

CHAPTER XV

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## Social Structure

The investigations we may enter into, in treating this subject, must not be considered as historical truths, but only as mere conditional and hypothetical reasonings, rather calculated to explain the nature of things, than to ascertain their actual origin; just like the hypotheses which our physicists daily form respecting the formation of the world.

J.-J. Rousseau, *On the Origin of Inequality*

**T**HE TERM "social structure" refers to a group of problems the scope of which appears so wide and the definition so imprecise that it is hardly possible for a paper strictly limited in size to meet them fully. This is reflected in the program of this symposium, in which problems closely related to social structure have been allotted to several papers, such as those on "Style," "Universal Categories of Culture," and "Structural Linguistics." These should be read in connection with the present paper.

On the other hand, studies in social structure have to do with the formal aspects of social phenomena; they are therefore difficult to define, and still more difficult to discuss, without overlapping other fields pertaining to the exact and natural sciences, where problems are similarly set in formal terms or, rather, where the formal expression of different problems admits of the same kind of treatment. As a matter of fact, the main interest of social-structure studies seems to be that they give the anthropologist hope that, thanks to the formalization of his problems, he may

borrow methods and types of solutions from disciplines which have gone far ahead of his own in that direction.

Such being the case, it is obvious that the term "social structure" needs first to be defined and that some explanation should be given of the difference which helps to distinguish studies in social structure from the unlimited field of descriptions, analyses, and theories dealing with social relations at large, which merge with the whole scope of social anthropology. This is all the more necessary, since some of those who have contributed toward setting apart social structure as a special field of anthropological studies conceived the former in many different manners and even sometimes, so it seems, came to nurture grave doubts as to the validity of their enterprise. For instance, Kroeber writes in the second edition of his *Ambropology*:

"Structure" appears to be just a yielding to a word that has a perfectly good meaning but suddenly becomes fashionably attractive for a decade or so—like "streamlining"—and during its vogue tends to be applied indiscriminately because of the pleasurable connotations of its sound. Of course a typical personality can be viewed as having a structure. But so can a physiology, any organism, all societies and all cultures, crystals, machines—in fact everything that is not wholly amorphous has a structure. So what "structure" adds to the meaning of our phrase seems to be nothing, except to provoke a degree of pleasant puzzlement.<sup>1</sup>

Although this passage concerns more particularly the notion of "basic personality structure," it has devastating implications as regards the generalized use of the notion of structure in anthropology.

Another reason makes a definition of social structure compulsory: From the structuralist point of view which one has to adopt if only to give the problem its meaning, it would be hopeless to try to reach a valid definition of social structure on an inductive basis, by abstracting common elements from the uses and definitions current among all the scholars who claim to have made "social structure" the object of their studies. If these concepts have a meaning at all, they mean, first, that the notion of structure has a structure. This we shall try to outline from the beginning as a precaution against letting ourselves be submerged by a tedious inventory of books and papers dealing with social relations, the

mere listing of which would more than exhaust the limited space at our disposal. At a further stage we will have to see how far and in what directions the term "social structure," as used by the different authors, departs from our definition. This will be done in the section devoted to kinship, since the notion of structure has found its chief application in that field and since anthropologists have generally chosen to express their theoretical views also in that connection.

#### DEFINITION AND PROBLEMS OF METHOD

Passing now to the task of defining "social structure," there is a point which should be cleared up immediately. The term "social structure" ~~has nothing to do with~~ empirical reality but with models which are built up after it. This should help one to clarify the difference between two concepts which are so close to each other that they have often been confused, namely, those of *social structure* and of *social relations*. It will be enough to state at this time that social relations consist of the raw materials out of which the models making up the social structure are built, while social structure can, by no means, be reduced to the ensemble of the social relations to be described in a given society.<sup>2</sup> Therefore, social structure cannot claim a field of its own among others in the social studies. It is rather a method to be applied to any kind of social studies, similar to the structural analysis current in other disciplines.

The question then becomes that of ascertaining what kind of model deserves the name "structure." This is not an anthropological question, but one which belongs to the methodology of science in general. Keeping this in mind, we can say that a structure consists of a model meeting with several requirements.

First, the structure exhibits the characteristics of a system. It is made up of several elements, none of which can undergo a change without effecting changes in all the other elements.

Second, for any given model there should be a possibility of ordering a series of transformations resulting in a group of models of the same type.

Third, the above properties make it possible to predict how the model will react if one or more of its elements are submitted to certain modifications.

Finally, the model should be constituted so as to make immediately intelligible all the observed facts.<sup>3</sup>

These being the requirements for any model with structural value, several consequences follow. These, however, do not pertain to the definition of structure, but have to do with the chief properties exhibited and problems raised by structural analysis when contemplated in the social and other fields.

*Observation and Experimentation.* Great care should be taken to distinguish between the observational and the experimental levels. To observe facts and elaborate methodological devices which permit the construction of models out of these facts is not at all the same thing as to experiment on the models. By "experimenting on models," we mean the set of procedures aiming at ascertaining how a given model will react when subjected to change and at comparing models of the same or different types. This distinction is all the more necessary, since many discussions on social structure revolve around the apparent contradiction between the concreteness and individuality of ethnological data and the abstract and formal character generally exhibited by structural studies. This contradiction disappears as one comes to realize that these features belong to two entirely different levels, or rather to two stages of the same process. On the observational level, the main—rule is that all the facts should be carefully observed and described, without allowing any theoretical preconception to decide whether some are more important than others. This rule implies, in turn, that facts should be studied in relation to themselves (by what kind of concrete process did they come into being?) and in relation to the whole (always aiming to relate each modification which can be observed in a sector to the global situation in which it first appeared).

This rule together with its corollaries has been explicitly formulated by K. Goldstein<sup>4</sup> in relation to psychophysiological studies, and it may be considered valid for any kind of structural analysis. Its immediate consequence is that, far from being contradictory, there is a direct relationship between the detail and concreteness of ethnographical description and the validity and generality of the model which is constructed after it. For, though many models may be used as convenient devices to describe and

explain the phenomena, it is obvious that the best model will always be that which is *true*, that is, the simplest possible model which, while being derived exclusively from the facts under consideration, also makes it possible to account for all of them. Therefore, the first task is to ascertain what those facts are.

*Consciousness and Unconsciousness.* A second distinction has to do with the conscious or unconscious character of the models. In the history of structural thought, Boas may be credited with having introduced this distinction. He made clear that a category of facts can more easily yield to structural analysis when the social group in which it is manifested has not elaborated a conscious model to interpret or justify it.<sup>5</sup> Some readers may be surprised to find Boas' name quoted in connection with structural theory, since he has often been described as one of the main obstacles in its path. But this writer has tried to demonstrate that Boas' shortcomings in matters of structural studies did not lie in his failure to understand their importance and significance, which he did, as a matter of fact, in the most prophetic way. They rather resulted from the fact that he imposed on structural studies conditions of validity, some of which will remain forever part of their methodology, while some others are so exacting and impossible to meet that they would have withered scientific development in any field.<sup>6</sup>

A structural model may be conscious or unconscious without this difference affecting its nature. It can only be said that when the structure of a certain type of phenomena does not lie at a great depth, it is more likely that some kind of model, standing as a screen to hide it, will exist in the collective consciousness. For conscious models, which are usually known as "norms," are by definition very poor ones, since they are not intended to explain the phenomena but to perpetuate them. Therefore, structural analysis is confronted with a strange paradox well known to the linguist, that is: the more obvious structural organization is, the more difficult it becomes to reach it because of the inaccurate conscious models lying across the path which leads to it.

From the point of view of the degree of consciousness, the anthropologist is confronted with two kinds of situations. He may have to construct a model from phenomena the systematic charac-

ter of which has evoked no awareness on the part of the culture; this is the kind of simpler situation referred to by Boas as providing the easiest ground for anthropological research. Or else the anthropologist will be dealing on the one hand with raw phenomena and on the other with the models already constructed by the culture to interpret the former. Though it is likely that, for the reasons stated above, these models will prove unsatisfactory, it is by no means necessary that this should always be the case. As a matter of fact, many "primitive" cultures have built models of their marriage regulations which are much more to the point than models built by professional anthropologists.<sup>7</sup> Thus one cannot dispense with studying a culture's "home-made" models for two reasons. First, these models might prove to be accurate or, at least, to provide some insight into the structure of the phenomena; after all, each culture has its own theoreticians whose contributions deserve the same attention as that which the anthropologist gives to colleagues. And, second, even if the models are biased or erroneous, the very bias and type of error are a part of the facts under study and probably rank among the most significant ones. But even when taking into consideration these culturally produced models, the anthropologist does not forget—as he has sometimes been accused of doing<sup>8</sup>—that the cultural norms are not of themselves structures. Rather, they furnish an important contribution to an understanding of the structures, either as factual documents or as theoretical contributions similar to those of the anthropologist himself.

This point has been given great attention by the French sociological school. Durkheim and Mauss, for instance, have always taken care to substitute, as a starting point for the survey of native categories of thought, the conscious representations prevailing among the natives themselves for those stemming from the anthropologist's own culture. This was undoubtedly an important step, which, nevertheless, fell short of its goal because these authors were not sufficiently aware that native conscious representations, important as they are, may be just as remote from the unconscious reality as any other.<sup>9</sup>

*Structure and Measure.* It is often believed that one of the main interests of the notion of structure is to permit the introduc-

tion of measurement in social anthropology. This view has been favored by the frequent appearance of mathematical or semimathe-matical aids in books or articles dealing with social structure. It is true that in some cases structural analysis has made it possible to attach numerical values to invariants. This was, for instance, the result of Kroeber's study of women's dress fashions, a landmark in structural research,<sup>10</sup> as well as of a few other studies which will be discussed below.

However, one should keep in mind that there is no necessary connection between *measure* and *structure*. Structural studies are, in the social sciences, the indirect outcome of modern developments in mathematics which have given increasing importance to the qualitative point of view in contradistinction to the quantitative point of view of traditional mathematics. It has become possible, therefore, in fields such as mathematical logic, set theory, group theory, and topology, to develop a rigorous approach to problems which do not admit of a metrical solution. The outstanding achievements in this connection—which offer themselves as springboards not yet utilized by social scientists—are to be found in J. von Neumann and O. Morgenstern, *Theory of Games and Economic Behavior*;<sup>11</sup> N. Wiener, *Cybernetics*;<sup>12</sup> and C. Shannon and W. Weaver, *The Mathematical Theory of Communication*.<sup>13</sup>

*Mechanical Models and Statistical Models.* A last distinction refers to the relation between the scale of the model and that of the phenomena. According to the nature of these phenomena, it becomes possible or impossible to build a model, the elements of which are on the same scale as the phenomena themselves. A model the elements of which are on the same scale as the phenomena will be called a "mechanical model"; when the elements of the model are on a different scale, we shall be dealing with a "statistical model." The laws of marriage provide the best illustration of this difference. In primitive societies these laws can be expressed in models calling for actual grouping of the individuals according to kin or clan; these are mechanical models. No such distribution exists in our own society, where types of marriage are determined by the size of the primary and secondary groups to which prospective mates belong, social fluidity, amount of information, and

the like. A satisfactory (though yet untried) attempt to formulate the invariants of our marriage system would therefore have to determine average values—thresholds; it would be a statistical model. There may be intermediate forms between these two. Such is the case in societies which (as even our own) have a mechanical model to determine prohibited marriages and rely on a statistical model for those which are permissible. It should also be kept in mind that the same phenomena may admit of different models, some mechanical and some statistical, according to the way in which they are grouped together and with other phenomena. A society which recommends cross-cousin marriage but where this ideal marriage type occurs only with limited frequency needs, in order that the system may be properly explained, both a mechanical and a statistical model, as was well understood by Forde and Elwin.<sup>14</sup>

It should also be kept in mind that what makes social-structure studies valuable is that structures are models, the formal properties of which can be compared independently of their elements. The structuralist's task is thus to recognize and isolate levels of reality which have strategic value from his point of view, namely, which admit of representation as models, whatever their type. It often happens that the same data may be considered from different perspectives embodying equally strategic values, though the resulting models will be in some cases mechanical and in others statistical. This situation is well known in the exact and natural sciences; for instance, the theory of a small number of physical bodies belongs to classical mechanics, but if the number of bodies becomes greater, then one should rely on the laws of thermodynamics, that is, use a statistical model instead of a mechanical one, though the nature of the data remains the same in both cases.

The same situation prevails in the human and the social sciences. If one takes a phenomenon such as suicide, for instance, it can be studied on two different levels. First, it is possible by studying individual situations to establish what may be called mechanical models of suicide, taking into account in each case the personality of the victim, his or her life history, the characteristics of the primary and secondary groups in which he or she developed, and the like; or else one can build models of a statistical nature, by recording suicide frequency over a certain period of time in one or more societies and in different types of primary and secondary groups,

etc. These would be levels at which the structural study of suicide carries a strategic value, that is, where it becomes possible to build models which may be compared (1) for different types of suicides, (2) for different societies, and (3) for different types of social phenomena. Scientific progress consists not only in discovering new invariants belonging to those levels but also in discovering new levels where the study of the same phenomena offers the same strategic value. Such a result was achieved, for instance, by psychoanalysis, which discovered the means to set up models in a new field, that of the psychological life of the patient considered as a whole.

The foregoing should help to make clear the dual (and at first sight almost contradictory) nature of structural studies. On the one hand, they aim at isolating strategic levels, and this can be achieved only by "carving out" a certain constellation of phenomena. From that point of view, each type of structural study appears autonomous, entirely independent of all the others and even of different methodological approaches to the same field. On the other hand, the essential value of these studies is to construct models the formal properties of which can be compared with, and explained by, the same properties as in models corresponding to other strategic levels. Thus it may be said that their ultimate end is to override traditional boundaries between different disciplines and to promote a true interdisciplinary approach.

An example may be given. A great deal of discussion has taken place lately about the difference between history and anthropology, and Kroeber and others have made clear that the time dimension is of minor significance in this connection.<sup>15</sup> From what has been stated above, one can see exactly where the difference lies, not only between these two disciplines but also between them and others. Ethnography and history differ from social anthropology and sociology, inasmuch as the former two aim at gathering data, while the latter two deal with models constructed from these data. Similarly, ethnography and social anthropology correspond to two different stages in the same research, the ultimate result of which is to construct mechanical models, while history (together with its so-called "auxiliary" disciplines) and sociology end ultimately in statistical models. The relations between these four disciplines may thus be reduced to two oppositions, one

between empirical observation and model building, which characterizes the initial stage of research, and the other between the statistical and the mechanical nature of models, which constitutes the products of research. By arbitrarily assigning the sign + to the first term of each opposition and the sign - to the second, we obtain the following chart:

	HISTORY	SOCIOLOGY	ETHNOG- RAPHY	SOCIAL ANTHRO- POLOGY
empirical observation/ model building	+	-	+	-
mechanical models/ statistical models	-	-	+	+

This is the reason why the social sciences, though they all have to do with the time dimension, nevertheless deal with two different categories of time. Anthropology uses a "mechanical" time, reversible and non-cumulative. For instance, the model of, let us say, a patrilineal kinship system does not in itself show whether or not the system has always remained patrilineal, or has been preceded by a matrilineal form, or by any number of shifts from patrilineal to matrilineal and vice versa. On the contrary, historical time is "statistical"; it always appears as an oriented and non-reversible process. An evolution which would take contemporary Italian society back to that of the Roman Republic is as impossible to conceive of as is the reversibility of the processes belonging to the second law of thermodynamics.

This discussion helps to clarify Firth's distinction between social structure, which he conceives as outside the time dimension, and social organization, where time re-enters.<sup>16</sup> Also in this connection, the debate which has been going on for the past few years between followers of the Boasian anti-evolutionist tradition and of Professor Leslie White<sup>17</sup> may become better understood. The Boasian school has been mainly concerned with models of a mechanical type, and from this point of view the concept of evolution has no operational value. On the other hand, it is certainly legitimate to speak of evolution in a historical and sociological sense, but the elements to be organized into an evolutionary process cannot be

borrowed from the level of a cultural typology which consists of mechanical models. They should be sought at a sufficiently deep level to insure that these elements will remain unaffected by different cultural contexts (as, let us say, genes are identical elements combined into different patterns corresponding to the different racial [statistical] models) and can accordingly permit the drawing of long statistical runs. Boas and his followers are therefore right in rejecting the concept of evolution, since it is not relevant on the level of the mechanical models which they employ exclusively. As for Leslie White, he is mistaken in his attempts to reintroduce the concept of evolution, since he persists in utilizing models of the same type as those of his opponents. The evolutionists would find it easier to regain their position if they consented to substitute statistical for mechanical models, that is, models whose elements are independent of their combinations and which remain identical through a sufficiently long period of time.<sup>18</sup>

The distinction between mechanical and statistical models has also become fundamental in another respect; it makes it possible to clarify the role of the comparative method in structural studies. This method was greatly emphasized by both Radcliffe-Brown and Lowie. The former writes:

Theoretical sociology is commonly regarded as an inductive science, induction being the logical method of inference by which we arrive at general propositions from the consideration of particular instances. Although Professor Evans-Pritchard . . . seems to imply in some of his statements that the logical method of induction, using comparison, classification and generalization, is not applicable to the phenomena of human social life . . . I hold that social anthropology must depend on systematic comparative studies of many societies.<sup>19</sup>

Writing about religion, he states:

The experimental method of social religion . . . means that we must study in the light of our hypothesis a sufficient number of diverse particular religions or religious cults in relation to the particular societies in which they are found. This is a task not for one person but for a number.<sup>20</sup>

Similarly, Lowie, after pointing out that "the literature of anthropology is full of alleged correlations which lack empirical sup-

port,"<sup>21</sup> insists on the need for a "broad inductive basis" for generalization.<sup>22</sup> It is interesting to note that by this claim for inductive support these authors dissent not only from Durkheim—"When a law has been proved by a well performed experiment, this law is valid universally,"<sup>23</sup>—but also from Goldstein, who, as already mentioned, has lucidly expressed what may be called "the rules of structuralist method" in a way general enough to make them valid outside the more limited field in which they were first applied by their author. Goldstein remarks that the need to make a thorough study of each case implies that the amount of cases to be studied should be small; and he proceeds by raising the question whether or not the risk exists that the cases under consideration may be special ones, allowing no general conclusions about the others. His answer is as follows:

This objection completely misunderstands the real situation . . . an accumulation of facts even numerous is of no help if these facts were imperfectly established; it does not lead to the knowledge of things as they really happen. . . . We must choose only those cases which permit of formulating final judgments. And then, what is true for one case will also be true for any other.<sup>24</sup>

Probably very few anthropologists would be ready to support these bold statements. However, no structuralist study may be undertaken without a clear awareness of Goldstein's dilemma: either to study many cases in a superficial and in the end ineffective way; or to limit oneself to a thorough study of a small number of cases, thus proving that in the last analysis one well done experiment is sufficient to make a demonstration.

Now the reason for so many anthropologists' faithfulness to the comparative method may be sought in some sort of confusion between the procedures used to establish mechanical and statistical models. While Durkheim and Goldstein's position undoubtedly holds true for the former, it is obvious that no statistical model can be achieved without statistics, that is, without gathering a large amount of data. But in this case the method is no more comparative than in the other, since the data to be collected will be acceptable only insofar as they are all of the same kind. We remain, therefore, confronted with only one alternative, namely, to make a thorough study of one case. The real difference lies in the selection of the

"case," which will be patterned so as to include elements which are either on the same scale as the model to be constructed or on a different scale.

Having thus clarified these basic questions revolving around the nature of studies in social structure, it becomes possible to make an inventory of the main fields of inquiry and to discuss some of the results achieved so far.

#### SOCIAL MORPHOLOGY OR GROUP STRUCTURE

In this section, "group" is not intended to mean the social group but, in a more general sense, the manner in which the phenomena under study are grouped together.

The object of social-structure studies is to understand social relations with the aid of models. Now it is impossible to conceive of social relations outside a common framework. Space and time are the two frames of reference we use to situate social relations, either alone or together. These space and time dimensions are not the same as the analogous ones used by other disciplines but consist of a "social" space and of a "social" time, meaning that they have no properties outside those which derive from the properties of the social phenomena which "furnish" them. According to their social structure, human societies have elaborated many types of such "continuums," and there should be no undue concern on the part of the anthropologist that, in the course of his studies, he might temporarily have to borrow types widely different from the existing patterns and eventually to evolve new ones.

We have already noticed that the time continuum may be reversible or oriented in accordance with the level of reality embodying strategic value from the point of view of the research at hand. Many other possibilities may arise: The time dimension may be conceived of as independent from the observer and unlimited or as a function of the observer's own (biological) time and limited; it may be considered as consisting of parts which are, or are not, homologous with one another, etc. Evans-Pritchard has shown how such formal properties underlie the qualitative distinctions between the observer's life span and history, legend, and myth.<sup>25</sup> His basic distinctions have been found, furthermore, to be valid for contemporary societies.<sup>26</sup>



What is true of the time dimension applies equally well to space. It has been Durkheim's and Mauss's great merit to call attention for the first time to the variable properties of space which should be considered in order to understand properly the structure of several primitive societies.<sup>27</sup> In this undertaking they received their inspiration from the work of Cushing, which it has become fashionable in recent years to belittle. However, Frank Hamilton Cushing's insight and sociological imagination entitle him to a seat on Morgan's right, as one of the great forerunners of social-structure studies. The gaps and inaccuracies in his descriptions, less serious than the indictment of having "over-interpreted" some of his material, will be viewed in their true proportions when it is realized that, albeit in an unconscious fashion, Cushing was aiming less at giving an actual description of Zuni society than at elaborating a model (his famous sevenfold division) which would explain most of its processes and structure.

Social time and space should also be characterized according to scale. There is in social studies a "macro-time" and a "micro-time"; the same distinction applies also to space. This explains why social structure may have to deal with prehistory, archaeology, and diffusion processes as well as with psychological topology, such as that initiated by Lewin or Moreno's sociometry. As a matter of fact, structures of the same type may exist on quite different time and space levels, and it is far from inconceivable that, for instance, a statistical model resulting from sociometric studies might be of greater help in building a similar model in the field of the history of cultures than an apparently more direct approach would permit.

Therefore, historico-geographical concerns should not be excluded from the field of structural studies, as was generally implied by the widely accepted opposition between "diffusionism" and "functionalism."<sup>28</sup> A functionalist may be far from a structuralist, as is clearly shown by the example of Malinowski. On the other hand, undertakings such as those of G. Dumézil,<sup>29</sup> as well as A. L. Kroeber's personal case of a highly structure-minded scholar devoting most of his time to distribution studies, are proofs that even history can be approached in a structural way.

Since synchronic studies raise fewer problems than diachronic ones (the data being more homogeneous in the first case), the simplest morphological studies are those having to do with the qualitative, non-measurable properties of social space, that is, the manner in which social phenomena can be situated on a map and the regularities exhibited in their configurations. Much might have been expected from the researches of the so-called "Chicago school" dealing with urban ecology, and the reasons for the gradual loss of interest in this line of research are not altogether clear. It has to do mostly with ecology, which was made the subject of another paper in this symposium.<sup>30</sup> However, it is not inappropriate to state at this point what kind of relationship prevails between ecology on the one hand and social structure on the other. Both have to do with the spatial distribution of phenomena. But social structure deals exclusively with those "spaces" the properties of which are of a purely sociological nature, that is, not affected by such natural determinants as geology, climatology, physiography, and the like. This is the reason why so-called urban ecology should hold great interest for the social anthropologist; the urban space is small enough and homogeneous enough (from every point of view except the social one) for all its differential qualitative aspects to be ascribed mostly to the action of internal forces accessible to structural sociology.

It would perhaps have been wiser, instead of starting with complex communities hard to isolate from external influences, to approach first—as suggested by Marcel Mauss<sup>31</sup>—those small and relatively isolated communities with which the anthropologist usually deals. A few such studies may be found,<sup>32</sup> but they rarely and then reluctantly go beyond the descriptive stage. There have been practically no attempts to correlate the spatial configurations with the formal properties of the other aspects of social life.

This is much to be regretted, since in many parts of the world there is an obvious relationship between the social structure and the spatial structure of settlements, villages, or camps. To limit ourselves to America, the camp shapes of the Plains Indians have long demanded attention by virtue of regular variations connected with the social organization of each tribe; and the same holds true for the circular disposition of huts in Ge villages of eastern and central Brazil. In both cases we are dealing with relatively homo-



geneous cultural areas where important series of concomitant variations may be observed. Another kind of problem results from the comparison of areas where different types of village structures may be compared to different types of social relations, for example, the circular-village structure of the Ge and the parallel-layers structure of the Pueblo. The latter could even be studied diachronically with the archaeologist's help, which would raise questions such as the possible linkage of the transition from semi-circular structures to parallel ones, with the shift of village sites from valley to mesa top, of the structural distribution of clan houses suggested by many myths to the present-day statistical one, etc.

These few examples are not intended to prove that spatial configuration is the mirror image of social organization but to call attention to the fact that, while among numerous peoples it would be extremely difficult to discover any such relation, among others (who must accordingly have something in common) the existence of a relation is evident, though unclear, and in a third group spatial configuration seems to be almost a projective representation of the social structure. But even the most striking cases call for a critical study; for example, this writer has attempted to demonstrate that, among the Bororo, spatial configuration reflects not the true, unconscious social organization but a model existing consciously in the native mind, though its nature is entirely illusory and even contradictory to reality.<sup>33</sup> Problems of this kind (which are raised not only by the consideration of relatively durable spatial configurations but also in regard to recurrent temporary ones, such as those shown in dance, ritual, etc.<sup>34</sup>) offer an opportunity to study social and mental processes through objective and crystallized external projections of them.

Another approach which may lead more directly to a mathematical expression of social phenomena starts with the numerical properties of human groups. This has traditionally been the field of demography, but it is only recently that a few scholars coming from different fields—demography, sociology, anthropology—have begun to elaborate a kind of qualitative demography, that is, dealing no longer with continuous variations within human groups selected for empirical reasons but with significant discontinuities

evidenced in the behavior of groups considered as wholes and chosen on the basis of these discontinuities. This "socio-demography," as it was called by one of its proponents,<sup>35</sup> is "on a level" with social anthropology, and it is not difficult to foresee that in the very near future it will be called upon to provide firm grounds for any kind of anthropological research. Therefore, it is surprising that so little attention was paid in anthropological circles to the study by a demographer, L. Livi, of the formal properties characteristic of the smallest possible size of a group compatible with its existence as a group.<sup>36</sup> His researches, closely connected with G. Dahlberg's, are all the more important for anthropologists, in that the latter usually deal with populations very close to Livi's minimum. There is an obvious relation between the functioning and even the durability of the social structure and the actual size of the population.<sup>37</sup> It is thus becoming increasingly evident that formal properties exist which are immediately and directly related to the absolute size of the population, whatever the group under consideration. These should be the first to be assessed and taken into account in an interpretation of other properties.

Next come numerical properties expressing, not the group size taken globally, but the size and interaction of subsets of the group which can be defined by significant discontinuities. Two lines of inquiry should be mentioned in this connection.

There is, first, the vast body of research deriving from the famous "rank-size law" for cities, which makes it possible to establish a correlation between the absolute size of cities (calculated on the basis of population size) and the position of each city within a rank order, and even, it appears, to infer one of the elements from the other.<sup>38</sup>

Of a much more direct bearing on current anthropological research is the recent work of two French demographers, who, by using Dahlberg's demonstration that the size of an isolate (that is, a group of intermarrying people) can be computed from the frequency of marriage between cross-cousins,<sup>39</sup> have succeeded in computing the average size of isolates in all French *départements*, thus throwing open to anthropological investigation the marriage system of a complex modern society.<sup>40</sup> The average size of the French isolate varies from less than 1,000 to over 2,800 individuals. This numerical evaluation shows that even in a modern society the

network of people united by kinship ties is much smaller than might be expected—about the same size as in primitive groups. The inference is that, while the absolute size of the intermarrying group remains approximately on the same scale in all human societies (the ratio of the French types in relation to the average primitive types being about 10 to 1), a complex society becomes such not so much because of an expansion of the isolate itself as on account of an expansion of other types of social links (economic, political, intellectual); and these are used to connect a great number of isolates which, by themselves, remain relatively static.

But the most striking result of this research is the discovery that the smallest isolates are found not only in mountain areas, as was expected, but also (and even more) in areas including a large urban center; the following *départements*: Rhône (Lyon), Gironde (Bordeaux), and Seine (Paris) are at the bottom of the list, with the size of their isolates respectively 740, 910, and 930. In the Seine *département*, which is practically limited to Paris and its suburbs, the frequency of consanguineous marriages is higher than in any of the fifteen rural *départements* which surround it.

It is not necessary to emphasize the bearing of such studies on social structure; the main fact, from the point of view of this paper, is that they, at the same time, make possible and call for an immediate extension on the anthropological level. An approach has been found which enables us to break down a modern complex society into smaller units which are of the same nature as those commonly studied by anthropologists; on the other hand, this approach remains incomplete, since the absolute size of the isolate is only a part of the phenomenon, the other one, equally important, being the length of the marriage cycles. For a small isolate may admit of long marriage cycles (that is, tending to be of the same size as the isolate itself), while a relatively large isolate can be made up of shorter cycles.<sup>41</sup> This problem, which could be solved only with the help of genealogies, points the way toward close cooperation between the structural demographer and the social anthropologist.

Another contribution, this time on a theoretical level, may be expected from this cooperation. The concept of isolate may help to solve a problem in social structure which has given rise to a controversy between Radcliffe-Brown and Lowie. The former

has labeled as "a fantastic reification of abstraction" the suggestion made by some anthropologists, mostly in America, that anthropology should be defined as the study not of society but of culture. To him, "European culture is an abstraction and so is the culture of an African tribe." All that exists are human beings connected by an unlimited series of social relations.<sup>42</sup> This, Lowie says, is "a factitious quarrel."<sup>43</sup> However, the misunderstandings which lie at its root appear to be very real, since they arose all over again on the occasion of the publication of a book by White<sup>44</sup> and its criticism by Bidney.<sup>45</sup>

It seems that both the reality and the autonomy of the concept of culture could better be validated if culture were treated, from an operational point of view, in the same way as the geneticist and demographer treat the closely allied concept of "isolate." What is called a "culture" is a fragment of humanity which, from the point of view of the research at hand and of the scale on which the latter is carried out, presents significant discontinuities in relation to the rest of humanity. If our aim is to ascertain significant discontinuities between, let us say, North America and Europe, then we are dealing with two different cultures; but should we become concerned with significant discontinuities between New York and Chicago, we would be allowed to speak of these two groups as different cultural "units." Since these discontinuities can be reduced to *invariants*, which is the goal of structural analysis, we see that culture may, at the same time, correspond to an objective reality and be a function of the kind of research undertaken. Accordingly, the same set of individuals may be considered to be parts of many different cultural contexts: universal, continental, national, regional, local, etc., as well as familial, occupational, religious, political, etc. This is true as a limit; however, anthropologists usually reserve the term "culture" to designate a *group* of discontinuities which is significant on several of these levels at the same time. That it can never be valid for all levels does not prevent the concept of "culture" from being as fundamental for the anthropologist as that of "isolate" for the demographer. Both belong to the same epistemological family. On a question such as that of the positivistic character of a concept, the anthropologist can rely on a physicist's judgment; it is Niels Bohr

who states that "the traditional differences of [human cultures] in many ways resemble the different equivalent modes in which physical experience can be described."<sup>46</sup>

#### SOCIAL STATICS OR COMMUNICATION STRUCTURES

A society consists of individuals and groups which communicate with one another. The existence of, or lack of, communication can never be defined in an absolute manner. Communication does not cease at society's borders. These borders, rather, constitute thresholds where the rate and forms of communication, without waning altogether, reach a much lower level. This condition is usually meaningful enough for the population, both inside and outside the borders, to become aware of it. This awareness is not, however, a prerequisite for the definition of a given society. It only accompanies the more precise and stable forms.

In any society, communication operates on three different levels: communication of women, communication of goods and services, communication of messages. Therefore, kinship studies, economics, and linguistics approach the same kinds of problems on different strategic levels and really pertain to the same field. Theoretically at least, it might be said that kinship and marriage rules regulate a fourth type of communication, that of genes between phenotypes. Therefore, it should be kept in mind that culture does not consist exclusively of forms of communication of its own, like language, but also (and perhaps mostly) of *rules* stating how the "games of communication" should be played both on the natural and on the cultural levels.

The above comparison between the fields of kinship, economics, and linguistics cannot hide the fact that they refer to forms of communication which are on a different scale. Should one try to compute the communication rate involved, on the one hand, in the intermarriages and, on the other, in the exchange of messages occurring in a given society, one would probably discover the difference to be of about the same magnitude as, let us say, that between the exchange of heavy molecules of two viscous liquids through a not very permeable film and radio communication. Thus, from marriage to language one passes from low- to

high-speed communication; this arises from the fact that what is communicated in marriage is almost of the same nature as those who communicate (women, on the one hand, men, on the other), while speakers of language are not of the same nature as their utterances. The opposition is thus one of *person* to *symbol*, or of *value* to *sign*. This helps to clarify the somewhat intermediate position of economics between these two extremes—goods and services are not persons, but they still are values. And, though neither symbols nor signs, they require symbols or signs in order to be successfully exchanged when the exchange system reaches a certain degree of complexity.

From this outline of the structure of social communication three important sets of considerations follow.

First, the position of economics in social structure can be precisely defined. Economics in the past has been suspect among anthropologists. Even in this symposium, no paper was explicitly assigned to economic problems. Yet, whenever this highly important topic has been broached, a close relationship has been shown to prevail between economic pattern and social structure. Since Mauss's pioneer papers<sup>47</sup> and Malinowski's book on the *kula*<sup>48</sup>—by far his masterpiece—every attempt in this direction has shown that the economic system provides sociological formulations with some of their more fundamental invariants.<sup>49</sup>

The anthropologist's reluctance originated in the condition of economic studies themselves; these were ridden with conflicts between bitterly opposed schools and at the same time bathed in an aura of mystery and conceit. Thus the anthropologist labored under the impression that economics dealt mostly with abstractions and that there was little connection between the actual life of actual groups of people and such notions as value, utility, profit, and the like.

The complete upheaval of economic studies resulting from the publication of Von Neumann and Morgenstern's book<sup>50</sup> ushers in an era of closer cooperation between the economist and the anthropologist, and for two reasons. First—though economics achieves here a rigorous approach—this book deals not with abstractions such as those just mentioned but with concrete individuals and groups which are represented in their actual and empirical relations of cooperation and competition. Surprising though the

parallel may seem, this formalism converges with certain aspects of Marxian thought.<sup>51</sup>

Next—and as a consequence—it introduces for the first time mechanical models which are of the same type as, and intermediate between, those used in mathematical physics and in social anthropology—especially in the field of kinship. In this connection it is striking that Von Neumann's models are borrowed from the theory of games, a line of thought which was initiated independently by Kroeber when he compared social institutions "to the play of earnest children."<sup>52</sup> There is, true enough, an important difference between games of entertainment and marriage rules: The former are constructed in such a way as to permit each player to extract from statistical regularities maximal differential values, while marriage rules, acting in the opposite direction, aim at establishing statistical regularities in spite of the differential values existing between individuals and generations. In this sense they constitute a special kind of "upturned game." Nevertheless, they can be treated with the same methods. Besides, such being the rules, each individual and group tries to play it in the "normal" way, that is, by maximizing his own advantage at the expense of the others (i.e., to get more wives, or better ones, whether from the esthetic, erotic, or economic point of view). The theory of courtship is thus a part of formal sociology. To those who are afraid that sociology might in this way get hopelessly involved in individual psychology, it will be enough to recall that Von Neumann has succeeded in giving a mathematical demonstration of the nature and strategy of a psychological technique as sophisticated as bluffing at the game of poker.<sup>53</sup>

The next advantage of this increasing consolidation of social anthropology, economics, and linguistics into one great field, that of communication, is to make clear that they consist exclusively of the study of *rules* and have little concern with the nature of the partners (either individuals or groups) whose play is being patterned after these rules. As Von Neumann puts it, "The game is simply the totality of the rules which describe it."<sup>54</sup> Besides that of game, other operational notions are those of play, move, choice, and strategy.<sup>55</sup> But the nature of the players need not be considered. What is important is to find out when a given player can make a choice and when he cannot.

This outlook should open the study of kinship and marriage to approaches directly derived from the theory of communication. In the terminology of this theory it is possible to speak of the information of a marriage system by the number of choices at the observer's disposal to define the marriage status of an individual. Thus the information is unity for a dual exogamous system, and, in an Australian kind of kinship typology, it would increase with the logarithm of the number of matrimonial classes. A theoretical system where everybody could marry everybody would be a system with no redundancy, since each marriage choice would not be determined by previous choices, while the positive content of marriage rules constitutes the redundancy of the system under consideration. By studying the percentage of "free" choices in a matrimonial population (not absolutely free, but in relation to certain postulated conditions), it would thus become possible to offer numerical estimates of its entropy, both absolute and relative.

As a consequence, it would become possible to translate statistical models into mechanical ones and vice versa, thus bridging the gap still existing between population studies on the one hand and anthropological ones on the other, thereby laying a foundation for prediction and control. To give an example: In our own society the organization of marriage choices does not go beyond (1) the prohibition of close kin, (2) the size of the isolate, and (3) the accepted standard of behavior, which limits the frequency of certain choices within the isolate. With these data at hand, one could compute the information of the system, that is, translate our loosely organized and highly statistical marriage system into a mechanical model, thus making possible its comparison with the large series of marriage systems of a "mechanical" type available from simpler societies.

Similarly, a great deal of discussion has been carried on recently about the Murngin kinship system, which has been treated by different authors as a seven-class system, or less than seven, or four, or thirty-two, or three,<sup>56</sup> before recent research resolved the question in favor of the last number.<sup>57</sup>

In the preceding pages an attempt has been made to assess the bearing of some recent lines of mathematical research upon an-

thropological studies. We have seen that their main contribution was to provide anthropology with a unifying concept—communication—enabling it to consolidate widely different types of inquiry into one, and at the same time providing the theoretical and methodological tools to further knowledge in that direction. The question which should now be raised is: To what extent is social anthropology ready to make use of these tools?

The main feature of the development of social anthropology in the past years has been the increased attention to kinship. This is, indeed, not a new phenomenon, since it can be said that, with his *Systems of Consanguinity and Affinity of the Human Family*, Lewis Morgan's genius at one and the same time founded social anthropology and kinship studies and brought to the fore the basic reasons for attaching such importance to the latter: permanency, systematic character, continuity of changes.<sup>58</sup> The views outlined in the preceding pages may help to explain this fundamental interest in kinship, since we have considered it as the anthropologist's special and privileged share in the science of communication.

Unfortunately, despite the enormous development of kinship studies in recent years, the amount of usable material in relation to that actually collected remains small. This is clearly reflected in the fact that, in order to undertake his survey, Murdock found it possible to retain information concerning no more than about 250 societies (from our point of view, a still overindulgent estimate) out of the 3,000 to 4,000 distinct societies still in existence.<sup>59</sup> It is somewhat disheartening that the enormous work devoted in the last fifty years to the gathering of ethnographic material has yielded so little, although kinship has been one of the main concerns of those undertaking this work.

However, it should be kept in mind that what has brought about this unhappy result is not a lack of coverage—on the contrary. If the workable material is small, it is rather on account of the inductive illusion; it was believed that as many cultures as possible should be covered, albeit lightly, rather than a few thoroughly enough to yield significant results. Accordingly, there is no lack of consistency in the fact that, following their individual temperaments, anthropologists have preferred one or the other of the alternatives imposed by the situation. While Radcliffe-Brown, Eggen, Spoehr, Fortes, and this writer have tried to consider limited

areas where dense information was available, Murdock has followed the complementary (but not contradictory) path of widening the field even at the expense of the reliability of the data, and Lowie<sup>60</sup> has tried to pursue a kind of middle road between the two approaches.

The case of the Pueblo area is especially striking, since for probably no other area in the world is there available such an amount of data of such controversial quality. It is almost with despair that one comes to realize that the voluminous material accumulated by Voth, Fewkes, Dorsey, Parsons, and, to some extent, Stevenson is practically unworkable, since these authors have been feverishly piling up information without any clear idea of what it meant or, above all, of the hypotheses which it should have helped to test. The situation changed when Lowie and Kroeber entered the field, but the lack of statistical data on marriage choices and types of intermarriages, which might have been gathered for more than fifty years, will probably be impossible to overcome. This is to be regretted, since Eggen's book<sup>61</sup> presents an outstanding example of what can be expected from intensive and thorough study of a limited area. Here we observe closely connected forms, each of which preserves a structural consistency, although they present, in relation to one another, discontinuities which become significant when compared to homologous discontinuities in other fields, such as clan organization, marriage rules, ritual, religious beliefs, etc.

It is by means of such studies, which exhibit a truly "Galilean" outlook,<sup>62</sup> that one may hope to reach a depth where social structure is put on a level with other types of mental structures, particularly the linguistic one. To give an example: It follows from Eggen's survey that the Hopi kinship system requires no less than three different models for the time dimension. There is, first, an "empty" time, stable and reversible, illustrated by the father's mother's and mother's father's lineages, where the same terms are consistently applied throughout the generations; second, there is a progressive, non-reversible time, as shown in (female) Ego's lineage with the sequence:

grandmother > mother > sister > child > grandchild;

and, third, there is an undulating, cyclical, reversible time, as in (male) Ego's lineage with the continuous alternation between sister



and sister's child. On the other hand, these three linear structures are clearly distinct from the circular structure of the Zuni (female) Ego's lineage, where three terms, mother's mother (or daughter's daughter), mother, and daughter, are disposed in a kind of ringlike arrangement, this conceptual grouping being accompanied, as regards the other lineages, by a greater poverty both of terms inside the acknowledged kin and of kin acknowledgment. Since time aspects also belong to linguistic analysis, the question can be raised whether or not there is a correlation between their manifestations in language and kinship and, if so, at what level.<sup>63</sup>

Progress in this and other directions would undoubtedly have been more substantial if general agreement had existed among social anthropologists on the definition of social structure, the goals which may be achieved by its study, and the methodological principles to be applied at the different stages of research. Unfortunately, this is not the case, but it may be welcomed as a promising sign that some kind of understanding can be reached, at least on the nature and scope of these differences. This seems an appropriate place to offer a rapid sketch of the attitude of the main contributors to social-structure research in relation to the working assumptions which were made at the beginning of this paper.

The term "social structure" is in many ways linked with the name of A. R. Radcliffe-Brown. Though his contribution is not limited to the study of kinship systems, he has stated the goal of these studies in terms which every scholar in the same field would probably be ready to underwrite. The aim of kinship studies, he says, is (1) to make a systematic classification; (2) to understand particular features of particular systems (a) by revealing the particular feature as a part of an organized whole, and (b) by showing that it is a special example of a recognizable class of phenomena; (3) to arrive at valid generalizations about the nature of human societies. And he concludes: "To reduce this diversity (of 2 or 300 kinship systems) to some sort of order is the task of analysis. . . . We can . . . find . . . beneath the diversities, a limited number of general principles applied and combined in various ways."<sup>64</sup> There is nothing to add to this lucid program besides pointing out that this is precisely what Radcliffe-Brown has done in his study of Australian kinship systems. He brought forth a tre-

mendous amount of material; he introduced some kind of order where there was only chaos; he defined the basic operational terms, such as "cycle," "pair," and "couple." Finally, his discovery of the Kariera system in the region, with the characteristics inferred from the study of the available data previous to visiting Australia, will forever remain one of the great results of socio-structural studies.<sup>65</sup> His masterly introduction to *African Systems of Kinship and Marriage* may be considered a true treatise on kinship; at the same time it takes a step toward integrating kinship systems of the Western world (which are considered in their early forms) into a worldwide theoretical interpretation. Another capital contribution by the same scholar, about the homologous structure of kinship terminology and behavior, will be dealt with later on.

However, it is obvious that, in many respects, Radcliffe-Brown's conception of social structure differs from the postulates which were set up at the outset of the present paper. In the first place, the notion of structure appears to him as a means to link social anthropology to the biological sciences: "There is a real and significant analogy between organic structure and social structure."<sup>66</sup> Then, instead of "lifting up" kinship studies to put them on the same level as communication theory, as has been suggested by this writer, he has lowered them to the same plane as the phenomena dealt with in descriptive morphology and physiology.<sup>67</sup> In that respect, his approach is in line with the naturalistic trend of the British school. In contradistinction to Kroeber<sup>68</sup> and Lowie,<sup>69</sup> who have emphasized the artificiality of kinship, Radcliffe-Brown agrees with Malinowski that biological ties are, at one and the same time, the origin of and the model for every type of kinship tie.<sup>70</sup>

These principles are responsible for two consequences. In the first place, Radcliffe-Brown's empirical approach makes him very reluctant to distinguish between *social structure* and *social relations*. As a matter of fact, social structure appears in his work to be nothing else than the whole network of social relations. It is true that he has sometimes outlined a distinction between *structure* and *structural form*. The latter concept, however, seems to be limited to the diachronic perspective, and its functional role in Radcliffe-Brown's theoretical thought appears quite reduced.<sup>71</sup> This distinction was thoroughly discussed by Fortes, who has contributed a great deal to the distinction, quite foreign to Radcliffe-Brown's



outlook (and to which I myself attribute considerable importance), between "model" and "reality": "Structure is not immediately visible in the 'concrete reality.' . . . When we describe structure . . . we are, as it were, in the realm of grammar and syntax, not of the spoken word."<sup>72</sup>

In the second place, this merging of social structure and social relations induces him to break down the former into the simplest forms of the latter, that is, relations between two persons: "The kinship structure of any society consists of a number of . . . dyadic relations. . . . In an Australian tribe, the whole social structure is based on a network of such relations of person to person. . . ." <sup>73</sup> It may be questioned whether such dyadic relations are the materials out of which social structure is built, or whether they do not themselves result from a pre-existing structure which should be defined in more complex terms. Structural linguistics has a lot to teach in this respect. Examples of the kind of analysis commended by Radcliffe-Brown may be found in the works of Bateson and Mead. However, in *Naven*,<sup>74</sup> Bateson has gone a step further than Radcliffe-Brown's classification<sup>75</sup> of dyadic relations according to order. He has attempted to place them in specific categories, an undertaking which implies that there is something more to social structure than the dyadic relations, that is, the structure itself.

Since it is possible to extend almost indefinitely the string of dyadic relations, Radcliffe-Brown has shown some reluctance toward the isolation of social structures conceived as self-sufficient wholes (in this respect he disagrees with Malinowski). His is a philosophy of continuity, not of discontinuity; this accounts for his hostility toward the notion of culture, already alluded to, and his avoidance of the teachings of structural linguistics and of modern mathematics.

All these considerations may explain why Radcliffe-Brown, though an incomparable observer, analyst, and classifier, has sometimes proved to be disappointing when he turned to interpretations. These, in his work, often appear vague or circular. Have marriage prohibitions really no other function than to help perpetuate the kinship system?<sup>76</sup> Are all the peculiar features of the Crow-Omaha system satisfactorily accounted for when it has been said that they emphasize the lineage principle?<sup>77</sup> These doubts, as

well as many others, some of which will be mentioned later on in this chapter, explain why the work of Radcliffe-Brown, to which no one can deny a central place in social-structure studies, has often given rise to bitter arguments.

For instance, Murdock has called the kind of interpretation to which Radcliffe-Brown seems to be addicted "mere verbalizations reified into causal forces,"<sup>78</sup> and Lowie expressed himself in similar terms.<sup>79</sup> As regards Murdock, the lively controversy which was carried on between him and W. E. Lawrence,<sup>80</sup> on the one hand, and Radcliffe-Brown,<sup>81</sup> on the other, may help to clarify the basic differences in their respective positions. This was about the so-called Murngin type of kinship system, a focal point in social-structure studies not only because of its many intricacies but because, thanks to Lloyd Warner's book and articles,<sup>82</sup> we possess a thorough and extensive study of this system.<sup>83</sup> However, Warner's study leaves some basic problems unanswered, especially the way in which marriage takes place on the lateral borders of the system.

For Radcliffe-Brown, however, there is no problem involved, since he considers any kind of social organization as a mere conglomerate of simple person-to-person relations and since, in any society, there is always somebody who may be regarded as one's mother's brother's daughter (the preferred spouse among the Murngin) or as standing in an equivalent relation. But the problem is elsewhere: It lies in the fact that the natives have chosen to express these person-to-person relations in a class system, and Warner's description of this system (as acknowledged by himself) makes it impossible in some cases for the same individual to belong simultaneously to the right kind of class and to the right kind of relation.

Under these circumstances, Lawrence and Murdock have tried to invent some system which would fit the requirements of both the marriage rules and a system of the same kind as the one described by Warner. They invented it, however, as a sort of abstract game, the result being that, while their system meets some of the difficulties involved in Warner's account, it also raises many others. One of the main difficulties implied in Warner's system is that it would require, on the part of the natives, an awareness of relationships too remote to make it believable. Since the new system adds a new line to the seven already assumed by Warner, it

goes still further in that direction. Therefore, it seems a good hunch that the "hidden" or "unknown" system underlying the clumsy model which the Murngin borrowed recently from tribes with completely different marriage rules is simpler than the latter and not more complicated.<sup>84</sup>

One sees, then, that Murdock favors a systematic and formal approach, different from Radcliffe-Brown's empirical and naturalistic one. But he remains, at the same time, psychologically and even biologically minded, and he can comply with the resulting requirements only by calling upon other disciplines, such as psychoanalysis and behavioristic psychology. Thus he succeeds in unloading from his interpretations of kinship problems the empiricism which still burdens Radcliffe-Brown's work, though, perhaps, at the risk of leaving them incomplete or having to be completed on grounds alien to anthropology, if not contradictory to its goals. Instead of seeing in kinship systems a sociological means to achieve a sociological result, he rather treats them as sociological results deriving from biological and psychological premises.<sup>85</sup>

Two parts should be distinguished in Murdock's contribution to the study of social structure. There is, first, a rejuvenation of a statistical method to test assumed correlations between social traits and to establish new ones, a method already tried by Tylor but which Murdock, thanks to the painstaking efforts of his Yale Cross-Cultural Survey and the use of a more complex and exacting technique, was able to carry much further than had his predecessor.

Everything has been said on the manifold difficulties with which this kind of inquiry is fraught,<sup>86</sup> and since no one is better aware of them than its author, it is unnecessary to dwell upon this theme. Let it only be recalled that while the uncertainty involved in the process of "carving out" the data will always make any alleged correlation dubious, the method is quite efficient in a negative way, that is, in exploding false correlations. In this respect Murdock has achieved many results which no social anthropologist can permit himself to ignore.

The second aspect of Murdock's contribution is a scheme of the historical evolution of kinship systems. This suggests a startling conclusion, namely, that the so-called "Hawaiian type" of social organization should be placed at the origin of a much greater

number of systems than has generally been admitted since Lowie's criticism of Morgan's similar hypothesis.<sup>87</sup> However, it should be kept in mind that Murdock's scheme is not based upon the consideration of individual societies taken as historico-geographical units or as coordinated wholes, but on abstractions and even, if one may say so, on abstractions "twice removed": In the first place, social organization is isolated from the other aspects of culture (and sometimes even kinship systems from social organization); next, social organization itself is broken up into disconnected elements which are the product of the traditional categories of ethnological theory rather than of the concrete analysis of each group. This being understood, the method for establishing a historical scheme can only be ideological; it proceeds by extracting common elements pertaining to each stage, in order to define a previous stage, and so on. Therefore, it is obvious that systems placed at the beginning can be only those which exhibit the more general features, while systems with special features must occupy a more recent rank. It is as though the origin of the modern horse were ascribed to the order of vertebrates instead of to *Hipparion*.

Regardless of the difficulties raised by his approach, Murdock's book should be credited with presenting new material and raising fascinating problems, many of which are new to anthropological thought. It is not doing him an injustice, then, to state that his contribution consists more in perfecting a method of discovering new problems than in solving them. Though this method remains "Aristotelian," it is perhaps unavoidable in the development of any science. Murdock has at least been faithful to the best part of the Aristotelian outlook by demonstrating convincingly that "cultural forms in the field of social organization reveal a degree of regularity and of conformity to scientific law not significantly inferior to that found in the so-called natural sciences."<sup>88</sup>

In relation to the distinctions made in the first section of this paper, it can be said that Radcliffe-Brown's work expresses a disregard for the difference between *observation* and *experimentation*, while Murdock shows a similar disregard for the difference between *mechanical* and *statistical* models (since he tries to construct mechanical models with the help of a statistical method). Conversely, Lowie's work seems to consist entirely in an exacting endeavor to meet the question (which was acknowledged as a

prerequisite for any study in social structure): *What are the facts?* When he became active in research as well as in theoretical ethnology, the latter field was fraught with philosophical prejudices and an aura of sociological mysticism; therefore, his paramount contribution toward assessing the subject matter of social anthropology has sometimes been misunderstood and thought of as wholly negative.<sup>89</sup> But, although this situation made it imperative at that time to state, in the first place, what the facts were *not*, the creative energy liberated by his merciless destruction of arbitrary systems and alleged correlations has furnished, to a very large extent, the power used by his followers. His own positive contributions are not always easy to outline on account of the extreme modesty of his thought and his aversion to any kind of wide-scope theoretical claim. He himself used the words "active skepticism" to define his position. However, it is Lowie who, as early as 1915, stated in modern terms the role of kinship studies in relation to social behavior and organization: "Sometimes the very essence of social fabric may be demonstrably connected with the mode of classifying kin."<sup>90</sup> In the same paper he was able to reverse the narrow historical trend which, at that time, was blinding anthropological thinking to the universal action of structural forces: Exogamy was shown to be a scheme defined by truly genetic characteristics and, whenever present, determining identical features of social organization, without calling for historico-geographical relations.

When, a few years later, he exploded the "matrilineal complex,"<sup>91</sup> he achieved two results which are the fundamentals of social-structure studies. First, by dismissing the notion that every so-called matrilineal feature was to be understood as an expression or as a vestige of the complex, he made it possible to break it up into several variables. Second, the elements thus liberated could be used for a permutative treatment of the differential features of kinship systems.<sup>92</sup> Thus he was laying the foundations for a structural analysis of kinship on two different levels: that of the terminological system, on the one hand, and, on the other, that of the correlation between the system of attitudes and terminology, thus revealing which later on was to be followed by others.<sup>93</sup>

Lowie should be credited with many other theoretical contributions. He was probably the first to demonstrate the true bi-

lateral nature of most of the so-called "unilineal" systems.<sup>94</sup> He made clear the impact of residence on descent.<sup>95</sup> He convincingly dissociated avoidance customs from incest prohibitions.<sup>96</sup> His care to interpret social organization not only as a set of institutionalized rules but also as the outcome of individual psychological reactions, which sometimes contradicted or inflected the rules, led to the strange result that the same scholar who was so abused for his famous "shreds and patches" statement on culture was able to offer some of the most thorough and well-balanced pictures we have of cultures treated as wholes.<sup>97</sup> Finally, Lowie's role as a promoter and exponent of South American social anthropology is well known; either directly or indirectly, through guidance and encouragement, he has contributed toward breaking new ground.

#### SOCIAL DYNAMICS: SUBORDINATION STRUCTURES

*Order of Elements (Individuals or Groups) in the Social Structure.* According to this writer's interpretation, which does not need to be expounded systematically since (in spite of efforts toward objectivity) it probably permeates this paper, kinship systems, marriage rules, and descent groups constitute a coordinated whole, the function of which is to insure the permanency of the social group by means of intertwining consanguineous and affinal ties. They may be considered as the blueprint of a mechanism which "pumps" women out of their consanguineous families to redistribute them in affinal groups, the result of this process being to create new consanguineous groups, and so on.<sup>98</sup>

If no external factor were affecting this mechanism, it would work indefinitely, and the social structure would remain static. This is not the case, however; hence the need to introduce into the theoretical model new elements to account for the diachronic changes of the structure, on the one hand, and, on the other, for the fact that kinship structure does not exhaust social structure. This can be done in three different ways.

As always, the first step consists in ascertaining the facts. Since the time when Lowie expressed regret that so little had been done by anthropologists in the field of political organization,<sup>99</sup>

some progress has been made; in the first place, Lowie himself has clarified the issue by devoting most of his recent book to problems of that sort and by regrouping the facts concerning the North American area.<sup>100</sup> A recent work has brought together significant data concerning Africa.<sup>101</sup> To this day, the best way to organize the still-confused material remains Lowie's basic distinctions among social strata, sodalities, and the state.<sup>102</sup>

The second type of approach would be an attempt to correlate the phenomena belonging to the order first studied, that is, kinship, with phenomena belonging to the new order but showing a direct connection with the former. This approach raises, in turn, two different problems: (1) Can the kinship structure by itself result in structures of a new type (that is, dynamically oriented)? (2) How do *communication structures* and *subordination structures* interact with one another?

The first problem should be related to education, i.e., to the fact that each generation plays alternately a submissive and a dominant part in relation to the preceding and to the following generation. This aspect has been dealt with chiefly by Margaret Mead.<sup>103</sup>

Another side of the question lies in the important attempt to correlate static positions in the kinship structure (as defined by terminology) with dynamic attitudes expressed, on the one hand, in rights, duties, obligations and, on the other, in privileges, avoidance, etc. It is impossible to go into the discussion of these problems, to which many writers have contributed. Especially significant is a protracted controversy between Radcliffe-Brown and others about the kind of correlation, if any, which exists between the *system of terminology* and the *system of attitudes*.<sup>104</sup>

According to Radcliffe-Brown's well-known position, such a correlation exhibits a high degree of accuracy, while his opponents have generally tried to demonstrate that it is neither absolute nor detailed. In contrast to both opinions, this writer has tried to establish that the relation between terminology and attitudes is of a dialectical nature. The modalities of behavior between relatives express to some extent the terminological classification, and they provide at the same time a means of overcoming difficulties and contradictions resulting from this classification. Thus the rules of behavior result from an attempt to overcome contradictions in the field of terminology and marriage rules; the functional unwedging

—if one may call it that—which is bound to exist between the two orders causes changes in terminology; and these, in turn, call for new behavior patterns, and so on indefinitely.<sup>105</sup>

The second problem confronts us with the kind of situation arising when the kinship system regulates marriage exchanges not between equals but between members of a hierarchy (either economic or political). Under that heading comes the problem of polygamy which, in some cases at least, may be shown to provide a bridge between two different types of guarantees, one collective and political, the other individual and economic,<sup>106</sup> and that of hypergamy (or hypogamy). This deserves much more attention than it has received thus far, since it is the doorway to the study of the caste system<sup>107</sup> and hence to that of social structures based on race and class distinctions.

The third and last approach to our problem is purely formal. It consists in an a priori deduction of the types of structures likely to result from relations of dominance or dependency as they might appear at random. Of a very promising nature for the study of social structure are Rapoport's attempts to formulate a mathematical theory of the pecking order among hens.<sup>108</sup> It is true that there seems to be a complete opposition between, let us say, the pecking order of hens, which is intransitive and cyclical, and the social order (for instance, the circle of *kava* in Polynesia), which is transitive and non-cyclical (since those who are seated at the far end can never sit at the top).<sup>109</sup> But the study of kinship systems shows precisely that, under given circumstances, a transitive and non-cyclical order can result in an intransitive and cyclical one. This happens, for instance, in a hypergamous society, where a circular marriage system with mother's brother's daughter leaves at one end a girl unable to find a husband (since her status is the highest) and at the other end a boy without a wife (since no girl, except his sister, has a status lower than his own). Therefore, either the society under consideration will succumb to its contradictions, or its transitive and non-cyclical order will be transformed into an intransitive and cyclical one, temporarily or locally.<sup>110</sup>

Thus, with the help of such notions as transitivity, order, and cycle, which admit of mathematical treatment, it becomes possible to study, on a purely formal level, generalized types of social

structure where both the communication and the subordination aspects are fully integrated. It is also possible to enlarge the field of inquiry and to integrate, for a given society, actual and potential types of order. For instance, in human societies the actual forms of social order are practically always of a transitive and non-cyclical type: If A is above B and B above C, then A is above C; and C cannot be above A. But most of the human "potential" or "ideological" forms of social order, as illustrated in politics, myth, and religion, are conceived as intransitive and cyclical; for instance, in tales about kings marrying lasses and in Stendhal's indictment of American democracy as a system where a gentleman takes his orders from his grocer.

*Order of Orders.* Thus anthropology considers the whole social fabric as a network of different types of orders. The kinship system provides a way to order individuals according to certain rules; social organization is another way of ordering individuals and groups; social stratifications, whether economic or political, provide us with a third type; and all these orders can themselves be ordered by showing the kind of relationships which exist among them, how they interact with one another on both the synchronic and the diachronic levels. Meyer Fortes has successfully tried to construct models valid not only for one type of order (kinship, social organization, economic relations, etc.) but where numerous models for all types of orders are themselves ordered inside a total model.<sup>111</sup>

When dealing with these orders, however, anthropologists are confronted with a basic problem which was taken up at the beginning of this paper, that is, to what extent does the manner according to which a society conceives its orders and their ordering correspond to the real situation? It has been shown that this problem can be solved in different ways, depending on the data at hand.

All the models considered so far, however, are "lived-in" orders: they correspond to mechanisms which can be studied from the outside as a part of objective reality. But no systematic studies of these orders can be undertaken without acknowledging the fact that social groups, to achieve their reciprocal ordering, need to call upon orders of different types, corresponding to a field external to

objective reality and which we call the "supernatural." These "thought-of" orders cannot be checked against the experience to which they refer, since they are one and the same as this experience. Therefore, we are in the position of studying them only in their relationships with the other types of "lived-in" orders. The "thought-of" orders are those of myth and religion. The question may be raised whether, in our own society, political ideology does not belong to the same category.

After Durkheim, Radcliffe-Brown has contributed greatly to the demonstration that religion is a part of the social structure. The anthropologist's task is to discover correlations between different types of religions and different types of social organization.<sup>112</sup> Radcliffe-Brown failed to achieve significant results, however, for two reasons. In the first place, he tried to link ritual and beliefs directly to sentiments; besides, he was more concerned with giving universal formulation to the kind of correlation prevailing between religion and social structure than in showing the variability of one in relation to the other. It is perhaps as a result of this that the study of religion has fallen into the background, to the extent that the word "religion" does not even appear in the program of this symposium. The field of myth, ritual, and religion seems nevertheless to be one of the more fruitful for the study of social structure; though relatively little has been done in this respect, the results which have been obtained recently are among the most rewarding in our field.

Great strides have been made toward the study of religious systems as coordinated wholes. Documentary material, such as P. Radin's *The Road of Life and Death*<sup>113</sup> and R. M. Berndt's *Kunapipi*,<sup>114</sup> should help in undertaking, with respect to several religious cults, the kind of ordering of data so masterfully achieved by Gladys Reichard for the Navaho.<sup>115</sup> This should be complemented by small-scale comparative studies on the permanent and non-permanent elements in religious thought as exemplified by Lowie.

With the help of such well-organized material it becomes possible, as Nadel puts it, to prepare "small-scale models of a comparative analysis . . . of an analysis of 'concomitant variations' . . . such as any inquiry concerned with the explanation of social facts must employ."<sup>116</sup> The results thus achieved may be small; they are,



however, some of the most convincing and rigorous in the entire field of social organization. Nadel himself has demonstrated a correlation between shamanism and some aspects of psychological development;<sup>117</sup> using Indo-European comparative material borrowed from Iceland, Ireland, and the Caucasus, Dumézil has interpreted an enigmatic mythological figure in relation to specific features of social organization;<sup>118</sup> Wittfogel and Goldfrank have shown how significant variations in mythological themes can be related to the socioeconomic background.<sup>119</sup> Monica Hunter has established beyond doubt that the structure of magical beliefs may vary in correlation with the structure of the society itself.<sup>120</sup> These results, together with some others (on which space prevents our commenting), give hope that we may be close to understanding not only what kind of function religious beliefs fulfill in social life (this has been known more or less clearly since Lucretius' time) but how they fulfill this function.

A few words may be added as a conclusion. This chapter was started by working out the notion of "model," and the same notion has reappeared at its end. Social anthropology, in its incipient stage, could only seek, as model for its first models, among those of the simplest kinds provided by more advanced sciences, and it was natural enough to seek them in the field of classical mechanics. However, in doing so, anthropology has been working under some sort of illusion, since, as Von Neumann puts it, "an almost exact theory of a gas, containing about  $10^{25}$  freely moving particles, is incomparably easier than that of the solar system, made up of 9 major bodies."<sup>121</sup> But when it tries to construct its models, anthropology finds itself in a situation which is neither the one nor the other: The objects with which we deal—social roles and human beings—are considerably more numerous than those dealt with in Newtonian mechanics, and at the same time, far less numerous than would be required to allow a satisfactory use of the laws of statistics and probability. Thus we find ourselves in an intermediate zone: too complicated for one treatment and not complicated enough for the other.

The tremendous change brought about by the theory of communication consists precisely in the discovery of methods to deal with objects—signs—which can be subjected to a rigorous study

despite the fact that they are altogether much more numerous than those of classical mechanics and much less than those of thermodynamics. Language consists of morphemes, a few thousand in number; significant regularities in phoneme frequencies can be obtained by limited counts. The threshold for the use of statistical laws becomes lower, and that for operating with mechanical models higher, than was the case when operating on other grounds. And, at the same time, the size-order of the phenomena has become significantly closer to that of anthropological data.

Therefore, the present conditions of social-structure studies can be summarized as follows: Phenomena are found to be of the same kind as those which, in strategics and communication theory, were made the subject of a rigorous approach. Anthropological facts are on a scale which is sufficiently close to that of these other phenomena as not to preclude their similar treatment. Surprisingly enough, it is at the very moment when anthropology finds itself closer than ever to the long-awaited goal of becoming a true science that the ground seems to fail where it was expected to be the firmest: The facts themselves are lacking, either not numerous enough or not collected under conditions insuring their comparability.

Though it is not our fault, we have been behaving like amateur botanists, haphazardly picking up heterogeneous specimens, which were further distorted and mutilated by preservation in our herbarium. And we are, all of a sudden, confronted with the need of ordering complete series, ascertaining original shades, and measuring minute parts which have either shrunk or been lost. When we come to realize not only what should be done but also what we should be in a position to do, and when we make at the same time an inventory of our material, we cannot help feeling in a disheartened mood. It looks almost as if cosmic physics were asked to work with Babylonian observations. The celestial bodies are still there, but unfortunately the native cultures from which we used to gather our data are rapidly disappearing and that which they are being replaced by can only furnish data of a very different type. To adjust our techniques of observation to a theoretical framework which is far more advanced is a paradoxical situation, quite opposite to that which has prevailed in the history of sciences. Nevertheless, such is the challenge to modern anthropology.



## NOTES

1. A. L. Kroeber, *Anthropology* (New York: 1948), p. 325. Compare with the statement by the same author: ". . . the term 'social structure' which is tending to replace 'social organization' without appearing to add either content or emphasis of meaning." A. L. Kroeber, "Structure, Function and Pattern in Biology and Anthropology," *Scientific Monthly*, LVI (1943), p. 105.
2. The same idea appears to underlie E. R. Leach's remarkable study, "Jinghpaw Kinship Terminology," *Journal of the Royal Anthropological Institute*, LXXV (1945).
3. Compare Von Neumann: "Such models [as games] are theoretical constructs with a precise, exhaustive and not too complicated definition; and they must be similar to reality in those respects which are essential to the investigation at hand. To recapitulate in detail: The definition must be precise and exhaustive in order to make a mathematical treatment possible. The construct must not be unduly complicated so that the mathematical treatment can be brought beyond the mere formalism to the point where it yields complete numerical results. Similarity to reality is needed to make the operation significant. And this similarity must usually be restricted to a few traits deemed 'essential' *pro tempore*—since otherwise the above requirements would conflict with each other." J. Von Neumann and O. Morgenstern, *Theory of Games and Economic Behavior* (Princeton: 1944).
4. K. Goldstein, *Der Aufbau des Organismus*. French translation (Paris: 1951), pp. 18-25. [English translation, New York: 1939.]
5. F. Boas (ed.), *Handbook of American Indian Languages*, Bureau of American Ethnology Bulletin 40 (1908), 1911, Part I.
6. See "Introduction: History and Anthropology," Chapter I of the present volume.
7. For examples and detailed discussion, see C. Lévi-Strauss, *Les Structures élémentaires de la parenté* (Paris: 1949), p. 558 ff.
8. R. Firth, *Elements of Social Organization* (London: 1951), pp. 28-31.
9. On this point, see Chapters VII and VIII of the present volume.
10. J. Richardson and A. L. Kroeber, "Three Centuries of Women's Dress Fashions: A Quantitative Analysis," *Anthropological Records* (Berkeley, Calif.), V, No. 2 (1940).
11. Princeton, 1944.
12. Paris-Cambridge-New York, 1948.
13. Urbana, 1950.
14. D. Forde, "Marriage and the Family among the Yakö in S. E. Nigeria," *Monographs in Social Anthropology*, No. 5, London School of Economics (1941); V. Elwin, *The Muria and Their Ghotul* (Oxford: 1947).
15. Despite the criticism which has been leveled at me, I also maintain that the time dimension is irrelevant to the argument. For these discussions, see Chapter I of the present volume, already cited, and my *Race and History* (Paris: 1952). These studies have elicited criticisms and commentaries from C. Lefort, "L'Echange et la lutte des hommes," *Les Temps Modernes* (February, 1951); "Sociétés sans histoire et histori-

16. R. Firth, *op. cit.*, p. 40.
17. L. A. White, *The Science of Culture* (New York: 1949).
18. This is, indeed, how modern biological evolutionism is being developed in the researches of J. B. S. Haldane, G. G. Simpson, and others.
19. A. R. Radcliffe-Brown, "Social Anthropology, Past and Present," *Man*, LII (1952).
20. A. R. Radcliffe-Brown, "Religion and Society," Henry Myers Lecture, *Journal of the Royal Anthropological Institute*, LXXV (1945), p. 1.
21. R. H. Lowie, *op. cit.*, p. 38.
22. *Ibid.*, p. 68.
23. E. Durkheim, *Les Formes élémentaires de la vie religieuse* (Paris: 1912), p. 593.
24. K. Goldstein, *op. cit.*, p. 25.
25. E. E. Evans-Pritchard, "Nuer Time Reckoning," *Africa* (1939), XII, 189-216; *The Nuer* (Oxford: 1940).
26. L. Bernot and R. Blancard, "Nouvelle, un village français," *Travaux et mémoires de l'Institut d'Ethnologie*, LVII (1953).
27. E. Durkheim and M. Mauss, "De quelques Formes primitives de classification: Contribution à l'étude des représentations collectives," *Année sociologique*, VI (1903), pp. 1-72.
28. This opposition was never accepted by Lowie; see the preface to his *Primitive Society* (New York: 1920).
29. These studies were summarized by their author in *L'Héritage indo-européen à Rome* (Paris: 1949).
30. We refer to the chapter "Human Ecology," by M. Bates in *Anthropology Today*, pp. 700-13.
31. "Division et proportion des divisions de la sociologie," *Année sociologique*, n.s., II (1924-1925), p. 98 ff.
32. See, for example, R. Firth, *We, the Tikopia* (London-New York: 1936); J. Steward, *Basin-Plateau Aboriginal Sociopolitical Groups*, Bureau of American Ethnology, Smithsonian Institution Bulletin 120 (Washington: 1938); S. F. Nadel, *The Nuba* (London-New York: 1947); D. Forde, "Double-Descent among the Yakö," in A. R. Radcliffe-Brown and D. Forde (eds.), *African Systems of Kinship and Marriage* (London: 1950).
33. See Chapters VII and VIII of this volume.
34. See, for instance, the "configuration" of a ritual at its various stages, as they were mapped in A. C. Fletcher, *The Hako: A Pawnee Ceremony*, 22nd Annual Report, Bureau of American Ethnology, Vol. II (1904).
35. M. de Lestrangé, "Pour une Méthode socio-démographique," *Journal de la Société des Africanistes*, XXI (1951).
36. L. Livi, *Trattato di demografia* (Padua: 1940-1941); "Considérations théoriques et pratiques sur le concept de 'minimum de population,'" *Population*, IV, No. 4 (1949), pp. 754-56.

37. C. Wagley, "The Effects of Depopulation upon Social Organization as Illustrated by the Tapirapé Indians," *Transactions of the New York Academy of Sciences*, Series 2, III, No. 1 (1940), pp. 12-16.
38. See K. Davis, *The Development of the City in Society: Proceedings of the First Conference on Long Term Social Trends* (Washington, D.C.: Social Science Research Council, 1947); J. Q. Stewart, "Empirical Mathematical Rules Concerning the Distribution and Equilibrium of Population," *Geographical Review*, XXXVII, No. 3 (1947), 461-85; G. K. Zipf, *Human Behavior and the Principle of Least Effort* (Cambridge, Mass.: 1949). An expert on the theater told me recently that Louis Jouvet was always surprised that the house was filled approximately to capacity every night—that is to say, that a house with a capacity of 500 should have approximately 500 customers, and that one with a capacity of 2,000 should have about this number, with few people being turned away in the first case and the house never being three-quarters empty in the second case. This built-in balance would indeed be unexplainable, if all seats in each house were similar. But since the bad seats quickly acquire a bad reputation, a compensating effect occurs: If only poor seats remain, connoisseurs prefer to attend another performance or go to another theater. It would be interesting to investigate whether this phenomenon is of the same type as the rank-size law. From a more general perspective, the study of this theater phenomenon, considered from a quantitative viewpoint (relationship among the number of theaters, their respective size, the size of the cities, box office receipts, etc.) would provide a convenient and heretofore neglected method of clarifying—almost as in a laboratory—both diachronically and synchronically, certain fundamental problems of social structure.
39. G. Dahlberg, *Mathematical Methods for Population Genetics* (London-New York: 1948).
40. J. Sutter and L. Tabah, "Les Notions d'isolat et de population minimum," *Population*, VI, No. 3 (1951), pp. 481-89.
41. These two situations correspond respectively to marriages of the matrilineal type (long cycles) and patrilineal type (short cycles). On this topic, see my *Les Structures élémentaires de la parenté*, Chapter XXVII. It is evident from this example that considerations of a purely quantitative nature are not adequate. We must add to them the study of structures, which differ qualitatively.
42. A. R. Radcliffe-Brown, "On Social Structure," *Journal of the Royal Anthropological Institute*, LXX (1940), pp. 10-11.
43. R. H. Lowie, "A Marginal Note to Professor Radcliffe-Brown's Paper on 'Social Structure,'" *American Anthropologist*, XLIV, No. 3 (1942), pp. 520-21.
44. L. A. White, *op. cit.*
45. D. Bidney, review of L. A. White, *The Science of Culture*, in *American Anthropologist*, LII, No. 4, Part I (1950), pp. 518-19. See also A. R. Radcliffe-Brown, "White's View of a Science of Culture," *American Anthropologist*, LI, No. 3 (1949), pp. 503-12.
46. N. Bohr, "Natural Philosophy and Human Culture," *Nature*, CXLIII (1939), p. 9.
47. M. Mauss, "Essai sur les variations saisonnières dans les sociétés eskimos:

- Étude de morphologie sociale," *Année Sociologique*, IX, 1904-1905 (1906), pp. 39-132; "Essai sur le don, forme archaïque de l'échange," *op. cit.*, I (1923-1924), pp. 30-186.
48. B. Malinowski, *Argonauts of the Western Pacific* (London: 1922).
49. F. G. Speck, *Family Hunting Territories and Social Life of Various Algonkian Bands of the Ottawa Valley*, Canada Department of Mines, Geological Survey Memoir 70, Anthropological Series, No. 8 (Ottawa: 1915); A. I. Richards, *Hunger and Work in a Savage Tribe* (London: 1932), "A Dietary Study in Northeastern Rhodesia," *Africa*, IX, No. 2 (1936), pp. 166-96, and *Land, Labour and Diet in Northern Rhodesia* (Oxford: 1939); J. H. Steward, *Basin-Plateau Aboriginal Sociopolitical Groups*, *op. cit.*; E. E. Evans-Pritchard, *The Nuer* (Oxford: 1940); M. J. Herskovits, *The Economic Life of Primitive Peoples* (New York: 1940); K. A. Wittfogel and E. S. Goldfrank, "Some Aspects of Pueblo Mythology and Society," *Journal of American Folklore*, LVI (1943), pp. 17-30.
50. *Op. cit.*
51. This parallel did not appear in the original version of this paper, but it was suggested in the course of the discussion that followed. I subsequently developed it in an article, "The Mathematics of Man," which was the introduction to "Mathematics and the Social Sciences," a special issue of the *International Social Science Bulletin*, VI, No. 4 (UNESCO, Paris: 1954), pp. 581-90.
52. A. L. Kroeber, "The Societies of Primitive Man," *Biological Symposia*, VIII (1942), p. 215.
53. Von Neumann and Morgenstern, *op. cit.*, pp. 186-219.
54. *Ibid.*, p. 49.
55. We attempted to introduce these notions in another work. See *Race and History*, already mentioned.
56. W. L. Warner, "Morphology and Functions of the Australian Murngin Type of Kinship System," *American Anthropologist*, XXXII, No. 2 (1930), pp. 207-56, and XXXIII, No. 2 (1931), pp. 172-98; C. Lévi-Strauss, *Les Structures élémentaires de la parenté*; W. E. Lawrence and G. P. Murdock, "Murngin Social Organization," *American Anthropologist*, LI, No. 1 (1949), pp. 58-65; A. R. Radcliffe-Brown, "Murngin Social Organization," *American Anthropologist*, LIII, No. 1 (1951), pp. 37-55; A. P. Elkin, personal correspondence.
57. See pp. 305-6, *supra*, and notes 83-84.
58. Smithsonian Institution Contributions to Knowledge, XVII, No. 218 (Washington: 1871).
59. C. S. Ford and F. A. Beach, *Patterns of Sexual Behavior* (New York: 1951), p. 5.
60. R. H. Lowie, *Social Organization* (New York: 1948).
61. F. Eggan, *Social Organization of the Western Pueblos* (Chicago: 1950).
62. That is, aiming to uncover the law of variation, in contradistinction to the "Aristotelian" outlook, which is concerned primarily with inductive correlations; for this distinction, fundamental to structural analysis, see K. Lewin, *A Dynamic Theory of Personality* (New York: 1935).
63. See Chapters III and IV of the present volume, where this problem is treated more fully.

64. A. R. Radcliffe-Brown, "The Study of Kinship Systems," *Journal of the Royal Anthropological Institute*, LXXI (1941), p. 17.
65. A. R. Radcliffe-Brown, "The Social Organization of Australian Tribes," *Oceania*, I, No. 1 (1930-1931), pp. 34-63, No. 2, pp. 206-46, No. 3, pp. 322-41, and No. 4, pp. 426-56.
66. A. R. Radcliffe-Brown, "On Social Structure," *Journal of the Royal Anthropological Institute*, LXX (1940), p. 6.
67. *Ibid.*, p. 10.
68. A. L. Kroeber, "Basic and Secondary Patterns of Social Structure," *Journal of the Royal Anthropological Institute*, LXVIII (1938), pp. 299-309, and "The Societies of Primitive Man," *op. cit.*, p. 205 ff.
69. R. H. Lowie, *Social Organization*, Chapter IV.
70. A. R. Radcliffe-Brown, "Father, Mother, and Child," *Man*, XXVI, No. 103 (1926), pp. 159-61.
71. Radcliffe-Brown, "On Social Structure," *op. cit.*, p. 4.
72. M. Fortes (ed.), *Social Structure: Studies Presented to A. R. Radcliffe-Brown* (Oxford: 1949), p. 56.
73. Radcliffe-Brown, "On Social Structure," *op. cit.*, p. 3.
74. G. Bateson, *Naven* (Cambridge: 1936).
75. Radcliffe-Brown, "The Study of Kinship Systems," *op. cit.*
76. A. R. Radcliffe-Brown, "White's View of a Science of Culture," *American Anthropologist*, LI, No. 3 (1949), pp. 503-12.
77. Radcliffe-Brown, "The Study of Kinship Systems," *op. cit.*
78. G. P. Murdock, *op. cit.*, p. 121.
79. R. H. Lowie, *The History of Ethnological Theory* (New York: 1937), pp. 224-5.
80. Lawrence and Murdock, *op. cit.*
81. Radcliffe-Brown, "Murngin Social Organization," *op. cit.*
82. W. L. Warner, "Morphology and Functions of the Australian Murngin Type of Kinship System," *op. cit.*; *A Black Civilization: A Social Study of an Australian Tribe* (New York: 1937).
83. For a further word on this question, which appeared after this chapter was first published, see R. M. Berndt, "'Murngin,' (Wulamba) Social Organization," *American Anthropologist*, n.s., LVII, No. 1, Part I (1955).
84. Warner postulated a system with seven lines of descent corresponding to seven classes. Lawrence and Murdock substituted for it a system with eight lines of descent and thirty-two classes. At that time (see my *Les Structures élémentaires de la parenté*, Chapter XII) I suggested reducing Warner's scheme to four lines of descent one of which would be ambiguous. In 1951, E. R. Leach, a British anthropologist, adopted my interpretation, which he then undertook to defend against me while attributing to me another interpretation invented by him to further the argument. See E. R. Leach, "The Structural Implications of Matrilateral Cross-Cousin Marriage," *Journal of the Royal Anthropological Institute*, LXXXI (1951). In the article cited in Note 83, above, Berndt set the number of lines of descent at three. Criticized by Leach at the time when he wrote his article, he acknowledged subsequently, in private conversation and correspondence, that I had obtained, on purely deductive grounds, the solution that among all those proposed until then

- came closest to the one which he validated himself in the field.
- My interpretation of the Murngin system was the object of an admirably lucid and penetrating analysis by J. P. B. de Josselin de Jong, *Lévi-Strauss's Theory on Kinship and Marriage* (Leiden: 1952).
85. Murdock, *op. cit.*, pp. 131-2.
  86. Lowie, *Social Organization*, *op. cit.*, Chapter III.
  87. Lowie, *Primitive Society*, *op. cit.*, Chapter III.
  88. Murdock, *op. cit.*, p. 259.
  89. A. L. Kroeber, review of R. H. Lowie, *Primitive Society*, in *American Anthropologist*, XXII, No. 4 (1920), pp. 377-81.
  90. R. H. Lowie, "Exogamy, and the Classificatory Systems of Relationship," *American Anthropologist*, XVII, No. 2 (1915); "Relationship Terms" in *Encyclopaedia Britannica* (1929).
  91. R. H. Lowie, "The Matrilineal Complex," *University of California Publications in American Archaeology and Anthropology*, XVI, No. 2 (1919), pp. 29-45.
  92. R. H. Lowie, "Notes on Hopi Clans," *American Museum of Natural History, Anthropological Papers*, XXX, Part VI (1929), pp. 303-60.
  93. A. R. Radcliffe-Brown, "The Mother's Brother in South Africa," *South African Journal of Science*, XXI (1924), pp. 542-55; C. Lévi-Strauss, "Structural Analysis in Linguistics and in Anthropology," Chapter II of the present volume.
  94. Lowie, *Primitive Society*; "Hopi Kinship," *American Museum of Natural History, Anthropological Papers*, XXX, Part VII (1929), pp. 361-88.
  95. Lowie, *Primitive Society*.
  96. *Ibid.*, pp. 104-5.
  97. R. H. Lowie, *The Crow Indians* (New York: 1935), and *Social Organization*, Chapters XV, XVI, XVII.
  98. On this point, see C. Lévi-Strauss, "The Family," in H. L. Shapiro (ed.), *Man, Culture and Society* (Oxford: 1956), Chapter XII.
  99. Lowie, *Primitive Society*, Chapter XIII.
  100. Lowie, *The Origin of the State* (New York: 1927), *Social Organization*, Chapters VI, VII, XII-XIV, "Some Aspects of Political Organization among American Aborigines" (Huxley Memorial Lecture), *Journal of the Royal Anthropological Institute*, LXXVIII (1948), pp. 11-24.
  101. M. Fortes and E. E. Evans-Pritchard, *op. cit.*
  102. Lowie, *Social Organization*.
  103. In connection with the approach of this chapter, see especially M. Mead's "Character Formation and Diachronic Theory" in M. Fortes (ed.), *Social Structure: Studies Presented to A. R. Radcliffe-Brown*, pp. 18-34.
  104. A. R. Radcliffe-Brown, "Kinship Terminology in California," *American Anthropologist*, XXXVII, No. 3 (1935), pp. 530-35, "On Joking Relationships," *Africa*, XIII, No. 3 (1940), pp. 195-210, and "A Further Note on Joking Relationships," *Africa*, XIX, No. 2 (1949), pp. 133-40; M. E. Opler, "Apache Data Concerning the Relation of Kinship Terminology to Social Classification," *American Anthropologist*, XXXIX, No. 2 (1937), pp. 201-12, and "Rule and Practice in the Behavior Pattern between Jicarilla Apache Affinal Relatives," *American*

*Anthropologist*, XLIX, No. 3 (1947), pp. 453-62; C. S. Brand, "On Joking Relationships," *American Anthropologist*, L (1948), pp. 160-1.

105. In a short book devoted to refuting *Les Structures élémentaires de la parenté*, Homans and Schneider attempt to reduce the rules of preferential marriage to systems of attitudes. They attack the principle, postulated in *Les Structures*, that there is no necessary connection between matrilineal or patrilineal marriage on the one hand, and the type of descent—patrilineal or matrilineal—on the other. In support of their own interpretation, according to which matrilineal marriage would be a function of patrilineal descent, they invoke statistical correlations which prove nothing. Actually, societies with patrilineal descent are much more numerous than societies with matrilineal descent. Furthermore, matrilineal marriage is more frequent than patrilineal marriage. Thus, if the distribution occurred at random, we might expect that the incidence of societies characterized by an association between patrilineal descent and matrilineal marriage would be higher, and thus the correlation claimed by my critics would be meaningless. After re-examining this postulated correlation on the basis of a larger sample (564 societies), Murdock concludes: "The worldwide incidence of such preferences . . . is so low as to cast some doubt on the validity of the theoretical interpretation advanced." (G. P. Murdock, "World Ethnographic Sample," *American Anthropologist*, n.s., LIX, No. 4 [1957], p. 687.)

I still maintain, in the same terms that I first employed, that there is no necessary connection between marriage with the unilateral cross-cousin and type of descent; in other words, that none of the conceivable combinations implies a contradiction. It is possible and even probable, however, that empirically the two types of marriage are more frequently associated with one or the other type of descent. If this should be the case, this *statistical correlation* (not to be confused with a *logical connection*) would require an explanation. I would be inclined to find it in the instability characteristic of matrilineal societies (a theme already developed in *Les Structures*), which would make it more difficult for them to adopt long cycles of reciprocity, while the extremely short cycles of patrilineal marriage would be less affected by the conflicts always found in matrilineal societies. Homans and Schneider's theoretical interpretation seems to me completely unacceptable. They explain the preference of patrilineal societies for matrilineal marriage in terms of psychological factors, such as the transference of an adolescent's emotional feelings to his maternal uncle's group. If such were the case, matrilineal marriage would be more frequent; but it would not have to be *prescribed*. With reference to a particular case, Homans and Schneider revert to the psychological theory advocated by Westermarck to explain the incest taboo; we had hoped that anthropology had outgrown its old errors. See G. C. Homans and D. M. Schneider, *Marriage, Authority, and Final Causes: A Study of Unilateral Cross-Cousin Marriage* (Glencoe: 1955).

106. See C. Lévi-Strauss, *Tristes Tropiques* (Paris: 1955), Chapter XXIX, which re-examines the themes of a previous study, "The Social and Psychological Aspects of Chieftainship in a Primitive Tribe," *Transac-*

- tions of the New York Academy of Sciences*, series 2, VII, No. 1 (1944).
107. A. M. Hocart, "Les Castes," *Annales du Musée Guimet, Bibliothèque de vulgarisation*, LIV (Paris: 1938); K. Davis, "Intermarriage in Caste Societies," *American Anthropologist*, XLIII (1941), pp. 376-95; C. Lévi-Strauss, *Les Structures élémentaires de la parenté*, Chapters XXIV to XXVII.
108. A. Rapoport, "Outline of Probabilistic Approach to Animal Sociology," *Bulletin of Mathematical Biophysics*, XI (1949), pp. 183-96, 273-81.
109. This qualification appears to me today (1957) to be superfluous. There are societies characterized by hierarchical and intransitive cycles and quite comparable to the pecking order—as, for instance, in the Fiji Islands, where the population was organized until the beginning of the twentieth century into fiefs which were interconnected by relations of fealty, so that fief A might be a vassal of fief B, B of C, C of D, and D of A. Hocart described and explained this structure, which at first sight seems unintelligible, by pointing out that in Fiji two forms of vassalage exist—vassalage by right and vassalage by conquest. Fief A might thus be traditionally a vassal of B, B of C, and C of D, whereas fief D might have recently become, as a result of an ill-fated war, a vassal of A. Not only is this structure the same as that of the pecking order, but—and this passed unnoticed—anthropological theory out-distanced mathematical interpretation by several years, since the latter is based on the distinction between two variables which operate with a time-lag between them—and this corresponds exactly to Hocart's (posthumous) description. See A. M. Hocart, *The Northern States of Fiji*, Occasional Publications No. 11, Royal Anthropological Institute (London: 1952).
110. For a striking example of the local transformation from one type into another, see K. Gough, "Female Initiation Rites on the Malabar Coast," *Journal of the Royal Anthropological Institute*, LXXXV (1955), pp. 47-48.
111. M. Fortes, in *Social Structure: Studies Presented to A. R. Radcliffe-Brown*.
112. Radcliffe-Brown, "Religion and Society," *op. cit.*
113. New York, 1945.
114. New York, 1951.
115. G. A. Reichard, *Navaho Religion: A Study in Symbolism*, 2 vols., Bollingen Series, No. XVIII (New York: 1950).
116. S. F. Nadel, "Witchcraft in Four African Societies: An Essay in Comparison," *American Anthropologist*, LIV, Part I (1952), pp. 18-29.
117. S. F. Nadel, "Shamanism in the Nuba Mountains," *Journal of the Royal Anthropological Institute*, LXXVI, Part I (1946), pp. 25-38.
118. G. Dumézil, *Loki* (Paris: 1948).
119. Wittfogel and Goldfrank, *op. cit.*
120. M. Hunter-Wilson, "Witch Beliefs and Social Structure," *American Journal of Sociology*, LVI, No. 4 (1951), pp. 307-13.
121. Von Neumann and Morgenstern, *op. cit.*, p. 14.