiation and extra-clan kinship and sociological paternity and so on and your pupils get more and more bewildered at every step. In contrast what I am saying is so easy that even professors ought to be able to understand! It is not algebra that is confusing but the lack of it. After all, you professionals have long been familiar with both the Trobriand and the Kachin ethnographic facts, but I suspect that you have *not* until this moment perceived that they represent two examples of the same pattern—you have been unable to perceive this because you were trapped by the conventional categories of structural classification. Now that I have pointed out the mathematical pattern the similarity is obvious. (Fig. 3, (a), (b).) But let me repeat. I am not telling you to become mathematicians. All I am asking is: Don't start off your argument with a lot of value loaded concepts which prejudge the whole issue.

The merit of putting a statement into an algebraic form is that one letter of the alphabet is as good or as bad as any other. Put the same statement into concept language, with words like paternity and filiation stuck in the middle of it, and God help you!

My time is running short and I don't suppose that I have convinced you as yet that my technique of 'generalization' really tells us anything new, but let me try again.

So far we have dealt with only half the story. My first variable ' x ', which is the ratio between matrification and patrification, corresponds, at an ethnographic level, to variations in the ideology of genetic inheritance.

At the two extremes the Trobriand child derives its substance exclusively from its mother's blood, while the Kachin child is the bony product of its father's semen. In more normal cases where children are filiated to both parents (as with the patrilineal Tallensi) the child gets its physical substance from both parents.

But this does not take into account Malinowski's curious statement that the Trobriand child should resemble its mother's husband and not its mother or any clan relative of the mother. Nor have I explained what Kachins are getting at when they say that the flesh and blood of a child come from its mother, though not its bone.

I won't bother you with the algebra this time but I hope you can see that if we take the Trobriand evidence to be extreme in one direction then the opposite extreme would be a society in which children resembled their mothers but not their fathers. And this is precisely what we do find. The North Burma Kachins have a patrilineal organization very similar to that of the Lakher whom I mentioned just now but, despite their patriliney, they consider that a child should resemble its mother and not its father—the exact antithesis, you see, of Malinowski's case.

In the field this baffled me completely. This was because I had too many empirical facts. The main fact was a prize pig. The Government, at enormous expense, had imported a prize Berkshire boar all the way from England. The villagers were instructed to castrate their own male pigs and have all their sows served by the boar. The boar was a sensation; no-one would talk about anything else—a regular nine days' wonder; but active co-operation in the scheme was virtually nil. It was then that I learnt that Kachin pigs derive all their physical characteristics from the sow; that being so, what on earth is the use of a prize boar?

Matrilineal pigs seemed to me a curious phenomenon so naturally I pursued the matter. I then learnt that the same thing applies to humans too—the mother feeds the child in her womb and at her breast and on that account a man's face (*myi-man*) comes from his maternal affines (this word for face, as in the Chinese equivalent, means 'reputation' as well as 'physiognomy'). The idea that appearances and reputations both come from the mother's side fits in with the idea that wives who are witches can infect by contagion both their husbands and children. The supreme manifestation of this is when a woman dies in childbirth; such a woman is deemed to be a witch of the most noxious kind and in former days all the possessions of her husband's household, including the house itself, had to be burnt so as to disinfect the community.

The crucial point to note here is that witch influence was thought to be transmitted in the food which the woman prepared—the husband was quite as liable to infection as the children. The original sources make it plain that Kachin witchcraft is contagious rather than hereditary. In structural terms Kachin witchcraft is associated with affinity, *not* filiation (Gilhodes, 1922, pp. 182–5, 296; Hanson, 1913, pp. 143f., 173–4; Leach, 1954, pp. 179f.).

If we compare this Kachin case with the Trobriand one it becomes clear that we are concerned with a single pattern of ideas which, in its general form, embodies something *other than* the notion of filiation. In both societies there is a concept of filiation which is thought of as genetic

influence and is symbolized by the dogma of common substance; but there is also something different, the idea of mystical influence which can be independent of *any* tie of blood or bone.

There is more to all this than a mere quibble over the use of words and the interpretations of symbols. Fortes (1959b) has said that 'complementary filiation can be thought of as the kinship reciprocal of affinal relationship in the marriage tie', but this terminology is bound to lead to confusion. In the first place, since the phrase 'complementary filiation' only has meaning in association with unilineal descent, Fortes's argument would imply that affinal relations only occur in the presence of unilineal descent, which is plainly absurd. But secondly, in view of the distinction which Fortes draws between filiation and descent his formula amounts to an assertion that 'affinal relationship in the marriage tie' is a category applicable only to relations between individuals. But empirically this is not the case. The Jinghpaw expression *mayu/dama* and analagous categories elsewhere denote relationships of enduring affinal alliance between whole groups of persons. It is quite misleading to think of such group relationship as 'reciprocal' of any particular relationship between an individual parent and an individual child.

In Chapter 3 of this book I show how such relations of enduring affinal alliance are expressed in the transfer of goods and in notions of differential political status. But here I am referring to something both more general and more metaphysical. My proposition is that the relationship which we denote by the word 'affinity' is very commonly given cultural expression as 'mystical influence', but that this in turn is only a special instance of something more general, the logical opposition between unity through incorporation and unity through alliance.

In each of my examples (Fig. 3) we see that certain ideas cluster together to form a pattern (a topological 'set'), and the elements in the pattern divide up to form a category opposition. Thus with Trobrianders—mystical influence is linked with physical appearance but opposed to blood relationship. With Kachins—mystical influence is linked with physical appearance, flesh and food but opposed to bone relationship. With Tallensi—genetic influence is associated with blood *and* bone *and* physical appearance and can be derived from both parents, but this is opposed to a form of mystical influence called *tyuk* and a tendency to witchcraft, both of which are derived from the mother's relatives only. In this last case the opposed categories overlap but even so, as Fortes shows clearly, the two kinds of influence, the genetic and the mystical, are, in the Tallensi view, quite distinct (Fortes, 1949, p. 35; also index refs. to *yin*).

The category distinctions involved in these different cases are all of much the same kind but they are *not* identical and it would be misleading to try to fit them into a typology by tagging them with precisely defined labels such as filiation, descent and affinity. Instead I suggest that the facts

can be generalized into a formula which would run something like this:

'A marriage creates an alliance between two groups, A and B. The children of the marriage may be related to either or both of these groups by incorporation, either permanent or partial, but they can also be related to either or both the groups by virtue of the marriage alliance itself. The symbols I have been discussing—of bone and blood and flesh and food and mystical influence, discriminate on the one hand between permanent and partial incorporation, and on the other between incorporation and alliance. These are variables which are significant in all societies and not merely in unilineal systems of a particular type.'

The value of such generalization is that it invites us to re-examine familiar material from a fresh point of view. For example, my cases indicate that the distinction between incorporation and alliance is always expressed in the difference between common substance and mystical influence—and surely, this is just what the Tikopia are talking about when a man refers to his own son as *tama* (child) but to the son of his

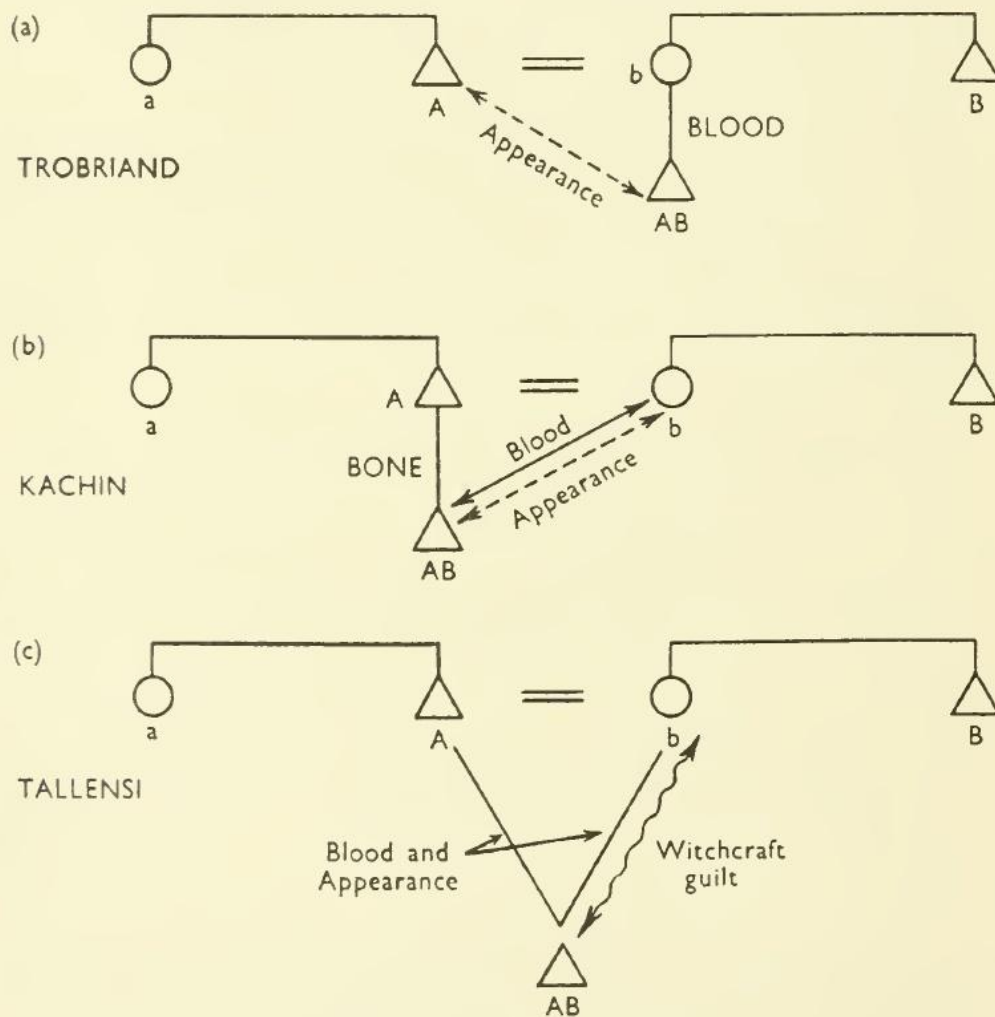


FIG. 3

sister's husband as *tama tapu* (sacred child)? But you won't find that recorded in the pages of *We, the Tikopia*.

Perhaps I may elaborate that point. The exceptional detail of Firth's ethnographic material is a standing invitation for every reader to try to 'rethink' the particular explanations which Firth himself offers us. Firth's discussion of the *tuatina/tama tapu* (mother's brother/sister's son) relationship is very extensive but nowhere does it serve to explain why the latter should carry the epithet 'sacred child'. Firth's general position seems to be similar to that adopted by Goody in the article which I have criticized above; the reader of *We, the Tikopia* gets the impression that the sister's son has a sort of second class membership in Ego's patrilineage (ramage) and that the relationship is one which Fortes and Goody would describe as 'extra-clan kinship'. As I understand it, Firth considers that the gifts which the *tama tapu* receives from his *tuatina* originate in rights of inheritance based in some kind of principle of descent (Firth, 1936, pp. 224-5, 279ff.). Yet this hardly seems consistent with the fact that although a man has certain rights of usufruct in land belonging to his mother's patrilineage, he loses these rights as soon as his mother is dead (*op. cit.* p. 391).

In contrast I would suggest that the description 'sacred child' has a logical fit with the notion that the child is formed in the mother's womb by the Female Deity associated with the mother's patrilineage (*op. cit.* p. 481) and that the same Female Deity has temporary charge over a man's soul during the intricate processes of transition from life to death (Firth, 1955, p. 17). This implies surely that the sister's child has a mystical rather than a substantial link with members of Ego's patrilineage? Firth's meticulously detailed record of Tikopian attitudes towards the *tuatina/tama tapu* relationship seems fully in accord with this. The Tikopia themselves appear to regard this relationship as an affinal link between whole lineages rather than as a simple tie between individuals (Firth, 1936, p. 213).

But let me repeat. Polemic apart, the principal generalized hypothesis which has so far emerged from this essay is that, in *any* system of kinship and marriage, there is a fundamental ideological opposition between the relations which endow the individual with membership of a 'we group' of some kind (relations of incorporation), and those other relations which link 'our group' to other groups of like kind (relations of alliance), and that, in this dichotomy, relations of incorporation are distinguished symbolically as relations of common substance, while relations of alliance are viewed as metaphysical influence.

The first part of this hypothesis has obvious links with the distinction between the 'internal' system and the 'external' system which has been stressed by Homans (1951) and by Fortes (1959b, p. 194). The latter part, though related to Fortes (1959a,) is novel.

At first sight it might be supposed that the proposition is readily dis-

proved, for although it is true that in many societies the threat of supernatural attack ('metaphysical influence') is expected to come from 'outsiders'—notably affinal kin and political associates—there are well known instances where the contrary is the case. Thus, in matrilineal Ashanti, the witch is habitually a lineage kinsman (Rattray, 1927, p. 30) and the same is true of the patrilineal Tiv (Bohannon, 1953, p. 85). Furthermore throughout patrilineal Polynesia it is the father's sister who must be particularly respected lest she invoke supernatural sanctions (Firth, 1936, p. 222; Mabuchi, 1958).

But my proposition is not quite so easily disposed of. The 'mystical influence' which has been discussed in this paper is of the same kind as that which we English denote by the word Fate, which the Tallensi denote by the term *yin*, and which Fortes has distinguished under the phrase 'prenatal destiny' (Fortes, 1959a). It is a power beyond human control. My thesis—and here for once Professor Fortes and I seem to be in agreement—is that in any particular case the ideas concerning such *uncontrolled* mystical influence must serve to specify something about the social structure. An individual is thought to be subject to certain kinds of mystical influence because of the structural position in which he finds himself and not because of the intentional malice or favour of any other individual.

Doctrines of this sort are quite distinct from those which credit particular individuals with the capacity to punish wrongdoers or attack their enemies by secret supernatural means.

Some examples will serve to illustrate this distinction.

In the ideology of Kachin witchcraft the witch is presumed to be an unconscious and involuntary agent; she brings disaster upon her husband and her children, not because she wishes to do so, but because she has the misfortune to be the host of a witch spirit (*hpyi*). She is a person tainted with contagion through no fault of her own and hence (in my terminology) she affects her victims through 'uncontrolled mystical influence'. Contrast with this the Ashanti doctrine which presumes that witches are adult persons, fully conscious of their misdeeds, who receive special training and initiation into their nefarious arts (Rattray, 1927, pp. 28–31).

This Ashanti witchcraft is not 'uncontrolled mystical influence' in my sense of the term but a form of 'controlled supernatural attack'. In this respect it is analogous to such conceptions as the threat of the father's sister's curse in Samoa, or the threat of the chief's sorcery in the Trobriands and Tikopia; the individuals who wield such *controlled* supernatural authority are persons who command respect (Mead, 1930, p. 146; 1934, pp. 309, 310, 314, 356; Firth, 1936, p. 222; Malinowski, 1932b, p. 85f.; Firth, 1959, p. 145).

Monica Wilson's Nyakyusa material brings out this distinction very clearly. In Nyakyusa belief 'good' and 'bad' witchcraft are both regarded as forms of 'controlled supernatural attack', but whereas a bad witch

acquires his witchcraft unconsciously by influence from his father's wife, a good witch ('defender') *acquires* his witchcraft intentionally by taking medicines (Wilson, 1949, pp. 24, 98-102).

Kachin evidence illustrates the same point in a different way. Kachins carry out 'controlled supernatural attack' by invoking the Spirit of Cursing, called *Mātsa Kānu* (Gilhodes, 1922, pp. 292-3). This name is a combination of two kinship categories *tsa* (father-in-law, mother's brother) and *nu* (mother); it embodies a formulation of the Kachin theory that the power of cursing and the power of witchcraft are of the same kind and emanate from the same source—namely the affinal relatives on the mother's side (*mayu*). The witch emits this power *unconsciously* having been infected by an uncontrolled mystical influence: the man who curses an opponent invokes precisely the same power but does so *consciously*.

As a demonstration that my procedure of topological generalization has some practical utility I propose now to develop this distinction so as to provide a gloss on one of the classical topics of anthropological theory.

Anthropologists have a wide and varied range of functionalist explanations as to why custom should so often require a man to display some special, rather bizarre, form of behaviour towards a father's sister or a mother's brother. Mostly these explanations focus in arguments about ambiguities in principles of descent and rights of inheritance (e.g. Goody, 1959). Each type of explanation throws illumination on appropriately selected case material but none of them are at all convincing as contributions to general theory. The material which I have now presented suggests that the whole topic might fruitfully be considered from quite a new point of view, namely the degree of coincidence between notions of 'uncontrolled mystical influence' on the one hand and notions of 'controlled supernatural attack' on the other. These opposed variables may be thought of as forming a topological set.

For brevity let us denote 'uncontrolled mystical influence' by the symbol x and 'controlled supernatural attack' by the symbol y and then consider the incidence of the x and y notions as reported from the societies which we have been discussing throughout this paper.

TIKOPIA:	x and y are separated. x comes from the mother; y from the father's sister.
LAKHER (KACHIN):	x and y coincide, both come from the <i>patong</i> (<i>mayu</i>), that is the mother's brother's patrilineage.
TROBRIANDS:	x and y do not necessarily coincide but may do so. x comes from the father; y comes from affinal relatives (as an expression of malice) or from the chief (as an expression of legitimate authority).
ASHANTI:	x and y are separated. x comes from the father; y from adult women of Ego's matrilineage.
TALLENSI:	x and y are separated. x derives from uterine kin; y from Ego's patrilineal ancestors.

This pattern variation is far from random, for the degree of coincidence between x and y corresponds to the degree to which affinal alliance plays a part in the ongoing political structure of the society. As is shown in Chapters 3 and 5 of this book the Kachin and the Lakher are societies in which the affinal ties of chiefs and lineage headmen have a structural permanence comparable to that provided by the idea of lineage perpetuity in unilineal descent systems. In contrast among the Tikopia, the Tallensi and the Ashanti there are no 'relations of perpetual affinity' which can serve to express enduring political relations of superordination and subordination. But in this respect the Trobrianders provide an intermediate case, for, while they have no ideal of permanent affinal relationship, they use the *urigubu* harvest payment, which is normally an obligation due to affines, as a device for expressing the tributary obligations of a village headman to his chief.

The general inference therefore is that, where x and y coincide, relations of affinity are being used to express political dominance.

The reader who wishes to verify my algebraic generalizations for himself will find the following references useful:

TIKOPIA

Evidence concerning the father's sister's curse and the mystical influence of the Female Deity has been cited above. In Tikopia the form of marriage serves to emphasize its lack of *political* importance. Once a marriage has been established a rather complex set of obligations is set up between the lineage of the husband and the lineage of the wife, but the marriage itself purports to be a 'marriage by capture' in which the parents of the bride remain ignorant of what is afoot until all is *fait accompli*. This marriage by capture is 'characteristic mainly of chiefly families' (Firth, 1936, p. 539), and seems to amount to an explicit denial that the chiefs are using marriage for political ends.

LAKHER

Parry (1932, pp. 244-5): 'It is *ana* (taboo) for a maternal uncle to curse or insult his nephew. . . . The highest term of respect in use among the Lakhers is *papu* (my maternal uncle) not *ipa* (my father); a villager addressing the chief always calls him *papu*.' Kachin behaviour is similar; a chief is addressed as *tsa* (mother's brother). *Tsa* possess particular potency at cursing (*mãtsa*); for references regarding the political significance of Lakher marriage alliances see Chapter 5 below.

TROBRIANDS

Malinowski (1932a, p. 137): 'It is characteristic of their ideas of the bonds of marriage and fatherhood which they regard as artificial and untrustworthy under any strain that the principle suspicion of sorcery attaches always to the wife and children.'

Here the mystical influence of father over children is separated from the controlled supernatural attack of children against father. On the other hand *ibid.* p. 190 shows the father to be in control of female sorcerers who are liable (unless the father is properly placated) to attack his pregnant daughter, and in numerous contexts we are told how the chief exercises his authority with the aid of professional sorcerers who obey his command (e.g. Malinowski, 1932b, pp. 85-6). Here the mystical influence of the father may coincide with the supernatural attack of the father-chief.

It should be remarked that the chief's relationship to his village headmen is typically that of father (*tama*) or brother-in-law (*lubou*). The tribute which a chief receives from his political subordinates is, from another point of view, the *urigubu* (harvest payment) paid to a father or to a brother-in-law (Malinowski, 1935, I, pp. 392-7, 414; Powell, 1956, pp. 481).

ASHANTI

For ideas about supernatural attack see Rattray (1923, chapter 2; 1927, pp. 28-31). Ashanti often marry near kin and approve of reciprocal cross-cousin marriage. In traditional Ashanti this was carried to the extreme that the royal family and also certain professional guilds had an almost caste-like aspect (Rattray, 1923, p. 301; 1927, chapters xxix, xxx). However this type of small group endogamy did not result in a structure in which ties of marriage alliance could serve political ends.

TALLENSI

For ideas about supernatural influence see especially Fortes (1959a) and the references to *yin* in Fortes (1949). Since a Tallensi may not marry any near kinswoman, however she be related, it is self evident that marriage cannot here serve as a relationship of perpetual political alliance in the sense which I have been discussing.

This finding has a bearing upon the argument of Fortes (1959a), for, with certain qualifications, Fortes's Oedipus theology corresponds to my *x* ('uncontrolled mystical influence'), while his Job theology corresponds to my *y* ('controlled supernatural attack'). In the West African examples which Fortes has discussed, *x* and *y* are complementary notions which tend to cancel out—the inescapable consequences of personal Fate modify the arbitrary dictates of an all powerful God and *vice versa*, but my additional evidence shows that this seeming balance is fortuitous. There are some societies where Fate and Implacable Deity are to be found personified in one and the same affinal personality, and in such cases the relation between religious ideas and political authority takes on a very different and very special aspect—the *mana* of the King and the *mana* of the witch coalesce in the person of the all powerful Father-in-Law.

Without the algebra, my *x/y* proposition reads thus: '*uncontrolled mystical influence* denotes a relation of alliance; *controlled supernatural attack* denotes a relation of potential authority of attacker over attacked or *vice versa*. Where the presumed source of *controlled supernatural attack* is the same as the presumed source of *uncontrolled mystical influence* that source is in a position of political authority vis-à-vis Ego.' In this form we have an hypothesis which might, in principle, be subjected to test. In practice I suspect that the establishment of convincing negative instances may prove rather elusive. For example the material which Firth has recently provided about the relation between spirit mediums and their familiars in Tikopia and elsewhere looks at first sight as if it ought to provide an excellent test case, but I rather think that, so far as my hypothesis is concerned, this particular evidence could be interpreted in several different ways (Firth, 1959, pp. 141-6). But here at any rate is a matter which invites investigation.

This whole digression into the structural implications of metaphysical

belief has been introduced only by way of illustration. The insights which emerge relate to facts which cut right across the conventional categories of anthropological discussion and my objective has been to demonstrate by example how an excessive interest in the classification of ethnographic facts serves to obscure rather than to illuminate our perception of social reality. And here I revert to the point from which I started.

I am constantly amazed by the feats of mental gymnastic which anthropologists perform in their efforts of produce universal definitions and discriminations; notable examples are Gough's definition of marriage (Gough, 1959, p. 32) and Fortes's discrimination between filiation, affinity and descent (Fortes, 1959b). My harsh view is that the value of such butterfly collecting activity is quite ephemeral and that the categories which result from it should always be highly suspect. This applies equally to the vague topological entities of my own analysis and to the polished concepts of Professor Fortes. We need to understand that the establishment of classifying categories is never more than a temporary *ad hoc* expedient. Most such categories have ceased to serve any useful purpose long before they achieve the distinction of appearing in print.

So far as our immediate discussion is concerned I readily admit that, in any given social system, we shall always find some kind of notion of corporate kinship which stands opposed to some kind of notion of marriage alliance as p is to q , but what we can usefully compare as between different societies are not these particular ps and qs (regarded as separate institutions) but the ratio of p to q considered as a mathematical function. Or, in non-metrical language, we need to think of the relationships which link children to their parents and the parents to one another as constituting a 'neighbourhood system'—a topological space.

No doubt many of you will want to dismiss my whole argument as a futile exercise in bogus mathematics. I don't accept that. I believe that we social anthropologists are like the mediaeval Ptolemaic astronomers; we spend our time trying to fit the facts of the objective world into the framework of a set of concepts which have been developed *a priori* instead of from observation.

It is some years since Professor Firth drew attention to the alarming proliferation of structuralist terminology. He noted with dismay that maximal, major and minimal lineages had been supplemented by medial, inner and nuclear lineages; effective lineages were distinguished from morphological lineages; social relations had acquired focal fields, vertebral principles and constellations of ties and cleavages (Firth, 1951a).

That was in 1951, but the process has continued. We now have not merely filiation but complementary filiation, not merely siblings but residual siblings. Of such cycles and epicycles there is no end.

The trouble with Ptolemaic astronomy was not that it was wrong but that it was sterile—there could be no real development until Galileo was prepared to abandon the basic premiss that celestial bodies must of

necessity move in perfect circles with the earth at the centre of the universe.

We anthropologists likewise must re-examine basic premises and realize that English language patterns of thought are not a necessary model for the whole of human society.

Malinowski's basic premiss was that the elementary family is a universal institution. Fortes would qualify this but retains a dogmatic view of the functional utility of incest which is very similar to Malinowski's. This leads logically to an acceptance of English categories and to the assumption that our words consanguinity and affinity have some universal value. It is this which leads anthropologists to treat the words sibling, filiation, descent and affinity as absolute technical terms which can be distinguished from one another by *a priori* reasoning without reference to ethnographic evidence.

My contrary thesis is that ethnographic facts will be much easier to understand if we approach them free of *all* such *a priori* assumptions. Our concern is with what the significant social categories are; not with what they ought to be.

If you feel you must start with assumptions then let them be logical (that is mathematical) assumptions—such as that the social relation between brothers must of necessity be in some sense the opposite of the social relation between brothers-in-law. But do not drag in private psychological theories behind a smoke screen of technical terms.

All I have tried to do here is to show that an unprejudiced re-examination of established ethnographic facts which does *not* start off with a battery of concepts thought up in a professorial study may lead to some unexpected conclusions.

And that must be my conclusion—stick to the facts of the case, and exercise your imagination; but don't get so personally involved in the situation that you cannot distinguish between the empirical facts and your private analytic concepts.

In this first Malinowski Memorial lecture I have set out to demonstrate, from a single small example, that Malinowski still has no rival in the penetration of his ethnographic observation. Where Malinowski's work was limited was that it was too exclusively Trobriand; his theoretical concepts were designed to fit Trobriand data just as, latterly, Fortes's concepts have been designed to fit Tallensi and Ashanti data. But it is still possible to base speculative generalizations on Malinowski's facts, and I believe that speculative generalization, even if it often proves wrong, is very well worth while. Even from tonight's popshy we may have learnt a little.