

Rodney Needham

Remarks on the Analysis of Kinship and Marriage 1970

'What is theory in musical composition? - Hindsight. It doesn't exist. There are compositions from which it is deduced.'

IGOR STRAVINSKY

I

INTRODUCTION

To consider the analysis of kinship and marriage leads at once into a paradox.

'Kinship is to anthropology what logic is to philosophy or the nude is to art,' declares Robin Fox; 'it is the basic discipline of the subject' (1967: 10). Certainly it is a well-established part of the subject: Lafitau began the comparative study of unilineal descent and classificatory terminologies as long ago as 1724, and Morgan inaugurated in 1871 what has since become a recognized topic of academic investigation and theory. The syllabuses of university instruction in anthropology now invariably include kinship; no textbook is thought adequate without some treatment of it; and in all the variety of examinations in anthropology it occupies a central and unavoidable position. You cannot gain a certificate of competence as a social anthropologist without demonstrating a command of kinship theory, and it is expected of most anthropologists that they will make some contribution to it. Indeed, most of those who have made great names in anthropology - e.g. Rivers, Kroeber, Radcliffe-Brown, Lévi-Strauss - have gained their prominence largely by their publications on kinship. If there is one topic, therefore, which is indispensable to social anthropology, and which defines what social anthropologists essentially do, it would appear to be kinship. Here, if anywhere in the subject, we should expect to find discipline, methodical rigour, and theoretical advance.

This much is, I suppose, a standard account of the matter; but an inside look at what really goes on reveals a curious

situation. The majority of students of anthropology, and their teachers, are apprehensive and uncertain about kinship, and they have as little to do with it as they can get away with. Examination scripts seldom show much enthusiasm or sound knowledge, and the professionals often seem not to be particularly good at the practical analysis of kinship systems. There is a comparative paucity of published works on the topic, and progress in understanding kinship systems has been sporadic and slight. The current theoretical position is obscure and confused, and there is little clear indication of what future developments we can expect or should encourage.

In view of the constant professional attention extending over roughly a century, and a general improvement in ethnographic accounts, this is a remarkably unsatisfactory situation in what is supposed to be a basic discipline. Obviously, after so long a time, and so much field research, it is not just facts that we need. Something more fundamental seems to have gone wrong. What we have to look for, perhaps, is some radical flaw in analysis, some initial defect in the way we approach the phenomena. Edmund Leach has already explored this possibility, in his salutary address 'Rethinking Anthropology' (1961: 1-27) but there is little sign that even his verve, clarity, and ingenuity have yet had much effect on received ideas and ordinary practice. What I want to do here, then, is to resume Leach's iconoclasm and to look with him for a way out of our present uncertainties.

A possible diagnosis may be that the trouble lies not so much in the substantive study of institutions of kinship and marriage as in our conceptual premisses, and most decisively in the way we conceive the classification of phenomena. The failings basically responsible for the present situation are, I shall argue, firstly what Wittgenstein has called 'a craving for generality' (1958: 17) and secondly the lingering delusion of a natural science of society, a conception which has led to a kind of analysis that has produced few useful results. You will already recognize in these contentions an echo of Leach's strictures on 'butterfly-collecting' and on biased premisses. For that matter, what I too want to press for is precisely to 'take each case as it comes' (Leach 1961: 10). But I think there is a conceptual therapy by means of which we can prepare ourselves better to

do so, and for this purpose I suggest that we should turn back to Lowie's *Culture and Ethnology* (1917) and to Wittgenstein's *Blue Book* (written in 1933-34, published 1958). You will see therefore that I cannot pretend to be telling you anything very new. But since it has taken me a long time to see the relevance and the useful effect of views published decades ago, I suppose there will be others to whom it will be helpful if I rehearse them on this occasion.

My argument is presented in the form of remarks on a series of topics, not as a progressive exposition, and without any large apparatus of scholarly and ethnographical references. I adopt this means for two chief reasons. The first is that I have already published enough work on kinship and marriage to excuse me presenting any detailed demonstration of what I think ought to be done by way of analysis. This permits me to make my points in a cursory style which may stick more readily in the memory. The second reason is that a fuller citation of pronouncements on kinship uttered by many of my colleagues would have to be rather dissentient, whereas my intention is to be positive. For the most part, then, I shall cite only those with whose opinions I agree, and not even many of these. The essential is not to tax you with facts or with academic controversy, but to concentrate on the concepts that we are professionally inclined to employ when we analyse institutions of kinship and marriage. I shall deal, rapidly, with the notions of kinship, marriage, descent, terminology, and incest.

II

KINSHIP

There has been a fair amount of discussion about what 'kinship' really is. My own view is that much of this debate is pretty scholastic and inconsequential, and I shall not recapitulate any of it or embark on yet another definitional exercise. Let me simply adopt the minimal premiss that kinship has to do with the allocation of rights and their transmission from one generation to the next. These rights are not of any specific kind but are exceedingly various: they include most prominently rights of group membership, succession to office, inheritance of property,

locality of residence, type of occupation, and a great deal else. They are all, however, transmissible by modes which have nothing to do with the sex or genealogical status of transmitter or recipient. Certainly they have no intrinsic connexion with the facts, or the cultural idioms, of procreation. It is true that the possession and exercise of these rights is defined by reference to the sex of the persons thus related; but then so is the division of labour in the simpler societies, yet we do not for that reason think this method of distinguishing statuses so remarkable as to deserve a special designation and to call for a distinct type of theory.

These jural systems and their component statuses can be genealogically defined. Why this should be so is a fundamental question that has never been properly resolved, and I cannot take it up here. Let us merely admit the fact. It is certainly a very convenient fact, but the method of description does not entail any particular property in what is described. The circumstance that two societies can be described by the same means does not argue any significant similarity, either sociologically or semantically, between them. Still less does it mean that the relationships in question are genealogical or that they are so conceived by the actors.

What information is given, then, by the report that an institution has to do with 'kinship'? Nothing, really, about social facts. For the label designates no distinct type of phenomena; it provides no clue to comprehension; and it does not indicate the kind of analysis that will be appropriate. The use of the word 'kinship' is to be found, rather, in the multiple connotations of common usage, in the organization of ethnographical accounts, and in the conventions of academic discourse. When an ethnographer gives one chapter the heading 'Kinship', and another the heading 'Sacrifice', we have a rough preliminary idea of the different matters they will describe. It may well turn out, though, that there is a close connexion between them, just the same, and very likely neither will be comprehensible without the other. Similarly, if a colleague tells you that he is interested in kinship, his choice of phrase implies that he could have stated instead that he was keen on subsistence economics or primitive law, and the word he actually employs does indeed give you a vague idea of his theoretical

bent, the books he has presumably read, and the kind of technical conversation he is likely to engage you in. In this case as well; however, it cannot be inferred that his interest in kinship will be unconnected with economics or law; and in fact, of course, it will probably turn out that he has to deal with these topics also and that they in turn demand a recourse to kinship.

I am not denying, therefore, that the word 'kinship' is useful; and still less should I wish to try to reform our professional vocabulary by narrowing the definition of the word or, on the other hand, by urging that it be abandoned altogether. What I am saying is that it does not denote a discriminable class of phenomena or a distinct type of theory. We are tempted to think that it must have this specificity, because it is a substantive and because it is an instrument of communication. But it has an immense variety of uses, in that all sorts of institutions and practices and ideas can be referred to by it. Segmentary organization, section systems, widow inheritance, polyandry, teknonymy, divorce rates, and so on - all these topics and very many more can be subsumed under the general rubric of kinship. In other words, the term 'kinship' is what Wittgenstein calls an 'odd-job' word (1958: 43-4), and we only get into trouble when we assume that it must have some specific function. In a way, it could be said that the trouble is not very serious, since when we actually investigate an institution, or compare ways of explaining it, we do not generally speak of kinship at all. Indeed, this common circumstance demonstrates that the word has in fact no analytical value. On the other hand, anthropologists do often get into trouble, of a timewasting and discouraging sort, when they argue about what kinship really is or when they try to propound some general theory based on the presumption that kinship has a distinct and concrete identity.

To put it very bluntly, then, there is no such thing as kinship, and it follows that there can be no such thing as kinship theory.

III

MARRIAGE

Very similar considerations apply to the concept of marriage and to the theoretical propositions of anthropologists about

because kinship meant to all other areas of SA

marriage. I need not say much about this topic because the case has been well made by Leach: 'marriage is . . . "a bundle of rights"; hence all universal definitions of marriage are vain' (1961: 105).

I think there is no refutation of this argument. What I should like to re-emphasize, simply, is Leach's conclusion that 'the nature of the marriage institution is partially correlated with principles of descent and rules of residence' (108). Perhaps it is not so much correlated, though, as it is defined in any particular instance by what we divisively call the 'other institutions' of the society. It is not only jural institutions, either that we have to take into account, but moral and mystical ideas as well, and these in an unpredictable and uncontrollable variety. The comparison of marriage in different societies needs therefore to be contextual, and ultimately 'total' in a Maussian sense, if we are to be sure that we understand what we are trying to compare.

In this connexion, the designation of marriage has a special interest. Ethnographers do not on the whole report the indigenous terms for marriage, or investigate the connotations of such terms, yet we need not look far to see that these can be revealing. For instance, the modern German *Ehe* derives from MHG *ē*, *ēve*, law, statute, and its recent narrower meaning merely singles out marriage as one of the most important jural institutions. The English 'marriage' and French *mariage*, however, come from the Latin *maritus*, husband, which is usually referred to IE *mer- *mor-, represented by various words meaning 'young man, young woman'. It is at once evident that even two European traditions can embody, etymologically, two quite distinct kinds of ideas about marriage. More than this, there may not be any designation for marriage at all. In classical Greek, as Aristotle observed, 'the union of man and woman has no name' (*Politics*, I, 3, 2). Even though marriage was essential for the preservation of the 'houses' (*οἴκοι*), which were the constituent elements of the Athenian city-state, there was no single word which could be taken to stand for 'marriage' – nor, for that matter, were there words in classical Greek which stood for 'husband' and 'wife' (Harrison 1968: 1). And to take a contrasted enough civilization, whereas the Penan of Borneo do have words for husband (*banen*) and wife

(*idu*, *do*), they too have no word for marriage. One wonders, therefore, how many other societies make no lexical recognition of that institution which has so commonly been regarded in anthropology as categorically essential and universal.

As soon, however, as we adopt some technical definition of marriage, whether or not it is held to be universal, we run the risk of leaving out of account precisely that feature (e.g. chastity, allegiance, life-giving) which in one or other of the societies compared is in fact central to the institution. This is of course a familiar quandary in comparative studies, but I think it is a question whether its lessons have everywhere sunk fully home. At any rate, large-scale correlations are still attempted, and these can be carried out only by means of fairly strict definitions which are nevertheless presumed to be widely applicable, but the stricter they are the less likely it is that they will cope adequately with social reality.

Once again, though, I am not denying that 'marriage' is a very useful word. On the contrary, it has all the resources of meaning which its long history has conferred upon it, and we should now find it hard to communicate without these. For that matter, it is a more indispensable word than 'kinship' is, and it directs us more precisely to an identifiable kind of relationship. If an ethnographer sets out to tell us about marriage, we have at least a preliminary indication that he is not going to focus directly on dam-building. But I choose this latter example, all the same, precisely because Onvlee has shown that in eastern Sumba, where marriage is prescribed with the matrilineal cross-cousin, you cannot understand the organization of dam-building unless you first understand the norms of marriage (Onvlee 1949). Conversely, you cannot understand the marriage institution without knowing the forms of co-operation which follow from it. There are also cosmological grounds to both aspects of Sumbanese social life. As Hocart says in another context, 'There is much more to the cross-cousin system than the classification of relatives; there is a whole theology . . .' (1952: 237). But nothing in the ordinary connotations of the English word 'marriage' prepares us to grasp a situation such as this, and nothing in anthropological usage gives the word any technical value either.

So 'marriage' too, is an odd-job word: very handy in all

sorts of descriptive sentences, but worse than misleading in comparison and of no real use at all in analysis.

relationships which come about through marriage are culturally different, should be understood in cultural context. By analyzing it generally we capture on aspects which are not relevant elsewhere. Eg notion of love

IV
DESCENT no. of people involved

The classification of modes of descent is a specially effective example of conceptual difficulty, because the topic has been a constant anthropological concern since Bachofen and McLennan. In spite of this prolonged concern, however, there is still no general agreement on the matter.

Anthropologists habitually use terms such as 'patrilineal' or 'matrilineal', yet cannot easily claim that these are specific descriptions. Even when the ethnographic facts are not in dispute, it is sometimes possible to argue about the type of descent system to which a given society should be assigned. Or when it is agreed that a society is patrilineal, for example, it is possible to argue about whether it is a strong or a weak instance of the type. Such arguments might be all to the good if they led to cogent and agreed decisions, but for the most part this is just what they cannot do. What we are left with is not theoretical advance but a wordy conflict of rival definitions. I need not go on, or supply examples, for the situation is familiar to any anthropologist. What is important is to find a way out of this typological confusion.

Some anthropologists (e.g. Köbben, Lewis) have contended that we cannot assimilate different societies on the mere ground that they are 'patrilineal', but that this wide designation needs to be broken down into the component functions. It is the kinds of rights that are governed by the rule of descent which decide the respects in which societies are to be counted as patrilineal and thus as comparable. 'The functional implications of descent are often much more significant than whether descent is traced in the patri- or matri-line' (Lewis 1965: 109). This approach has a well-established ancestry - it was embarked on by Fison in 1879 and continued by Wake in 1889 - but it is not, I think, the answer. The difficulty remains that this substantive concentration on complexes of rights makes comparison as uncontrollable and as hard to carry out as the rights may be various.

Simply to specify more narrowly what 'patrilineal' means in a particular description does not make the rubric any more useful; in fact, it tends to show how inappropriate it really is. On the other hand, no one needs it in order to define a single jural system that is under study, for an exact survey can report the social facts without summing them up under any such general label. In any case, the proposed reconsideration is not fundamental enough. Lewis, for example, still takes it that 'descent' is traced in some line, only that this tracing is affected by 'other principles of organization' (1965: 106). Yet it is this very notion of 'descent' that needs investigation.

A more radical course is pointed by Leach's suggestion that such typifying devices as 'patrilineal' and 'matrilineal' may have no sociological significance whatever:

'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' (1961: 4).

This is a bracing notion, and in fact it is far from clear that there is any convincing defence of the class of matrilineal societies. My own inclination is to doubt whether there are any useful propositions about matrilineal systems which distinguish these, as a class, from societies with other rules of descent and thereby justify the typology. This may be thought a matter of debate, but at least there is enough reasonable doubt about the issue to call the conventional typology into renewed question.

Yet can we do without it? After all, Bachofen and McLennan created a stir because they had discovered something; and the jural differences between the Minangkabau and the Batak are real, not the result of an unfortunate typology. The troubles begin, however, when we try to characterize and compare the Minangkabau and the Siriono, merely on the ground that they are both matrilineal; in other words, when we extend the characterization beyond those features which prompt the description 'matrilineal' and then talk as though this label applied to all the other institutions also. Here I think we have a double instance of the craving for generality: we classify societies together because by some definition they possess in common the

feature of 'matrilineal descent', even though the rights governed and the functions they serve may be quite disparate and in each case we classify institutions together, as composing a 'matrilineal society', because various other usages, which may not be common properties of members of the class, happen to accord with that mode of transmitting the definitive rights.

Clearly the method is wrong, but all the same there is something to be wrong about. What is the solution? There is only one way out, I think, namely to resort to purely formal criteria, i.e. to cease concentrating immediately on substantive and juridical aggregates of many kinds, exercising different functions, and to think instead in terms of logical possibilities. I am not referring to the use of systematic models, but to a far more basic procedure. It may strike you as excessively simple, but I think it has advantages.

Given two sexes, and transmission of rights defined by these, we may distinguish six elementary modes of descent. Let us denote male by m , and female by f . The modes are then as follows:

all possible

1. $m \rightarrow m$
2. $f \rightarrow f$
3. $(m \rightarrow m) + (f \rightarrow f)$
4. $(m \rightarrow f) + (f \rightarrow m)$
5. $(m \rightarrow m) \parallel (f \rightarrow f)$
6. $m/f \rightarrow m/f$

These formal modes correspond severally to functional paradigms. Mode 1, male to male: patrilineal. Mode 2, female to female: matrilineal. Mode 3, a combination of modes 1 and 2 in the definition of any status: two conjoint rules of descent, i.e. bilineal. Mode 4, male to female, female to male: alternating. Mode 5, male to male, female to female, defining distinct sexual statuses: two disjunct rules of descent, i.e. parallel. Mode 6, male or female to male or female: cognatic.

Naturally, once alternation is admitted the number of additional possibilities (i.e. variations on mode 4) is increased, but these six modes are elementary.

The modes are not to be conceived as characterizing societies holistically. Indeed, modes 4 and 5 probably could not be employed socially as regular and exclusive principles of trans-

mission and incorporation, though certain rare and uncertain approximations to them have been reported. My point, rather, is that in any society different rights may be transmitted according to different modes. The ethnographer's task is then to sort out the rights, according to the indigenous classification in the first place, and to establish the various rules by which they are governed. To cite an extremely simple case, Penan society could be partly analysed in this way:

descent name	mode 1
residence	mode 2
inheritance	mode 5
group membership	mode 6

The result is thus a complex of rights and rules, not corresponding entirely to any single principle of descent. The jural complex thus arrived at is likely to be singular, not only in the kinds of rights culturally distinguished, but also in the association of these with different modes. Only in the extremely improbable event that a society were to transmit all rights uniformly by one single mode would it conform literally to any conventional type such as 'patrilineal' or 'matrilineal'.

It would of course be possible to construct a new and much extended typology of descent systems, composed of all the logically possible combinations of the elementary modes. This would give a total of 63 types. Whether this was worth doing would depend on the results, but since my present concern is to undermine such typologies it is not an undertaking that I should recommend. The intention is to preserve the specificity of the social facts, yet at the same time to make possible a comparison that is not based on merely contingent assemblages of institutions or 'functions'. This we can do by resorting to those simple logical possibilities which govern equally both the practical fabrication of social systems and our own abstract conceptions of the forms they can take. That is, our analysis will be guided by the same logical constraints as must have been effective in producing the systems that we study.

Of course, there is still ample room for the arbitrary or mistaken discrimination of kinds of rights or modes of transmission, as well as for speculative alternatives in the selection or classification of those rights or modes that an investigator is

interested in. We can never obviate these sources of imprecision and variations, but at least a purely formal approach gives us the chance of making a less biased start.

Let me admit at once, moreover, that this has been a cursory exposition, and that I have not distinguished exactly, as might ultimately be done, all of the jural variables involved. The main reason is that I am not trying to work out a technique, complete with instructions for use, but to bring about a change in the conception of descent systems. This change offers, it seems to me, a number of advantages.

1. To begin by listing logical possibilities, without regard to their social feasibility or known realization, actually accords better with social reality. The indigenous classification of rights can be directly adopted, and these can then be distributed without prejudice among the formal modes, not in accordance with some theoretical predisposition or academic fashion. The approach thus conduces to an accurate ethnographic description, a possible benefit which is surely of the very greatest importance.
2. It removes the temptation to characterize a descent system univocally as patrilineal or matrilineal, etc., and in doing so to make a biased choice of any one kind of right which is then taken to define 'the' rule of descent.
3. It renders less easy, consequently, a comparison of different systems by reference to the transmission of only one kind of right, e.g. membership in a descent group, and the overriding of jural and other differences which might qualify or invalidate the comparison.
4. A logical analysis presents a constant discouragement to the employment or elaboration of defective empirical typologies which have proved not to advance our understanding of the phenomena.
5. The formal approach allows anthropologists of any theoretical persuasion to collaborate, or to understand each other, without the customary obstacles of variant technical vocabularies or opposed connotations implicitly attached to conventional labels.

A consequence of conceiving descent systems in this way, and one that has a special analytical value, is that among a number of societies compared in any formal respect there will not be assumed to exist any empirical feature common to all. It will more readily be seen, in other words, that they may not compose a class in the conventional sense. Instead, they can exhibit what might be called serial likenesses. As a simple illustration, take three societies (A, B, C), each constituted by three features (p, \dots, v):

A	p, q, r
B	r, s, t
C	t, u, v

Let r and t be each a type of right transmitted in mode 1. There is then a resemblance, r , between A and B, and another, t , between B and C, but none between A and C. Yet in ordinary anthropological practice they could all be classed together as 'patrilineal'. A crucial misdirection can thus be given to our thought by the uncritical employment of the received idea as to what a class is; whereas to analyse and compare by reference to formal, rather than empirical, types can lead to a clearer view of the distinctive features by which similarities and dissimilarities are gauged.

In sum, I am suggesting that the present theoretical confusion about descent has its origin in two basic conceptual defects: (1) an addiction to empirical generalizations, and (2) a subscription to a conventional but unrealistic idea of how a class is formed. These are matters to which I shall revert below in the discussion of other topics. For the present, I must introduce a note of reserve. A direct consequence of the approach that I am advocating is that comparison becomes far more difficult, and on any large and detailed scale perhaps impracticable; for the distribution of rights among logical types increases the factors in question and does not make the rights thus discriminated any more comparable.

"matrilineal" is a useful adjective, but very misleading noun. Use it to describe, but not to classify.

TERMINOLOGIES

In the study of relationship terminologies we ought to be on firmer ground, one might think, than when we are dealing with

anthropological abstractions such as overall rules of descent for in this field of inquiry we are dealing with the explicit linguistic categories by which other peoples classify their social experience. But even here certain familiar conceptual defects have led to a theoretical stultification. Here too a premature urge to construct empirical typologies has led to a battery of nominal types that we can do very little with because the phenomena have been mistakenly grouped.

A good example is the so-called 'Omaha' type of terminology defined essentially by the equation $MB = MBS$. This famous type has a current and very prominent employment in ethnographical reports, in comparative studies, and in theoretical exercises. Yet I do not think it is going too far to say that nothing of any real elucidatory value has come out of all this attention. The reason is simply that a variety of terminologies all possess this supposedly definitive feature but differ from each other in practically everything else. Thus the Araucanians, Iatmul, Miwok, Nyoro, and Thado, to cite no more societies, have all been typed and classed together on the ground that they have Omaha terminologies, whereas the quickest survey shows that their forms of social classification (not to mention their institutions) are systematically disparate. Intensive analyses of individual 'Omaha terminologies' have repeatedly confirmed the invalidity of the type, and with increasing certainty as the structural range of terminologies studied is extended. The Ungarinyin terminology, for instance, exhibits the supposed Omaha equation yet differs extremely (in statuses distinguished, number of descent lines, and rule of marriage) from the Purum, which similarly exhibits the Omaha feature, and the Purum terminology in turn differs extremely from the Gurage, which also makes the equation in question but is structurally quite different again. When an integral comparison of these terminologies is carried out, it is at once obvious that there is no systematic resemblance among them.

This is an expectable result by now, and it gives rise more forcefully to the question of what the 'Omaha' label is supposed to tell us. The answer is that there exists no useful generalization about this factitious class of Omaha terminologies. It is true of course that the minimal equation is associated with patriliney (in a conventional acceptation), but no one could well

claim that this was a theoretical result arrived at by means of the type. It has been well known, at any rate, since the last century, when both Kohler and Durkheim made the point. And it is surely not surprising that the terminological identification of a man and his son is accompanied by a transmission of status through males. If, then, an ethnographer reports an Omaha terminology, he tells us nothing of any descriptive value; and in representing the terminology to himself under this label he tells himself nothing of any analytical value either. No systematic comprehension is thereby provided, nor does the assignment to the Omaha type offer any clue to the analysis of the society which employs the terminology. There really is no such thing as an Omaha terminology, except that of the Omahas themselves, and it leads only to confusion and wrong conclusions to suppose that there is.

A parallel case is that of the so-called 'Crow' terminologies, typically defined by the minimal equation $FZ = FZD$. There are many very different forms of terminology which possess this feature, and they do not compose a systematically definable class. A telling illustration is provided by Eggan's paper on historical changes in the Choctaw kinship system (1937). He begins by conceding that 'if we examine various Crow . . . kinship systems we find a series of variations . . . , so that there is some difficulty in deciding whether a given kinship system is a Crow type or something else' (35). The cases that he compares demonstrate this point. The Choctaw terminology varies from the Crow type in the curious features $FZ \neq FZD$ and $FZS = FZSS$. The Chickasaw classification differs in other particulars, the Creek from these, and the Cherokee yet again. Finally, the Yuchi terminology, which shows still further divergences from the Crow type, is thought to suggest a Crow system because of the inferred equation $(FM) = FZ$. But these terminologies, in spite of all their differences one from the other, can nevertheless be classed together as 'Crow' - and it is a question why anthropologists should wish to do so.

Eggan's own purpose is primarily historical: 'These kinship structures,' he writes, 'originally Crow in type, were progressively modified by varying degrees of the same acculturational process' (47). This argument is convincing, but it serves at the same time to impair the concept of a Crow terminology.

Even though these terminologies can apparently be traced back to forms of classification which more nearly resemble each other, in their reported condition they are really very different - yet they are still typed as 'Crow'. And even if they have undergone a similar kind of change, under similar pressures, the degrees of variation which they exhibit must reduce any theoretical effect in classing them all as Crow terminologies. For the point in discriminating a class of phenomena is to be able to formulate propositions which hold for all members of the class. But in these cases the terminological variations are argued to correspond to different degrees of social change. In other words, however similar these societies may previously have been, they are now disparate in certain institutional respects, and these differences are ignored when they are subsumed under the 'Crow' label. On the other hand, the label is neither essential nor helpful in the sociological analysis of each of the societies which are thus classed together.

I have cited Eggan's investigation precisely because the ethnic connexions and other similarities of circumstance make a comparative study especially feasible, but even in this case the conventional typology of terminologies has no comparative or analytical value. This conclusion becomes yet more clear when a comparison is extended to societies in other culture areas, e.g. to the Mota of the Banks Islands or the Ramkokamekra of Brazil; for the Crow designation then refers to nothing more than the common feature of matriline, which is exactly the basis on which the class-designation is applied, and there is no further sociological property which can be ascribed to members of the class.

Similar conclusions could readily be reached if we were to scrutinize other conventional types of relationship terminology, e.g. 'Dakota', 'Sudanese', 'Eskimo', 'Hawaiian'. In each instance it is possible to demonstrate that the class has been invalidly constituted; and in each instance, so far as I can judge, no propositions of scientific value have been arrived at by means of the typology.

What I wish to propose therefore is quite seriously that this kind of typology, i.e. one in which the types are defined by isolated features of named societies arbitrarily selected as paradigm cases, should be entirely abandoned. It is method-

ally faulty, it misdirects research, and it has served no useful purpose.

I cannot even say, as though in recognition of a theoretical advance now accomplished, that the time has come to make this conceptual change, for what I recommend is no more than what Lowie pressed for in 1917. Our only advance since then, as far as this issue is concerned, has been in factual knowledge, such that we now possess superior ethnographic resources for the demonstration of his points. Lowie argued, in his neglected but masterly work *Culture and Ethnology*, that any given system is a complex historical growth that cannot be adequately defined as a whole by some such 'catchword' as classificatory, Hawaiian, or what not (1917: 116). 'It cannot be too strongly urged,' he emphasized, 'that a given nomenclature is molded by disparate principles' (122). 'There is no Hawaiian system, no Dakota system' (123).

How, then, should we analyse relationship terminologies? By abandoning types and concentrating on principles. 'We shall . . . do well to amend our phraseology and to speak rather of kinship categories, features, or principles of classification than of types of kinship systems' (105).

For my part, I have tried to make this point in a number of places, so perhaps I may simply refer you, for a recent demonstration, to my analysis of the Gurage terminology of social classification (1969). This terminology includes the features MB = MBS and FZC = ZC, so that by these criteria it clearly belongs to the 'Omaha' type. But the really interesting thing about the Gurage terminology is that it is a quite singular form of classification, particularly in the lineal equations in the line of reference, and systematically unlike any other that I have been able to discover. I have tried to show in that paper that the distribution of the terms can well be elucidated by a sociological analysis of Gurage institutions; but at no point in the investigation is the Omaha label, or any other aspect of the typology to which this designation belongs, of the slightest use. Instead, the analysis proceeds by an attempt to understand Gurage social classification through the categories and principles proper to the terminology itself. The most prominent principle of this classification happens to be exhibited in the extensive lineal equations that are effected. But this feature

does not constitute a type, or assimilate the Gurage terminology to the Omaha or to any other type. It is simply one cultural exemplification of the fact that there are only two formal possibilities in the terminological identification of statuses in a descent system, viz. lateral and lineal. A terminology may recognize either dimension, exploit either possibility, in a variety of ways. All that the Gurage have done, by their lineal equations, is to make an unusual exploitation of an elementary principle of classification. What is interesting, then, is the specific configuration in which a terminology takes advantage of this possible dimension of categorical order. The reasons for which it distributes certain categories lineally can only be discovered, I have argued, by an intensive examination of the facts of the particular case. A recourse to principles and to formal possibilities enables this to be done; but to place a terminology under a substantive rubric such as 'Omaha', in the conventional typology, serves no such purpose. We must indeed take each case as it comes; only I would add that the way we take it can be guided by formal considerations which help us to recognize more clearly the intrinsic characteristics of the case.

This example introduces the stock question about the connexion between terminology and social forms. It will support the general burden of this paper to make just a few remarks on this topic.

There are presumably two main purposes in trying to establish correlations between categories and action. One is to permit such inferences that the analysis of a particular society shall be facilitated: given a certain distribution of categories, the investigator will then know what to expect in his study of institutions. The other is to permit sociological comparisons: given similar terminologies, it can then be assumed, for the sake of whatever theoretical issue is under investigation, that the institutions will be similar also.

The sad fact of the matter, though, is that neither of these conditions obtains. The outcome has been generally discouraging in these regards; but let us review the situation and see what lessons nevertheless emerge.

1. Relationship terminologies can be divided formally into (a) lineal and (b) non-lineal. By the former term (lineal) I refer to

classifications in which the typical feature, in the medial three genealogical levels at least, is the distinction of statuses according to whether relationship is traced through persons of the same sex or not. The consequence of this principle is to assort lineal statuses into descent lines, such that, e.g. F = FB, FBS = MZS, S = BS, as contrasted with F ≠ MB, FBS ≠ MBS, S ≠ ZS. Examples are lineal descent systems such as the Kaguru, Mapuche, or Nyoro. Non-lineal terminologies have no such positive feature, but are characterized by the lack of this principle of distinction. Examples are cognatic societies such as the Penan, Sarakatsani, or Siwang.

This is such a simple and fundamental division of forms of classification that here, if anywhere, we might expect to find a good correlation with institutions. There are indeed certain common associations, but it is never safe to make sociological inferences from the presence or absence of the distinctive feature. A lineal terminology does not entail a lineal rule of descent, and still less does it entail the presence of lineal descent groups. Examples of societies with lineal terminologies but without fixed rules of unilineal descent or corresponding descent groups are the Sinhalese of Pul Eliya and the Trio of Surinam. On the other hand, a non-lineal terminology can be accompanied by a lineal transmission of certain rights, e.g. succession to office or inheritance of real property. Examples are Bali and rural Japan. These empirical conclusions accord with what I have suggested above (sec. IV) about descent.

2. Where we find lineal terminological equations, either direct (e.g. MB = MBS, as among the Gurage) or alternating (e.g. MB = MBSS, MBS = MBSSS, as among the Iatmul), we can be fairly confident that we shall find in the sphere of institutions some explicit expression of a mode of descent (in these examples, patrilineal). But this can never be an absolute inference, as we have seen in sec. IV above, for there is no telling to what extent rights of other kinds may prove to be transmitted by different modes, and there is no *a priori* scale of evaluation by which it might be presumed that the mode exhibited in the terminology was the most important.

For that matter, it is conceivable, and may in one alleged

case be so in fact, that a terminology composed of matrilineal should govern the affairs of a society that was preponderantly patrilineal. In any case, even if a rule of descent could be inferred from a terminology, nothing else could be. That is, one could not infer what groups were formed, how rights were ascribed, or what values were recognized.

3. Prescriptive terminologies can readily be identified, by the invariant relation that articulates relations between lines, and can thus be distinguished from non-prescriptive.

They fall into two main types: (a) symmetric, and (b) asymmetric. But this contrast, even when most clearly established, does not permit inferences about the ways in which alliances are actually contracted. A symmetric terminology does not entail symmetric (reciprocal) alliances but, on the contrary, may govern a strictly asymmetric transference of women. An asymmetric terminology, on the other hand, is indeed incompatible with symmetric alliance, but nothing more positive than this can be inferred. In systems with either symmetric or asymmetric terminologies, the exercise of preferences can lend a marked bias to the conduct of social relations, but a preference cannot necessarily be read off from a prescriptive terminology.

4. The employment of any type of terminology as a practical instrument of social classification can be considerably affected by innumerable factors which are not recognized in the terminology itself.

The most general factor of the kind is relative age. Individuals of the same age will belong naturally to different categories, and individuals of different ages will belong to the same category. Social relations may therefore be ordered, in principle, either by category or by relative age. If age is sometimes the dominant criterion in classification, then in such instances it is to some extent incorrect to regard the terms as denoting distinct classes of persons (Needham 1966a).

Other common factors are residence, which Kroeber indeed argued to be of fundamental importance (1938), genealogical degree, and collective sentiments. There are in addition innumerable further possible factors which affect the employment of terminologies but cannot be inferred from them. They can

only be discerned, and their consequences gauged, by the empirical investigation of each particular case.

A limitation of a fundamental kind is placed on the interpretation of relationship terminologies by the fact that we cannot even infer anything about the degree of significance that a category may have.

This was so well recognized by Murdock, in *Social Structure*, that he went so far as to isolate what he called a criterion of 'immateriality'. By this he meant 'a negative similarity resulting from the functional unimportance of the relatives of two kin-types, whereby a sufficient basis for differentiating them is lacking' (1949: 136). Leach, similarly, in analysing the Jinghpaw system, stressed that 'On the one hand, individuals are classed together because, individually and as a group, they stand in a significant and important relationship to the speaker; but on the other hand they may be classed together precisely because they are unimportant and remote' (1961: 52).

The latter case is well exemplified in systems, as among the Kachin, of asymmetric prescriptive alliance. In these, the categories of lineal relatives are of the greatest jural importance, and it is in accordance with this importance that more terminological discriminations are made among these than among affines. The terms for affines, wife-givers and wife-takers, complete a universal classification of social relations. There are thus three major classes of persons and groups: wife-givers, lineal relatives, and wife-takers. Actual contact will be maintained with some people of all three classes, but how are those to be classified with whom there is no recognized relationship? The solution adopted by the Haka Chin of Burma and the Mamboru of Sumba is to classify them as agnates. So the terms for lineal relatives can apply both to the most important people in the social universe and to the least important. But there is no means of guessing this from a study of the terminology. In other words, we cannot even tell whether, or in what circumstances, a term will mean anything or not.

All of this looks pretty negative, I admit, but I should not wish it to be thought for a moment that I underrate the significance of relationship terminologies. What I am arguing against is, in the first place, the conventional typology which - by the

very fact of assigning terminologies to substantive types - leads us to imagine that the form of a society's relationship terminology tells us more than it ever can. Secondly, I am arguing against the related assumption that societies with similar terminologies, in the conventional typology, are thereby sociologically similar also. But these objections do not at all mean that I am opposed to the analysis of terminologies or even to the construction of typologies. There are good and bad methods of analysis, after all, and good and bad typologies. Relationship terminologies are of prime and indispensable significance in the analysis and comparison of institutions and social action (Needham 1966b: 1967), especially once we realize what limitations we are under in our interpretation of them. The question is whether there is some method, or conceptual revision, by means of which we can escape the prejudices and confusions into which received anthropological ideas tend to lead us.

The means to an improvement in analysis has, I think, already been provided by Lowie, basing himself on Kroeber's brilliant paper on classificatory systems of relationship (Kroeber 1909). Instead of talking about a Hawaiian system, for example, and then being forced to decide whether a given case was or was not Hawaiian, he isolated a 'Hawaiian principle', i.e. a mode of classification which underlies non-linear terminologies. Instead of adopting the Dakota system as a paradigm, he abstracted 'the Dakota principle', i.e. that which orders linear terminologies. Then there were two 'variants' on the Dakota scheme: the Crow and Omaha principles of classification, i.e. matri- and patri-lineal identifications. Finally, he concentrated on the terminological correlates of special forms of marriage, including 'prescribed' marriage with the bilateral cross-cousin, a form which he saw as 'mirrored in the nomenclature' of certain Melanesian societies (Lowie 1917: 107, 119, 150, 151, 152, 172). Thus a terminology did not need to be labelled as a whole, and thence be assigned to one type or another, because of certain typological features that it happened to possess, but was regarded as 'molded by disparate principles'. This not only came closer to the facts of the matter, but it also provided a clearer view of the historical change of terminologies; e.g. instead of a clumsy succession of types, the Hawaiian principle could be

seen as gradually taking over from the Dakota principle, producing a variety of terminologies of which none might wholly correspond to either the Dakota or the Hawaiian type.

Perhaps this will not sound like very much, but it was a great step to take and I do not think that essentially we can do much better. The central advantage of Lowie's method is that in any analysis it dispenses with a crude substantive class and concentrates instead on the operation of classification. Instead of relying upon a paradigm case, and a class of other cases bearing more or less attenuated resemblances to it, the method investigates the intricate combination of classificatory principles by which any particular terminology may in fact be constituted. The craving for generality is checked, and the quest for essential features is given up.

In the study of relationship terminologies as well, therefore, we find that analytical advance is blocked by the familiar conceptual habits which have so much hindered the study of descent systems. The remedy is not only urged by Wittgenstein but has actually been put into prior effect by Lowie. In this case too, however, there is the consequence that while analysis is made more exact, comparison is made more intricate and difficult. In fact, it is hard to see how a large-scale comparison can be carried out when each case is analysed rather than typed.

Nevertheless, it may be that this line of argument will be found far too simple, perhaps even obvious. Let me just quote, therefore, what an authority (Murdock 1955: 361) has not long ago concluded about the study of descent systems and relationship terminologies:

'In anthropology, the initial classificatory task has now been substantially accomplished in the field of social structure. . . . We now possess satisfactory criteria for differentiating types of family organization, kin and local groups, and kinship terminology and behavior patterns. . . . These features are combined with one another in particular ways to produce a finite number of types of social organization, which in their totality represent a systematic classification comparable to those of Linnaeus and Mendelyev.'

Well, I do not wish to disparage Murdock's decades of industrious application to these matters, but I am bound to say that

I think these statements are mistaken in every particular. The notion of a finite and total classification is logically indefensible and this methodological ambition has achieved no result which might give it a pragmatic justification.

Yet the ideal continues to guide research. A recent illustration is furnished by Romney's analysis of the Kalmuk Mongol terminology (1967). It is a long and painstaking piece of work and I should not desire to call its analytical precision into question, but the formulation of its theoretical conclusions displays very clearly the typological concern which, especially since Radcliffe-Brown, has had such a deleterious effect on anthropological thought. It is simply that the Kalmuk terminology cannot be accommodated by Lounsbury's rules and hence that the differences 'warrant the recognition of an additional type of Omaha system' (Romney 1965: 141). Now this might be a positive result if only it meant that the Kalmuk Mongol system could be assimilated to a class of social phenomena about which there was a body of tested theoretical propositions. But merely to add a further type to the catalogue of 'Omaha' systems, when nothing enlightening or interesting has been said about this ill-founded class of systems, does not seem a very useful exercise.

VI

INCEST

Lastly, I want to make some remarks on the concept of 'incest'. This is a notion that is as prominent in social anthropology, and as persistent, as 'kinship' has been. The contributions to what is called incest theory have been very numerous, elaborate, and ambitious, culminating (as far as theoretical extremity is concerned) with Lévi-Strauss's dictum: 'The incest prohibition is . . . culture itself' (1969: 12).

The variety of explanations proposed has been remarkable: fear of menstrual blood, harmful genetic effects, instinctive repugnance (Radcliffe-Brown even declared that 'It is this emotional reaction that we have to explain if we are to have a theory of incest'), disruption of the solidarity of the family, disorganization of the status distinctions preserved by the prohibition, and so on. But two general assumptions are shared

by the incest theorists, however much at odds they may otherwise be. (1) That we know what 'incest' really is, whatever form its regulation may take. Thus Radcliffe-Brown easily asserts, as though it were to be taken for granted, that 'incest is properly speaking the sin or crime of sexual intimacy between immediate relatives within the family' (1950: 69; italics supplied). (2) That the prohibition of incest, in spite of its highly varied patterning, is a universal. Thus according to Lévi-Strauss it is 'a rule; but a rule which, alone among all social rules, possesses at the same time a universal character' (1969: 8-9).

It is curious, therefore, that there is nevertheless nothing like any general agreement about the explanation of incest prohibitions. Yet the endless academic debate is carried on as if the variegated and often unrelated theories were (1) rival explanations of the same phenomenon, and hence (2) universally applicable. 'Disputes are multiplied, as if every thing was uncertain; and these disputes are managed with the greatest warmth, as if every thing was certain' (Hume, *Treatise*, Bk. I).

These circumstances give rise to the suspicion that here too something has gone radically wrong. The trouble may lie, namely, not in the comprehension of the facts or in the ingenuity of the explanations, but in the conception of that which is to be explained. It looks as though the classificatory concept of 'incest' may itself actually have conduced to the confusion, and to the negative results, which characterize the present theoretical situation.

Revel, in his irreverent critical comments on *Les Structures élémentaires de la parenté*, makes this excellent and revealing observation: 'If there is one thing that emerges from Lévi-Strauss's book, it is that it will not do to talk about the prohibition of incest in general, because the institutions falling under this concept are so diverse and sometimes contradictory' (1957: 154). It sounds a simple enough point, but I think it contains the key. Incest prohibitions do not in fact compose a definite class, and if this is so there cannot be a general theory that applies to all of them.

There are two prime considerations which support this view. The first is the wide and variable range of statuses to which the prohibitions apply. The scope of application is in each case an integral feature of the social system, and in some sense a

function of it; i.e. the complex of prohibitions in a society cannot be comprehended except by a systematic purview of the institutions with which they are implicated. By this account of the matter there are as many different kinds of incest prohibitions as there are discriminable social systems.

The second consideration is that incest prohibitions are in part moral injunctions; they are expressions of indigenous ethical doctrines and, whether or not they are touched with peculiar emotional quality (something I doubt as a general concomitant), they have cultural meanings which no attempt at explanation can reasonably neglect.

Something of these latter connotations can be seen in the disparity of the words which ethnographers translate by the English word 'incest'. Let us begin with this word itself, taking it not as a quasi-technical term in social anthropology but philologically, as a particular testimony to a cultural tradition that remains implicit in its composition. It comes from the Latin *castum*, chaste, and belongs to a family of Romance words with the same root; e.g. French *inceste*, Italian *incesto*, Spanish *incesto*, etc. The idea ultimately underlying the English word seems therefore to be that incest is an offence against purity and moral decency. The Germanic family of languages, however, makes a significant contrast. In this we find related words such as German *Blutschande*, Dutch *bloedschande*, Norwegian *blødskanie*, etc. These terms are all compounded of the word for 'blood' and the word for 'shame'. The implicit idea, then, is not apparently about the guilt of sullied purity but about the public disgrace of an offence against kinship, conceived (as we know from other evidence) as a community of blood. Next, to go far outside the European tradition, the Chinese term *luan lun* implies a more sociological view. It is composed of characters meaning respectively 'disorder' and 'social relationship' (also: kind, class, order, sequence). In classical Chinese political theory the stability of the state depended on the 'five relationships' (*wu lun*), viz. sovereign/subject, father/son, elder brother/younger brother, husband/wife, friend/friend. The offence in this case, then, is that of disrupting a jural and moral order, namely that of confusing statuses. Finally, as a further culturally distinct example, let us take the Indonesian word *sumbang*, a word that is found in

one or another form (e.g. *suvang*) in probably the majority of the islands of the archipelago. *Sumbang* is commonly rendered as 'incestuous', but it also means 'improper' or 'repugnant'. In social conduct it refers generally to what is offensive because it is out of place or unseemly, a subversion of propriety. Forbidden sexual congress is only one form of such unsuitable behaviour. For that matter, *sumbang* covers not only incest but simple adultery as well. And cheating at cards or some other game is also *sumbang*. Very prominently, moreover, the word has aesthetic meanings. A connexion between the sexually wrong and the aesthetically shocking is made revealingly by a phrase in horticulture: a tree which has been grafted so that it bears blossoms of two different kinds is a *pokok sumbang*. Perceptually, the word applies to what is deformed, disharmonious, or discordant. A person's voice can be *sumbang*, hard on the ear; and an everyday material object, such as a battered chess-board, can also be *sumbang*, unsightly. Incest, therefore, is indeed denoted by this word, but it would be a mistake for an ethnographer to translate *sumbang* simply by the English word 'incest'.

In these four linguistic areas, etymological indications or current usages thus exhibit a variety of meanings which cannot validly be classed together as the semantic component of the prohibition of incest.

In addition to all this, there are cultures in which there is no such explicit notion as that of 'incest'. In fifth-century Athens, not only was the verbal concept entirely lacking but there was no action at law which was or could be brought against those guilty of an offence of the kind (Harrison 1968: 22).

By the above criteria, therefore, it is evident that incest prohibitions do not compose a class of homogeneous social phenomena. The most that might be claimed is that they characteristically include certain minimal prohibitions, e.g. that on sexual intimacy between mother and son. This is in fact disputable as a universal proposition, but even to concede it would still not justify the conception that these supposedly common features are what essentially define the class which is the subject of incest theory. It is this presumption, however, which suggests the received idea that the differential patterns of prohibition are 'extensions' of certain basic interdictions.

Thus Murdock writes that 'incest taboos and exogamous restrictions of whatever sort seem clearly to be extensions of the sex taboos between parent and child and between brother and sister in the nuclear family' (1949: 284). This of course is neither logical inference nor established fact, but the very presumption indicates the diagnosis of the initial conceptual error about incest. In this case also the difficulties arise from the invalid constitution of a class and, especially clearly, the unbounded exercise of the craving for generality.

We obviously need to find a radically different solution to the problem of incest. A structural theory is unlikely to serve since there are no systematic resemblances among all known sets of prohibitions. A formal approach via logical possibilities as in the reinterpretation of the concept of descent, does not seem feasible either, for there are no self-evident or readily acceptable premisses. Nor, finally, should we commit ourselves to a reductionist type of theory, for this is precisely what most anthropological explanations of the prohibition of incest have been, and they have not worked. We have, it seems, to look for a quite different way of understanding the facts.

Before we do so, let us make a brief appreciation of the situation. Each set of cultural prohibitions forms a coherent but variable assemblage of rules; the contextual explication of these rules demands a recourse to history, language, moral concepts, and many other contingent particulars. The various sets of prohibitions thus do not compose a concrete class such as might be open to a unitary explanation; the functional or semantic explication of the rules of one society may not apply at all to those of another.

Here, then, is a possible line of argument about incest theory:

1. In each instance, we are dealing with explicit rules, i.e. with collective representations. These rules may perhaps have emotional concomitants, and for all we know may even be grounded ultimately in some common instinct or psychic character, but empirically we can deal with them only as social facts.
2. The rules, by definition, have to do with access to women.
3. Women are social valuables; for many peoples, and in the opinion of some anthropologists, they are the prime valuables.

1. Access to socially recognized valuables is always socially regulated; the regulation expresses the evaluation.

2. Rules define what is permitted and what is prohibited.

3. The regulation of access to women is in these respects just like any other kind of regulation; access to certain categories of women is permitted, and to certain others is prohibited. These rules, positive and negative, compose in any particular instance a coherent set and should be comprehended as such, i.e. correlatively. It is methodically defective to consider only prohibitions, apart from permissions and prescriptions, and it is yet more invalid to consider only isolated prohibitions as though these alone were definitive or essential.

4. What is at issue in the study of 'incest' prohibitions is simply the negative aspect of the regulation of access to women. All that is common to incest prohibitions is the feature of prohibition itself.

I conclude that 'incest' is a mistaken sociological concept and not a universal. There can hence be no general theory of incest.

*Essence of the term "incest" are prohibitions
→ and these vary with different cultures
x what is universal there are prohibitions, but
extend a character of prohibitions is varies with
diff. societies* **CONCLUSIONS**

The intention behind these remarks has been to argue that anthropological research has been misguided, in even the supposedly basic topics of kinship and marriage, by certain conceptual failings. The most consequential of these is the uncritical attachment to an inappropriate conception of a 'class', namely that a class is a number of objects possessing certain attributes, or even perhaps only one attribute, in common. What is common, moreover, has been conceived as a specific or essential feature, and this feature has been represented in empirical terms. This mode of thought is the product, I have been contending, of a craving for generality. The craving is the resultant,

Wittgenstein maintains, of a number of tendencies, the first of which is:

"The tendency to look for something in common to all the entities which we commonly subsume under a general term - We are inclined to think that there must be something in common to all games, say, and that this common property is the justification for applying the general term "game" to the various games; whereas games form a *family* the members of which have family likenesses. Some of them have the same nose, others the same eyebrows and others again the same way of walking; and these likenesses overlap' (1958:17).

The phenomena classed together under one term may be related to one another in many different ways; as we scrutinize instance after instance we find that what we had taken for characteristic features prove not to be such and that similarities crop up and disappear. 'We see a complicated network of similarities overlapping and criss-crossing: sometimes overall similarities, sometimes similarities of detail' (Wittgenstein 1953, sec. 66).

There need not be, accordingly, any one thing that the phenomena of 'kinship' have in common, or that the phenomena of 'marriage', 'descent', 'relationship terminology', or 'incest' respectively have in common. Under each general term the instances may be classed together, instead, as in spinning a thread we twist fibre on fibre: 'And the strength of the thread does not reside in the fact that some one fibre runs through its whole length, but in the overlapping of many fibres' (Wittgenstein 1953, sec. 67). The trouble, however, is that we are inclined by our conceptual habits to assume that there must be something in common to all the phenomena that we class together as 'kinship' and the rest, and it is this unanalysed premiss that leads us into theoretical difficulty. Wittgenstein's response to this kind of confusion in other fields of inquiry is the injunction: 'Don't say there *must* be something common, or they would not be called [X] - but *look and see* whether there is anything common to all' (1953, sec. 66). The consequence of doing so in social anthropology is, I suggest, the realization that the phenomena classed as 'kinship', for instance, do not in all cases possess any specific features such as could justify the formulation of general propositions about them.

What is common to descent systems may be only that they variously exploit certain logical possibilities in a sexually defined transmission of rights from one generation to the next. What is common to the myriad forms of marriage may be only the contractual union of sexual statuses. What is common to relationship terminologies is the exploitation of certain formal possibilities in the classification of sexual statuses. What is common to incest prohibitions is merely the fact of prohibition. There may thus be something in common, under each general term, but not necessarily a definite set of characteristic, specific, or essential features. The common feature of prohibition, for example, does not entail that the incest regulations of different societies shall in any specific respect be at all comparable.

Another tendency which Wittgenstein argues is responsible for the craving for generality is our preoccupation with the method of science. 'I mean the method of reducing the explanation of natural phenomena to the smallest number of primitive natural laws . . .' (1958: 18). In social anthropology this preoccupation has most notoriously taken the form of a search for sociological laws, and although this particular ambition is now relatively out of favour, or less grandiosely declared, the outlook still flourishes in the attempts to find statistical correlations. Now these cannot be established without a precisely specified typology of phenomena, and it is necessary for the purpose that the phenomena classed under one type shall have in common certain specific features. But if it is conceded that the social facts in question do not necessarily compose a conventional class of this homogeneous kind, but may exhibit instead an immense array of serial and more complex resemblances, then the grounds for this method of comparison and explanation are removed.

This does not mean, though, that comparative studies are unfeasible or should be put at a discount. On the contrary, if we give up the reificatory typologies that are usual in social anthropology we shall actually be in a better position to compare, for we shall at least see the social facts in a less distorted way. More positively still, we can carry out comparisons by reference to logical features, and by the formulation of more suitable abstractions than have been customary. I have given some indications, here and elsewhere, of the formal approach.

That of improved abstraction may readily be illustrated by the study of prescriptive alliance. This type of organization defined by the terminology, and the terminology is constituted by the regularity of a constant relation that articulates lines and categories. It is by reference to this relational abstraction, not to lineages, groups, offices, and other such institutions that effective comparisons can be initiated.

For that matter, I think that there are not only logical features to which we can resort, but that there are psychical features as well which can be recognized through the screen of cultural differentiation and which make comparison possible. But that is another matter; at any rate, it does not seem directly relevant to the analysis of kinship and marriage. Yet it points in the same theoretical direction. If I may offer a more speculative observation, it may be that all social anthropology will be able to do – or perhaps what it may best be able to do – is to comprehend, in one case after another, the schemes in which cultures have variously taken advantage of logical and psychical facilities which are the elementary resources available to all mankind in the ordering of experience.

In this case, to adopt a phrase of Wittgenstein's (1953, sec. 90), 'our investigation ... is directed not towards phenomena, but, as one might say, towards the "possibilities" of phenomena'.

He argues against our idea that to every word there is a very specific essence about this word. He argues for their usage

ACKNOWLEDGEMENTS

I am indebted to Dr Francis Korn for calculating the number of combinations of the six elementary modes of descent. Dr Ian Hamnett has kindly worked out other possibilities, on the basis of permutations instead of combinations, which show that according to the assumptions and methods adopted the total could be 325, 1,956, 3,125, or 46,656 types.

but also at the same time be aware of broadness of the term. He argues against empirical typology. He argues about these redolent terms.

REFERENCES

EGGAN, FRED. 1937. Historical Changes in the Choctaw Kinship System. *American Anthropologist* 39: 34-52.
 FOX, ROBIN. 1967. *Kinship and Marriage: An Anthropological Perspective*. Harmondsworth, Middlesex: Penguin Books.
 HAMMEL, E. A. (ed.). 1965. Formal Semantic Analysis. *American Anthropologist* (Special Publication) 67. Menasha: American Anthropological Association.

Argues about use of concepts → not one-to-one correspondence between a word & essence
 Useful as long as we recognise limitations.

HARRISON, A. R. W. 1968. *The Law of Athens*. Oxford: Clarendon Press.
 HOGART, A. M. 1952. *The Life-giving Myth*. Edited by Lord Raglan. (2nd impression, ed. with a foreword by Rodney Needham, 1970.) London: Methuen.
 HUME, DAVID. 1739-40. *A Treatise of Human Understanding*. London.
 KROEBER, A. L. 1909. Classificatory Systems of Relationship. *Journal of the Royal Anthropological Institute* 39: 77-84.
 — 1938. Basic and Secondary Patterns of Social Structure. *Journal of the Royal Anthropological Institute* 68: 299-309.
 LEACH, E. R. 1961. *Rethinking Anthropology*. (London School of Economics Monographs on Social Anthropology, No. 22.) London: Athlone Press.
 LEVI-STRAUSS. 1969. *The Elementary Structures of Kinship*. Rev. ed. Edited by Rodney Needham, translated by J. H. Bell, J. R. von Sturmer, and R. Needham. Boston: Beacon Press; London: Eyre and Spottiswoode.
 LEWIS, I. M. 1965. Problems in the Comparative Study of Unilineal Descent. In *The Relevance of Models for Social Anthropology* (ASA Monograph 1): 87-112. London: Tavistock Publications.
 LOWIE, R. H. 1917. *Culture and Ethnology*. New York.
 MURDOCK, GEORGE PETER. 1949. *Social Structure*. New York: Macmillan.
 — 1955. Changing Emphases in Social Structure. *Southwestern Journal of Anthropology* 11: 361-70.
 NEEDHAM, RODNEY. 1966a. Age, Category, and Descent. *Bijdragen tot de Taal-, Land- en Volkenkunde* 122: 1-33.
 — 1966b. Terminology and Alliance, I: Garo, Manggarai. *Sociologus* 17: 141-57.
 — 1967. Terminology and Alliance, II: Mapuche, Conclusions. *Sociologus* 18: 39-53.
 — 1969. Gurage Social Classification: Formal Notes on an Unusual System. *Africa* 39: 153-66.
 ONVLEE, L. 1949. Naar Inleiding van de Stuwdam in Mangili. (Opmerkingen over de Sociale Structuur van Oost-Soemba.) *Bijdragen tot de Taal-, Land- en Volkenkunde* 105: 445-59.
 RADCLIFFE-BROWN, A. R. 1950. Introduction (pp. 1-85) to *African Systems of Kinship and Marriage*, ed. by A. R. Radcliffe-Brown and Daryll Forde. London: Oxford University Press for the International African Institute.

Rodney Needham

- REVEL, JEAN-FRANÇOIS. 1957. *Pourquoi des philosophes*. Paris: Pauvert.
- ROMNEY, A. KIMBALL. 1967. Kalmuk Mongol and the Classification of Lineal Kinship Terminologies. In Hammel (ed.) 1967: 127-41.
- WITTGENSTEIN, LUDWIG. 1953. *Philosophical Investigations*. Translated by G. E. M. Anscombe. Oxford: Blackwell.
- 1958. *The Blue and Brown Books*. Oxford: Blackwell.

© Rodney Needham 1971