THE FUTURE OF KINSHIP STUDIES

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CLAUDE LEVI-STRAUSS

Collège de France

IN 1941, A. R. Radcliffe-Brown began his presidential address to the Royal Anthropological Institute by remarking that: 'for seventy-five years the subject of kinship has occupied a special and important position in social anthropology' (1941). In the course of his lecture, he made clear that his reckoning was based upon the date of publication of John F. M'Lennan's essay on *Primitive marriage*. Since this event took place in 1865, it can be said that the year now ebbing toward its close marks the one hundredth anniversary of kinship studies, which fact would suffice to justify my choice. However, before looking into the future of a centenarian topic, I should like to gain perspective by going farther back in time to what I believe is the first account of a kinship system to be found in western literature. This account, though fanciful, and perhaps even for this very reason, can teach us many a lesson.

As an episode in Rabelais' *Pantagruel*, it composes chapter IX of the Fourth Book which may be regarded as an anticipated satire (since it was published in 1552) of the travel books that were to appear during the second half of the sixteenth century. We know that the first eleven chapters of the Fourth Book were handed over to the printer as early as 1547. Therefore Rabelais could not have had many published sources to work from. Still, his second stay in Rome had provided him with the opportunity of talking with André Thevet, who though he had not yet visited South America, was already collecting eyewitness reports of strange lands, peoples and customs. And he either knew or heard about Verrazzano and Cartier.

The passage I have in mind deals with Pantagruel's visit to an island whose inhabitants entertained the strangest ideas about kinship and affinity: 'For although all of them were kinsmen or affines to one another, we found that none of them was father or mother, brother or sister, uncle or aunt, nephew or niece, son-in-law or daughter-in-law, godfather or godmother, to any other. Except indeed, for a tall, elderly man who, as I myself heard, called a small girl, three or four years old, "father" while the child addressed him as "daughter".' (1920).

That this system of appellations smacks of the Eskimo is all the more startling in view of the name Rabelais gives these people: *Ennase*, 'No-nose', because, he says, men, women and children had noses shaped like an ace of clubs, suggesting a depressed bridge, and wide alae. But what should interest us most is to find stated at such an early date the exigency for internal coherence which we have become accustomed to look for as a criterium of validity when confronted with a list of kinship terms.

Nor is that all. Proceeding with the description of the system, Rabelais explains that the noseless people did not conceive of kinship and affinity as we do, but rather on the model of such functional relationships as those between axe and handle, bench and law-suit, crumb and crust, shovel and poker, oyster and shell, bean and pod, slipper and shoe, whip and spinning-top, hole and plug, bag and pipe, as well as some others which it would be too difficult or improper to translate! This way of thinking reminds us of the joking relationships between clansmen and women among the Luapula of Northern Rhodesia so vividly described by I. G. Cunnison (1959). It also emphasises another theoretical requirement formulated by most of us when trying to explain a kinship system: namely, that the ways parents and affines are allotted to specific classes have a meaning, and that from this allotment derive specific sets of complementary rights and obligations.

Internal coherence on the one hand, meaning and purpose on the other; thus, over four hundred years ago it had already been understood that those were the two requirements that any kinship system, however fantastic and improbable, would have to meet in order to be recognised as such. Taken together or separately, regarded at times as sustaining each other or considered as a source of mutually exclusive principles of interpretation, they have never ceased to inspire and orient the study of kinship systems, and there is no reason to believe that they will not continue to do so in the future.

Since my lecture will deal mostly with the problems raised by the second notion (to which I have given more attention in my work without ever dissociating it from the first), I shall begin by paying tribute to the important results obtained and the fascinating prospects unfolded by scholars now engaged in formulating rules whereby the internal coherence of kinship nomenclatures can be rigorously demonstrated. F. G. Lounsbury (1964 a) and I. R. Buchler (1964 a ; b) have proved that these nomenclatures manifest a kind of logical perfection which makes them authentic objects of scientific study; this approach has also permitted Lounsbury to expose the unreliability of some of the documentary material we are accustomed to handling without ever questioning its value. The fact that his analytical procedures could have resulted in a thorough reinterpretation of a system, such as that of the Iroquois, we believed we knew so well (1964: 1079, n. 4; cf. Buchler: 1964 a; b), should contribute new insights that may help us to understand similarly difficult systems from other

parts of the world. I instance that of the South American Bororo which raises problems of a similar nature. We may thus consider that by refining its analytical tools and by reducing a collection of empirical data to a set of primitive elements on the one hand, a set of rules for operating upon these elements on the other, the study of kinship may soon avail itself of a documentary material so radically renovated, that even if we assumed that the whole gamut of possible variations had been covered by past enquiries—which quite obviously is not the case—there would still remain to be done as much as has been achieved previously.

However, once we have demonstrated that they show a high degree of self-consistency, we have not, for all that, solved the problems raised by kinship systems. This property of theirs will undoubtedly provide us with adequate means to classify them, to reduce them to a number of elementary types and to set the sub-types in proper relationship to one another. But the question remains as to why they themselves perform the same office in respect to the members of social groups. We shall distinguish three main categories of answer: according to the first, the taxonomic principles adopted by a given kinship system are the indirect result of psychological and sociological factors operating on an independent level. This was Rivers' solution, when he tried to explain terminological peculiarities by certain types of marriage preferences and privileges. Strangely enough, Lounsbury himself seems to defer to this type of explanation when he suggests that what he calls 'shunts' across the genealogical network (presuming the existence of such a network prior to the one delineated by the terminology) are determined by the laws of succession. I say strangely, since his structural analysis ultimately calls upon factors that are decidedly non-structural: external instead of internal, and heterogeneous, inasmuch as in human societies kinship has a much wider range than succession, so that in order to make both fields coincide, it becomes necessary to pool status of every conceivable kind and fill up the remaining gaps with psychological affects such as 'uneasiness' (1964 b: 384), which may indeed occur here and there, but cannot account for the rigid properties of a conceptual system.

The second answer is best exemplified by Radcliffe-Brown, who claims that there is close correspondence between the structure of a kinship system and the network of rights and obligations, so that each society recognises as many types of kinsmen and women as there are distinct and prescribed modes of behaviour between them. But while anthropologists are less and less convinced that such correspondence obtains in all societies, and within each society, for all acknowledged degrees of kinship and affinity, it can also be argued that both terminological and behavioural differences, whether they be parallel or at variance, are the outcome of more basic structural features which alone can account for their similarities as well as for their differences.

We thus come to the third answer which is the one I have tried to validate in my work, namely, that the function of a kinship system is to generate marriage possibilities or impossibilities, either directly between people calling one another by certain terms, or indirectly between people calling themselves by terms which are derived, according to certain rules, from the terms used by their ascendants.

It can be said that, according to the first interpretation, a kinship system is the product of one or several efficient causes, while, according to the second, it reflects or represents, as would a diagram, a homologous social configuration, and according to the third, it acts as operating agent to a system of matrimonial exchange within the community. Further on, I shall attempt to show that the last type of explanation can be conveniently applied to various pending problems in the field of kinship studies. But I must first consider the theoretical points involved, since my phrasing was intended to make clear that the much debated question of the teleological character of kinship systems does not pertain in particular to the prescriptive rules of matrilateral or patrilateral cousin marriage. Indeed, we meet with it at a more general level, whenever we state that the primary function of a kinship system is to define categories from which to set up a certain type of marriage regulations.

A kinship system thus appears as a means toward an end. But is this not precisely the postulate which has been criticised on the grounds that, by introducing in our field consideration of final causes, it conflicts with the requirements of a truly scientific explanation?

Needham is quite right, however, when he points out that for a kinship system to emerge in the first place, and for it to remain operative, the people who make use of it need not be aware of the type of solidarity it generates among them (1962: 26-7). A system of kinship and marriage is an institution, and institutions more likely to endure are those that function the more effectively. Far from being in opposition to the actual trend of scientific thought, I should even suggest that by following this line of reasoning, one keeps well abreast of the neo-darwinian trend of modern biology, while the advocates of efficient causes, such as G. C. Homans and D. M. Schneider in their famous book (1955), go back to Lamarck. They try to account for the birth of an institution by a trend of individual behaviour which is supposed to have repeated itself for generations, until it became an organic part of the social body.

However, the objection that a teleological outlook is distasteful to modern science need not alarm us, for it is obviously false. The ghost of teleology was exorcised over twenty years ago by A. Rosenbluth, N. Wiener and J. Bigelow who have explained teleology as a special case of determinism to be found in mechanisms capable of feed-back operations (1943). Pointing out that my approach was, in fact, teleological (and I agree with him at that), P. Maranda gives examples of the same approach to be noted in the scientific domain (1964). To his we may add still

others such as, for instance, the treatment of problems of physics by means of the theory of games and the formulation by biologists of the genetic codes in linguistic terms. Indeed, if the breach between nature and culture appears today far less wide than was believed twenty or thirty years ago, the reason is not that we have succeeded in reducing the second to the first. In fact, quite the opposite is true, for explanatory models derived from the study of cultural phenomena are now borrowed by the physical and biological sciences to account for natural ones.

The problem has another aspect to which I should like to call attention, as it appears to have been considerably overlooked, though I think it very likely that when duly recognised, new vistas will be opened up to the student of kinship systems. Those opposed to explanations that call upon the notion of final causes usually argue that individuals are unlikely to give up their freedom of choice, or what they believe to be their immediate interest, for the good of the society to which they belong. Some have even gone to the extreme of saying that, in any case, such an eventuality is unthinkable, since 'nonliterate men lacking a comparative knowledge of kinship systems could not determine which form of marriage were best' (Coult 1963). We fail to understand what in the world prevents the authors of such statements from extending their reasoning to axes and weaving looms, without the comparative knowledge of which no efficient tools could be devised, or even to languages, without the comparative knowledge of which no human society could have succeeded in elaborating a satisfactory means of verbal communication.

Only by professing the crudest form of the naturalist philosophy in vogue in the late eighteenth century can one be brought to believe that scientific knowledge is the blind product of a series of trials and errors. Today we know that this is not even true on the level of animal learning. In my own past work, I may have been trying in some degree to evade the issue when I invoked rather hastily the unconscious processes of the human mind, as if the so-called primitive could not be granted the power to use his intellect otherwise than unknowingly. But the findings of physical anthropology are extending further and further back the span of man's existence on earth and I see no reason, just because we know almost nothing of this protracted past, not to admit that plenty of theoretical thinking of the highest order has been carried on all the time, not among all the representatives of the human species —that would not even be true of ourselves—but among a small minority of learned individuals. We may assume that they were concerned not with the same problems as we are, since they did not attempt to solve them, but with some others of a different nature, among which were those dealing with kinship and marriage. Elegant solutions such as the rules of bilateral, patrilateral or matrilateral cross-cousin marriage, so well adapted to small, stable groups, or that of bride-price, or of extended prohibited degrees better suited to larger or more fluid ones, far from being the recent outcome of unconscious processes, now appear to me as true discoveries, the legacy of an age-old wisdom for which more evidence can be found elsewhere. Let us consider, for instance, the rules of poetry which are so basically similar in several respects throughout the world, from the earliest documents until the present times. Only at distant intervals did men appear to become aware of these rules, for the problems which they raise were practically forgotten between the time of the rhetoricians of the Renaissance and that of Ferdinand de Saussure. But while Saussure was confident that he had penetrated the rules of ancient poetry, he was greatly worried at not being able to find evidence that these particular rules were actually known to the poets themselves (1964; Benveniste 1964). Why not admit that they were invented in such a remote past and born along from generation to generation, that they became imbedded, as it were, in the poetic tradition, exactly as in our own case, until Saussure rediscovered them?

If social anthropologists were half as interested in material culture as they ought to be, they would probably have paid some attention to Carl Schuster's fascinating survey of the world-wide occurence of a type of geometrical pattern which, from its geographical distribution and from some known early examples, he thinks goes back to palaeolithic times (1956-8). These patterns are best understood when compared with kinship diagrams not unlike those used by modern anthropologists. Let us recall that in Australia and Melanesia, natives have been actually observed making such drawings. If Schuster is right, not only the facts of kinship but the theory as well may be scores of thousands years old. What we have painstakingly unearthed beneath the facts might be nothing else than this age-old theory, just as Saussure did in his researches on poetry. None the less Saussure has the advantage of being aware that he was not supplementing the facts with a theory suitable to explain them in retrospect, but rediscovering the true and earlier theory which has given birth to the facts.

Mythology deals with problems of kinship in ways of its own which clearly show that their theoretical implications are understood. In a book to be published next year, I was led to compare the family relationships involved in the respective plots of two groups of myths from the same South American tribes. One group has to do with the origin of fire, cooking and meat, the other with the origin of cultivated plants which is said to have followed the other series of events. In both cases, the story tells of a conflict between individuals related by marriage, but the degree of affinity is not the same in each group. The myths devoted to the acquisition of fire and meat bring into play in-laws who are either a wife's brother or a husband's sister, while the myths concerned with the beginning of cultivation name the wife's sister and the husband's brother. Now, if as E. R. Leach, Needham and myself have always maintained, marriage must be envisaged as a kind of exchange between

social units, then there should be a big difference between the two kinds of relatives implicated in the myths. A wife's brother and a husband's sister are what can be called 'compulsory' in-laws, inasmuch as, in theory at least, no man may obtain a wife if he does not surrender a sister who—as a wife for somebody else—will directly or indirectly fill up the gap left by the departure of somebody else's sister. The other kind of in-laws, that is a wife's sister or a husband's brother, play no part in this reciprocal game. Their presence is not required for exogamous marriage to take place: they could just as well not be there, and the myths imply even that it would be much better, since the only social role which they seem to think up for such in-laws is that of potential seducers of their sibling's spouse.

What renders the distinction still more interesting is that the same myths link the birth of agriculture with population increase, dispersal of the primal group and diversification of languages and customs. It can thus be said that by making a clear opposition between 'cross' in-laws and 'parallel' in-laws, the first considered necessary and the second contingent, myths use what, for the sake of argument, I shall call a 'palaeolithic' and a 'neolithic' model, without loading these terms with historical content. And since the first model serves to explain the origin of *culture*, while the second has to do with *society* when it becomes unmanageable on account of too many members and of their haphazard repartition, the question may be raised as to whether the superiority in functional yield, if I may so express myself, which most societies attribute to cross-relationships over parallel ones, far from being the outcome of unconscious processes, does not stem from a mature and well-balanced reflection.

As a first consequence, I would suggest that the capacity of the so-called primitives for theoretical thinking of a quite abstract nature deserves a great deal more respect than we usually give it when inquiring into the 'efficient' causes of their systems of kinship and marriage. These causes may often reside in a correct forecast of the very results which it takes the more sophisticated kind of anthropological theory to deduce. A second consequence is that models do not exist solely in the minds of anthropologists, but are to be found in the minds of the native themselves, and in conscious form far more often than superficial evidence would lead us to believe. In most cases, however, we do not know where to look for them. Kinship systems consist therefore of two types of objects equally real: the actual working of the system within a given society on the one hand, and on the other, a model, that is a set of rules. Since the latter may not coincide with the former and even perhaps contradict it, the study of the model should be given logical priority over its empirical applications.

This, of course, runs counter to the present trend of kinship studies, which advocates that not models, but real peoples and empirical terminologies be taken as the starting point. I shall not deny the importance of the results that have been obtained by approaching the problem in this way. Suffice it to mention Needham's fruitful re-evaluation of the ethnographic data available for several tribes, and his patient unravelling of the actual network of matrimonial links. On the other hand, I wonder if too strong a tendency to make the model stick to empirical reality may not suscitate false problems which will needlessly block the way. Take, for instance, the recent discussions as to the possible existence of patrilateral cross-cousin marriage (Needham 1959; 1963; Lane 1962; Livingstone 1964). The model of such a system does undoubtedly exist, not only in the mind of the anthropologist who can represent it in diagram form, but also in that of most of the natives who advocate, permit or *reject* this formula. However, the constraints are so rigid and so numerous that if put into practice the system would soon break down, or only endure at the cost of numberless irregularities. But is that not always the case? We are greatly indebted to P. Kundstadter and his associates for having demonstrated by computer methods that even in a population with so-called 'prescriptive' cross-cousin marriage, only twenty-five to thirty per cent. of all marriages could actually conform to the pattern (1963). I cannot agree with him, however, when he concludes 'that no description of a marriage system, even in ideal terms, is complete without a statement of the demographic conditions within which the system operates'. For ideal terms—that is, the model—take no more into account demographic conditions than the physicist's definition of a crystal takes into account the local conditions of heat, pressure and the intrusion of foreign bodies, all of which prevent empirical crystals from assuming a perfect shape. If computer simulations of kinship problems have made so little headway in recent years, could it not be because of too hasty a desire to handle simultaneously a set of rules, which of themselves can present immense problems (as I shall attempt to show later), and demographic factors which the people who have conceived the rules are all the more unwilling to consider since they devised them with the ever-frustrated intent of making the demographic factors inoperative? From what is actually known, the programmes of Randolph, Coult and Frederickson in America, of Izard in France (Hymes 1965; Coult 1965) seem to be more cautiously oriented. The foundation for the growth of structural linguistics has been Saussure's distinction between *la langue* and *la parole* which has permitted us, during the formative period at least, to free the study of the former from consideration of the latter. In similar manner, and since many kinship systems function according to rules that are too complicated to be analysed manually, let us first ask computers to explicate the consequences for what I called earlier a 'palaeolithic' model, that is the model of a population constant throughout time and composed solely of the individual terms required for the system to remain in operation (the principle of sibling equivalence was not invented by anthropologists alone?) Only when the rules of these difficult

games are known and understood, may we venture to play them with models behaving like real populations.

If we are to assume that for the time being, the study of kinship systems should remain first and foremost a study of models rather than of empirical realities, what place can be left to the distinction between 'prescriptive' and 'preferential' marriage systems, which has become almost a classic in contemporary British social anthropology? Since such distinguished scholars as Leach and Needham have fully endorsed it and are obtaining important results from the use of it, I feel somewhat embarrassed to confess that I fail to grasp its significance and fear that it may give rise to many a theoretical difficulty.

It seems to me that by prescriptive marriage systems two widely different things can be meant, one of which is supposed to differ in nature, the other in degree, from the so-called 'preferential' systems.

There has been a tendency of late to limit the scope of prescriptive systems to societies which do not define their marriage rules in terms of kinship, but rather in terms of social groups which may or may not give wives to or receive wives from one another. Since few societies of this type are known and since their number is being further reduced by the exacting requirements made by the advocates of this interpretation, I find myself in the puzzling situation of having written an enormous book whose theoretical implications are recognised, though, practically, it would deal with no more than a dozen or so societies. This being the case, it becomes hard to understand (at least for me) why the study of such a limited number of tribes should have such important bearing upon the theory of kinship.

Furthermore, if we exclude all consideration of marriage preferences expressed in terms of kinship degrees, the definition of the social structure will become empty and tautological, since all we shall know about the marriage system of these tribes is that each group is supposed to receive its wives from 'wife-giving' groups and to give its daughters to groups concerning which nothing can be said either, except that they are 'wife-takers' in relation to the former[1]. Leach (1954 iv, viii) and Needham (1962 iv) have indeed shown that the empirical network may be intricate enough to place us in this awkward situation (1964). But I feel sure, contrary to B. Lane (1961)[2], and L. G. Loffler (1964) that the ideal model which would enable us to understand the workings of asymmetric systems such as these (and which is present in the minds of the people themselves when they state that they permit marriage with the mother's brother's daughter, but not with the father's sister's daughter, or that it is a good thing to marry a matrilateral cross-cousin) must rest in the long run, upon the notion of permitted or prohibited degree. For it is only by turning back to this notion that we may understand the significant features of the system, if only in an ideal situation in which marriages would always occur between the same groups. As a matter of fact, Lane's argumentation is entirely based upon the special case illustrated by an asymmetric system operating among four groups, and we are still waiting for her to demonstrate how it may apply to five or seven.

As J. P. B. de Josselin de Jong has already pointed out (1952), there can be no doubt that the diagram of an Aranda system as well as that of a patrilateral system include long cycles. In the latter case, however, I find myself at variance with D. H. P. Maybury-Lewis' recent paper (1965), for nothing more is involved there than an optical illusion. That the short cycles, implying the notion of kinship degree, constitute the one and only pertinent feature of patrilateral systems, is amply demonstrated by native ideology all over the world, which calls upon the notion of a prompt return, either to commend or to condemn this type of marriage. It is better to agree with the universal judgement of the natives than to be at odds not only with the evidence, but with oneself, in stating simultaneously that long cycles must exist because they can be visualised in the diagram, and that patrilateral systems could not succeed in closing even the shortest cycles. For this would mean confusing empirical reality not only with the model, but also with the diagram.

If we are to understand the meaning of an asymmetrical system, whether patrilateral or matrilateral, we cannot avoid taking into account the philosophical relevance, if I may so express myself, of permitted, preferred or prohibited degrees, *at the level of the model*. This is the only clue to what Radcliffe-Brown would have called the 'spirit' of those systems (1958: 145). Hence it follows that the first acceptation given the notion of 'prescriptive marriage systems' merges with the second which, if I am not mistaken, is the one used by Needham when he speaks of the 'Formal analysis of prescriptive patrilateral cross-cousin marriage". The marriage system is obviously not conceived here as operating between social groups, but between certain types of relatives. What then of the difference between 'prescriptive' and 'preferential'? It becomes, so it seems to me, a mere difference of degree, and the choice of term will depend on whether, in a given society, everybody marries according to rule or not. But what is meant by everybody? This is where the merging of model and empirical reality becomes evident, for *even a preferential system is prescriptive at the level of the model, while even a prescriptive system cannot but be preferential at the level of the reality*. Whatever the system, the choice is only between formulating strict rules that cannot be closely adhered to in practice, or framing observances of so broad a nature as to empty them of much of their content. Thus I would not hesitate to call 'prescriptive' a system which advocates marrying a mother's brother's daughter, though very few are actually doing it. For such a society is nevertheless equipped with what I shall call a 'matrilateral operator' which will progressively shape the genealogical space and imprint it with a specific curvature, which is sufficient

to place the society in question in the same group as a theoretical society in which everyone would marry according to rule, and of which the former can best be understood as an approximation.

If, in the preceding discussion, I have used rather loosely the terms 'prescriptive' and 'preferential', it is because I am unable to consider this distinction as important as the far more fundamental one between 'elementary structures' and 'complex' ones. An elementary structure may be preferential as well as prescriptive, the important fact here being that the contemplated spouse, whether preferred or prescribed, is selected for the reason that he or she belongs to a particular social group or to a given category of kinship, in other words, because the relationship between the inter-marrying pair is defined in terms pertaining to the social structure. On the other hand, we are dealing with a complex structure when the reason for the preference or the prescription does not belong to the social structure, as in the case of a girl to be married because she is lovable, pretty, small or slender, or because she belongs to a rich or influential family (this last being a social criterion, but one which remains a matter of individual appreciation since the system does not define it structurally). Thus, in the case of elementary structures as well as in that of complex ones, the use of the term preferential does not suggest a subjective inclination toward a certain degree of kinship, but states an objective fact: I call 'preferential' a system in which, notwithstanding the lack of any 'prescriptive' constraints, the rate of marriage with a given type of kin is higher than would be the case if all marriages were made at random. This objective rate, of which members of the society may remain unaware, expresses certain structural properties of the system which I assume to be isomorphic with those which are directly known to us in societies showing the same 'preference' more systematically, that is in prescriptive form.

To sum up: the difference between 'prescriptive' and 'preferential' does not appertain to the systems themselves, but to the way in which these systems are conceptualised, according to what I called elsewhere (1958) a 'mechanical' or a 'statistical' model.

It should be kept in mind, however, that the notions of 'elementary structures' and of 'complex structures' are purely heuristic—they provide a tool for investigation—and that they cannot be used alone to define a system. All systems of kinship and marriage contain an 'elementary' core which manifests itself in the incest prohibition. Similarly, all systems have a 'complex' aspect, deriving from the fact that more than one individual can usually meet the requirements of even the most prescriptive systems, thus allowing for a certain freedom of choice. This remark brings up a question which bears closely upon the future of our studies, namely whether they can progress from the field of elementary structures where they have been quartered so far, to that of complex structures?

From the answer it will receive depends the future of anthropology itself: should we try to include modern societies in our sphere of investigation, and may we do so by applying the same conceptual framework which has proved so valuable for the study of simpler societies? As long as this question remains unanswered, we cannot tackle the problem of the nature of kinship, which I should much prefer to see defined operationally than through the kind of theoretical debate which has been raging lately among my English-speaking colleagues at such high level of abstraction as to make even a 'preposterous' Frenchman shudder, if I may borrow Leach's expression (1965)! It is useless to attempt to find out what kinship really is before we succeed in ascertaining whether its field of operation covers all types of human society, and in this case to what extent, and in which way its modes of operation change as it passes from simple societies to complex ones. Coming now to the last part of my lecture, I shall try to show that the answers to these all-important questions hinge upon the solution of a problem which blocks entirely the path ahead of us, and all the more so as its very nature has never been properly understood. I am thinking of the problem raised by the so-called Crow-Omaha kinship systems.

Radcliffe-Brown (1941) and F. Eggan (1950) have undoubtedly thrown great light upon these systems by showing that they were organised along a lineage principle over-riding the genealogical principle characteristic of most of the other systems. Unfortunately, the importance of their discovery led to an immediate widening of its scope to such an extent that the notion of lineage systems came to include practically all systems which merge, upward along one line and downward along another, relatives belonging to consecutive generations. This amalgam which harks back to Lowie's early paper (1930) has remained unchallenged throughout the years and still looms large in Murdock's *Social structure* (1949: 240). The result has been to treat as variants Crow-Omaha systems and those of societies with asymmetrical cross-cousin marriage. I maintain, however, that although certain terminological resemblances may be noted, as far as their mode of functioning is concerned, those systems are as widely dissimilar as say, fish and whale.

In order to explain this dissimilarity, I shall begin with an apparently trifling remark. While there is nothing easier than to give graphic representation to the kinship systems of a society practising asymmetrical alliance, the same is not true of a Crow-Omaha system. In the first case, the diagram may take the form of a closed network spread across the surface of a cylinder which can be projected in the plane. But Crow-Omaha systems cannot be represented in drawing, or at least, as Radcliffe-Brown and Eggan have taught us, require several diagrams instead of one, each of which would correspond to a narrow perspective view upon the system. There is a deep reason for this discrepancy: an asymmetrical marriage system is tri-dimensional, while a Crow-Omaha system calls on many more dimensions[3], because on the one hand, the position of any descent line within the system is a complex function of perhaps as

many clans or descent lines as the system may include, and on the other, because the system unfolds through time, and consequently a time dimension should be added to the spatial ones. For each marriage changes the structural pattern according to which marriages may or may not take place in the following generations.

Let us now consider how a keen observer like Deacon undertakes the description of a system belonging to the Crow type, that of the Seniang. He states that 'the selection of a consort is controlled by a number of prohibitions rather than by injunctions' and he adds that 'theoretically indeed, a man should not marry a woman belonging to a clan into which a man of his own clan has married within living memory' (1930: 134). One needs only reverse the wording of these two criteria to get a perfectly good definition of an asymmetrical marriage system. There, in fact, the choice of a mate is controlled by a single injunction: for a man to marry a mother's brother's daughter or a girl belonging to a wife-giving group. A man should also marry a woman belonging to a clan into which men of his own clan have already married within living memory.

I am not implying that all systems of the Crow-Omaha family lack any type of injunction or allow such a wide range of possibilities. For instance, among the matrilineal Cherokee, marriage prohibitions are limited to two clans, that of the mother and that of the father, while it is recommended that a man marry a 'grand-mother', in other words a girl belonging to either the mother's father's or the father's father's clan (Gilbert 1943 : 208). In theory, the Hopi forbade marriage within one own's, one's father's or one's mother's father's phratry (Eggan 1950 : 121). If such societies were limited to the clans or phratries of the two grand-fathers and the two grand-mothers, their marriage regulation would more or less be of the Kariera or Aranda type, which would mean that in order to find a suitable mate each individual would have to drop two or three descent lines and pick up a fourth. But all Crow-Omaha systems have more than four descent lines: seven among the Cherokee, ten among the Omaha, thirteen among the Crow and probably more in the past, twelve phratries and about fifty clans among the Hopi, thirty to forty clans among the Seniang. Since marriage is allowed with all the clans that are not under specific interdictions, the Aranda-like structure, which is the limit (in a mathematical sense) of a Crow-Omaha system, will remain, so to speak, immersed in a flow of probabilistic events. Without ever crystallising, its spectrum alone will materialise faintly here and there, in an otherwise undifferentiated medium.

And in most cases, this will not even happen, for the generalised definition of a Crow-Omaha system may best be formulated by saying that whenever a descent line is picked up to provide a mate, all individuals belonging to that line are excluded from the range of potential mates for the first lineage, during a period covering several generations. Since this process repeats itself with each marriage, the system is kept in a state of permanent turbulence which is quite the reverse of that regularity of functioning and periodicity of returns which conform with the ideal model of an asymmetric marriage system. While the latter could be said to work like a clock whose entire movement is enclosed within its case, the former would more nearly resemble a pump which requires an external supply to draw upon according to its needs, and an outlet through which to restitute the by-products of its ongoing operations.

It would then be misleading to liken the Crow-Omaha systems to an asymmetrical terminology on the grounds that in both cases, one type of cross-cousin is moved up one generation and the other type down one generation, for by so doing, we should be overlooking what turns out to be an essential difference. An asymmetric system makes one cross-cousin into a 'father-in-law' and the other into a 'son-in-law', that is, into people belonging to the line I can marry into or who can marry into mine, while the Crow-Omaha systems (if I may use an over-simplified formula) make them into 'father' and 'son', in other words, into people with whose line it is impossible to marry. *Thus, an asymmetrical system endeavours to turn kinsmen into affines, whereas a Crow-Omaha system takes the opposite stand by turning affines into kinsmen.* In so doing, they are both serving symmetrical if inverted purposes: in one case, the aim is to make either possible or compulsory for matrimonial alliances to be kept within the circle of kin, and in the other, to make it either possible or compulsory for kinship and affinity to become mutually exclusive ties.

The last formula accounts for the strategic position which we must concede to Crow-Omaha systems in the theory of kinship and marriage. For they provide the hinge which articulates elementary structures (whose perfect expression can be found in the first type), with complex structures (which find their perfect expression in the second). In fact the Crow-Omaha systems still belong to the elementary structures from the point of view of the marriage prohibitions they frame in sociological terms, but they already belong to the complex structures from the point of view of the probabilist alliance network which they produce. In my terminology, they use a negative, mechanical model at the level of the norms, to generate a positive, statistical model at the level of the facts.

Of course, the same can be said of complex structures, since the incest prohibitions that we deem sufficient to ensure a probability distribution of alliance links coextensive with the society itself, still persists among us as a mechanical device. There is, however, a difference: our mechanical model is much lighter than the Crow-Omaha ones, which extend to whole descent lines while ours include only a few prohibited degrees. Conversely, we may surmise that the statistical results secured by a Crow-Omaha system are less substantial than our own, since they deal with small societies in which, despite the mingling effected by heavy prohibitions, an average degree of

proximity is likely to appear between marrying pairs, after a span of several generations. For the sake of the theory, it would then be of paramount importance to find out if this is effectively the case, and if so, to measure for each system this average degree of proximity. But here we encounter a considerable number of methodological hindrances.

I have asked mathematicians to translate, so to speak, Crow-Omaha systems in terms of elementary structures in the strict sense of the words. In order to effect this, each individual has to be represented by a polynome consisting of as many figures as there are clans limiting his freedom of choice in marriage. All pairs of such polynomes, in which the same figure does not appear twice, make up the list of permitted types of marriage, and these render possible or impossible other types of marriage in the following generations. I was told that according to the above computation, there are in a group containing only two prohibited clans, the mother's and the father's, twenty-three thousand four hundred and thirty-six (23,436) marriage types for an overall figure of seven clans, three million seven hundred and sixty-six thousand, one hundred and forty (3,766,140) for fifteen clans and two hundred and ninety-seven million, four hundred and twenty-three thousand, eight hundred and fifty-five (297,423,855) for thirty clans. Three clanic interdictions instead of two would lower the above figures but slightly[4].

This sounds interesting enough already, as it proves that we are dealing with figures of a very different order from those exemplified by elementary structures, and which probably bring us nearer to what we may expect to find in limited geographic areas of our own societies where a high coefficient of endogamy happens to prevail. By extending this reasoning to the last case, we may hope to bridge the gap between elementary structures and complex ones. One of my associates, Izard, has been working in that direction, but our attempts have resulted so far in over-loading the computer with more information than it can handle, so that it appears necessary to first devise methods of reducing genealogical data to a smaller body of pertinent facts along lines similar to those which I understand Coult and Randolph are following in America (1965).

But this is not all. While still finite, the figures I have quoted confront us with systems not unlike complicated games such as chequers, chess and card games, in which calculation shows that if there is a theoretical limit to the number of possible combinations, this limit runs into such high figures that for all practical purposes and on a human scale, their combinative resources remain inexhaustible. While in theory, these systems are anhistorical, since they admit of periodical returns, the latter could only occur at such prolonged intervals that in fact the games in question may be considered as remaining immersed in the stream of history. This explains why an history of the strategy of chess can be written. Similarly, Crow-Omaha systems, though formally akin to elementary structures, allow history to play a part in social life. Instead of acting as a regulating device which is constantly tending to set the society back on its old tracks, they leave it a certain measure of freedom which may lead to change. Thus it can be said that with the advent of Crow-Omaha systems, history comes to the foreground in the life of simple societies.

This coefficient of freedom is difficult to evaluate for two reasons. In the first place, the number of combinations which we should have to handle prevents us from studying the working of such systems otherwise than by computer methods. Since, however, for each generation, marriage choices are a function of those that have taken place within previous generations, we may never be able to reach an initial stage from where our simulation process could start. This seems to be one of the major problems confronting the theory of kinship.

In the second place, if Crow-Omaha systems call for a high number of marriage types, societies using these systems have apparently been composed of a much smaller number of individuals: less than five thousand in the American examples. Therefore, in each generation, only a very limited percentage of all possible marriages may have taken place: we have thus to deal with a second factor of randomness which should be considered together with the one already inherent to the system. A full account of its functioning should then combine these two factors of randomness with their opposite, namely the rigid terminology which, in turn, reflects a set of prohibitions only partly determined, since they are themselves a function of the factors of randomness. If kinship theory can be carried that far, there should emerge a meaningful whole, integrating the mechanical model of marriage prohibitions, the statistical model of proximity degrees between affines, and the terminology.

Only then shall we be equipped with the analytical tools permitting us to grapple with the problems raised by kinship and marriage in modern societies, which are probably as far removed from the Crow-Omaha problems as the latter are removed from the only ones we are able to solve at present. Have kinship and marriage ties become non-operative in our social structure, or do they still preserve some traces of the function which they fulfill in simpler societies? Recent findings such as that of a matrilocal operator in some sections of British contemporary society, suggest that a great deal remains still unknown to us. I do not imply that we would find among ourselves anything resembling marriage cycles, either closed or curved, through such cycles may still be apparent at certain levels, for instance in royal families. But if it could be shown that just as the atoms of radioactive bodies disintegrate at a fixed rate without it being possible to predict where and when the phenomenon will take place, in the same way the faint outlines of incipient cycles may be seen materialising here and there, and if their rate of appearance, their average length and their orientation could be determined, we should more nearly approach understanding what

kinship really is, and what obscure forces remain at work in the more advanced societies.

For such a program to be initiated, the conceptual framework of our studies will have to undergo a transformation whose magnitude is comparable to the one which may be said to exist between Keplerian and quantum mechanics. For the world we should prepare ourselves to enter, will no longer be composed of commutative classes and networks endowed with a periodical structure, but of unpredictable events, whose statistical distribution only will show regularities and provide meaningful clues. But has not the study of large African societies, exemplified in the work of Evans-Pritchard, Fortes, Forde, Schapera, Gluckman, Nadel and many others, already taught us that significant regularities cannot always be found at the level of preferred degrees but should be drawn out of complex clusters of relations between local groups? For the past twenty or thirty years, new observations and interpretations have been piling up from the four corners of the Earth, treating of bilateral descent groups in Polynesia by Firth, of diachronic models for rules of filiation and residence by Fortes and Goodenough, of the Arab system by Cuisenier, of the Indian ones by Dumont, and of Polynesian and Melanesian systems which compensate a certain freedom of clan affiliation by surrounding it, so to speak, with an exoskeleton made up of rights and obligations based upon the ownership of land. Attempts to integrate these findings have split the seams of the fabric that held our traditional categories together. Even though we should evade the results of over-bold theoretical speculation, the very facts would compel us to a more flexible outlook, and to devise new methods of research, thus keeping ourselves in readiness for the tasks that lie ahead. May I predict that in so doing we will find ourselves more and more in agreement with native theories, either expressedly formulated or still hidden in symbolic representations, rituals and mythologies. For, as Thomas Henry Huxley whose memory we are celebrating tonigth once wrote: 'Ancient traditions, when tested by severe processes of modern investigation, commonly enough fade away into mere dreams: but it is singular how often the dream turns out to have been a half-waking one, presaging a reality' (1863: 1).

NOTES

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1. A similar objection was formulated in a somewhat different form by D. M. Schneider (1965).

2. Lane failed to understand that what I have called '*échange generalise*' should not be defined by circularity *versus* reciprocal exchange, but by the capacity for the former to organise within an unchanging structure any number of participating units, thus freeing the structure from the contingency of historical processes which could more easily ruin it were it more rigid. That Australian systems with sections and subsections may be considered as cyclic in some way has long been known. But the cycles include a fixed number of units which must remain even. Should a Kariera system change, its four units will resolve into two or eight, but never in an odd number.

3. In my book *Anthropologie structurale*, 84, I mentioned two and three dimensions respectively, but I was then discussing the problem in a limited perspective which did not require more dimensions. However, the fundamental difference between Crow-Omaha and other systems in terms of dimensions was clearly emphasised although on a smaller scale.

4. I wish to thank Dr Bernard Jaulin, the head of the *Centre de Calcul* of the *Maison des Sciences de l'Homme*, Paris, who devised a method and made the computations.

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