Claude Lévi-Strauss (1958)

Structural Anthropology
Chapter II

Structural Analysis in Linguistics and in Anthropology

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LINGUISTICS OCCUPIES a special place among the social sciences, to whose ranks it unquestionably belongs. It is not merely a social science like the others, but, rather, the one in which by far the greatest progress has been made. It is probably the only one which can truly claim to be a science and which has achieved both the formulation of an empirical method and an understanding of the nature of the data submitted to its analysis. This privileged position carries with it several obligations. The linguist will often find scientists from related but different disciplines drawing inspiration from his example and trying to follow his lead. *Noblesse oblige*. A linguistic journal like *Word* cannot confine itself to the illustration of strictly linguistic theories and points of view. It must also welcome psychologists, sociologists, and anthropologists eager to learn from modern linguistics the road which leads to the empirical knowledge of social phenomena. As Marcel Mauss wrote - already forty years ago: "Sociology would certainly have progressed much further if it had everywhere followed the lead of the linguists. . . ." The close methodological which exists between the two disciplines imposes a special obligation of collaboration upon them.

Ever since the work of Schrader it has been unnecessary to demonstrate the assistance which linguistics can render to the anthropologist in the study of kinship. It was a linguist and a philologist (Schrader and Rose) who showed the improbability of the hypothesis of matrilineal survivals in the family in antiquity, to which so many anthropologists still clung at that time. The linguist provides the anthropologist with etymologies which permit him to establish between certain kinship terms relationships that were not immediately apparent. The anthropologist, on the other hand, can bring to the attention of the linguist customs, prescriptions, and prohibitions that help him to understand the persistence of certain features of language or the instability of terms or groups of terms. At a meeting of the Linguistic Circle of New York, Julien Bonfante once illustrated this point of view by reviewing the etymology of the word for uncle in several Romance languages. The Greek *theios* corresponds in Italian, Spanish, and Portuguese to *zio* and *tio*; and he added that in certain regions of Italy the uncle is called *barba*. The "beard," the "divine" uncle - what a wealth of

suggestions for the anthropologist! The investigations of the late A. M. Hocart into the religious character of the avuncular relationship and "theft of the sacrifice" by the maternal kinsmen immediately come to mind. Whatever interpretation is given to the data collected by Hocart (and his own interpretation is not entirely satisfactory), there is no doubt that the linguist contributes to the solution of the problem by revealing the tenacious survival in contemporary vocabulary of relationships which have long since disappeared. At the same time, the anthropologist explains to the linguist the bases of etymology and confirms its validity. Paul K. Benedict, in examining, as a linguist, the kinship systems of South East Asia, was able to make an important contribution to the anthropology of the family in that area.

But linguists and anthropologists follow their own paths independently. They halt , no doubt, from time to time to communicate to one another certain of their findings; these findings, however, derive from different operations, and no effort is made to enable one group to benefit from the technical and methodological advances of the other. This attitude might have been justified in the era when linguistic research leaned most heavily on historical analysis. In relation to the anthropological research conducted during the same period, the difference was one of degree rather than of kind. The linguists employed a more rigorous method, and their findings were established on more solid grounds; the sociologists could follow their example in renouncing consideration of the spatial distribution of contemporary types as a basis for their classifications. But, after all, anthropology and sociology were looking to linguistics only for insights; nothing foretold a revelation.

The advent of structural linguistics completely changed this situation. Not only did it renew linguistic perspectives; a transformation of this magnitude is not limited to a single discipline. Structural linguistics will certainly play the same renovating role with respect to the social sciences that nuclear physics, for example, has played for the physical sciences. In what does this revolution consist, as we try to assess its broadest implications? N. Troubetzkoy, the illustrious founder of structural linguistics, himself furnished the answer to this question. In one programmatic statement, he reduced the structural method to four basic operations. First, structural linguistics shifts from the study of *conscious linguistic* phenomena to study of their *unconscious* infrastructure; second, it does not treat *terms* as independent entities, taking instead as its - basis of analysis the *relations* between terms; third, it introduces the concept of *system* - "Modern phonemics does not merely proclaim that phonemes are always part of a system; it *shows* concrete phonemic systems and elucidates their structure" finally, structural linguistics aims at discovering *general laws,* either by induction "or . . . by logical deduction, which would give them an absolute character."

Thus, for the first time, a social science is able to formulate necessary relationships. This is the meaning of Troubetzkoy's last point, while the preceding rules show how linguistics must proceed in order to attain this end. It is not for us to show that Troubetzkoy's claims are justified. The vast majority of modern linguists seem sufficiently agreed on this point. But when an event of this importance takes place in one of the sciences of man, it is not only

permissible for, but required of, representatives of related disciplines immediately to examine its consequences and its possible application to phenomena of another order.

New perspectives then open up. We are no longer dealing with an occasional collaboration where the linguist and the anthropologist, each working by himself, occasionally communicate those findings which each thinks may interest the other. In the study of kinship problems (and, no doubt, the study of other problems as well), the anthropologist finds himself in a situation which formally resembles that of the structural linguist. Like phonemes, kinship terms are elements of meaning; like phonemes, they acquire meaning only if they are integrated into systems. "Kinship systems," Eke "phonemic systems," are built by the mind on the level of unconscious thought. Finally, the recurrence of kinship patterns, marriage rules, similar prescribed attitudes between certain types of relatives, and so forth, in scattered regions of the globe and in fundamentally different societies, leads us to believe that, in the case of kinship as well as linguistics, the observable phenomena result from the action of laws which are general but implicit. The problem can therefore be formulated as follows: Although they belong to *another order of reality*, kinship phenomena are *of the same type* as linguistic phenomena. Can the anthropologist, using a method analogous *in form* (if not in content) to the method used in structural linguistics, achieve the same kind of progress in his own science as that which has taken place in linguistics?

We shall be even more strongly inclined to follow this path after an additional observation has been made. The study of kinship problems is today broached in the same terms and seems to be in the throes of the same difficulties as was linguistics on the eve of the structuralist revolution. There is a striking analogy between certain attempts by Rivers and the old linguistics, which sought its explanatory principles first of all in history. In both cases, it is solely (or almost solely) diachronic analysis which must account for synchronic phenomena. Troubetzkoy, comparing structural linguistics and the old linguistics, defines structural linguistics as a "systematic structuralism and universalism," which he contrasts with the individualism and "atomism" of former schools. And when he considers diachronic analysis, his perspective is a profoundly modified one: "The evolution of a phonemic system at any given moment is directed by the *tendency toward a goal.* ... This evolution thus has a direction, an internal logic, which historical phonemics is called upon to elucidate." The "individualistic" and "atomistic" interpretation, founded exclusively on historical contingency, which is criticised by Troubetzkoy and Jakobson, is actually the same as that which is generally applied to kinship problems. Each detail of terminology and each special marriage rule is associated with a specific custom as either its consequence or its survival. We thus meet with a chaos of discontinuity. No one asks how kinship systems, regarded as synchronic wholes, could be the arbitrary product of a convergence of several heterogeneous institutions (most of which are hypothetical), yet nevertheless function with some sort of regularity and effectiveness.

However, a preliminary difficulty impedes the transposition of the phonemic method to

the anthropological study of primitive peoples. The superficial analogy between phonemic systems and kinship systems is so strong that it immediately sets us on the wrong track. It is incorrect to equate kinship terms and linguistic phonemes from the viewpoint of their formal treatment. We know that to obtain a structural law the linguist analyses phonemes into "distinctive features," which he can then group into one or several "pairs of oppositions." Following an analogous method, the anthropologist might be tempted to break down analytically the kinship terms of any given system into their components. In our own kinship system, for instance, the term *father* has positive connotations with respect to sex, relative age, and generation; but it has a zero value on the dimension of collaterality, and it cannot express an affinal relationship. Thus, for each system, one might ask what relationships are expressed and, for each term of the system, what connotation - positive or negative - it carries regarding each of the following relationships: generation, collaterality, sex, relative age, affinity, etc. It is at this "micro-sociological" level that one might hope to discover the most general structural laws, just as the linguist discovers his at the infraphonemic level or the physicist at the infra-molecular or atomic level. One might interpret the interesting attempt of Davis and Warner in these terms.

But a threefold objection immediately arises. A truly scientific analysis must be real, simplifying, and explanatory. Thus the distinctive features which are the product of phonemic analysis have an objective existence from three points of view: psychological, physiological, and even physical; they are fewer in number than the phonemes which result from their combination; and, finally, they allow us to understand and reconstruct the system. Nothing of the kind would emerge from the preceding hypothesis. The treatment of kinship terms which we have just sketched is analytical in appearance only; for, actually, the result is more abstract than the principle; instead of moving toward the concrete, one moves away from it, and the definitive system - if system there is - is only conceptual. Secondly, Davis and Warner's experiment proves that the system achieved through this procedure is infinitely more complex and more difficult to interpret than the empirical data. Finally, the hypothesis has no explanatory value; that is, it does not lead to an understanding of the nature of the system and still less to a reconstruction of its origins.

What is the reason for this failure? A too literal adherence to linguistic method actually betrays its very essence. Kinship terms not only have a sociological existence; they are also elements of speech. In our haste to apply the methods of linguistic analysis, we must not forget that, as a part of vocabulary, kinship terms must be treated with linguistic methods in direct and not analogous fashion. Linguistics teaches us precisely that structural analysis cannot be applied to words directly, but only to words previously broken down into phonemes. *There are no necessary relationships at the vocabulary level*. This applies to all vocabulary elements, including kinship terms. Since this applies to linguistics, it ought to apply *ipso facto* to the sociology of language. An attempt like the one whose possibility we are now discussing would thus consist in extending the method of structural linguistics while ignoring its basic requirements. Kroeber prophetically foresaw this difficulty in an article written many years ago. And if, at that time, he concluded that a structural analysis of

kinship terminology was impossible, we must remember that linguistics itself was then restricted to phonetic, psychological, and historical analysis. While it is true that the social sciences must share the limitations of linguistics, they can also benefit from its progress.

Nor should we overlook the profound differences between the phonemic chart of a language and the chart of kinship terms of a society. In the first instance there can be no question as to function; we all know that language serves as a means of communication. On the other hand, what the linguist did not know and what structural linguistics alone has allowed him to discover is the way in which language achieves this end. The function was obvious; the system remained unknown. In this respect, the anthropologist finds himself in the opposite situation. We know, since the work of Lewis H. Morgan, that kinship terms constitute systems; on the other hand, we still do not know their function. The misinterpretation of this initial situation reduces most structural analyses of kinship systems to pure tautologies. They demonstrate the obvious and neglect the unknown.

This does not mean that we must abandon hope of introducing order and discovering meaning in kinship nomenclature. But, we should at least recognise the special problems raised by the sociology of vocabulary and the ambiguous character of the relations between its methods and those of linguistics. For this reason it would be preferable to limit the discussion to a case where the analogy can be clearly established. Fortunately, we have just such a case available.

What is generally called a "kinship system" comprises two quite different orders of reality. First, there are terms through which various kinds of family relationships are expressed. But kinship is not expressed solely through nomenclature. The individuals or classes of individuals who employ these terms feel (or do not feel, as the case may be) bound by prescribed behaviour in their relations with one another, such as respect or familiarity, rights or obligations, and affection or hostility. Thus, along with what we propose to call the *system of terminology* (which, strictly speaking, constitutes the vocabulary system), there is another system, both psychological and social in nature, which we shall call the *system of attitudes.* Although it is true (as we have shown, above) that the study of systems of terminology places us in a situation analogous, but opposite, to the situation in which we are dealing with phonemic systems, this difficulty is "inversed," as it were, when we examine systems of attitudes. We can guess at the role played by systems of attitudes, that is, to insure group cohesion and equilibrium, but we do not understand the nature of the interconnections between the various attitudes, nor do we perceive their necessity. In other words, as in the case of language, we know their function, but the system is unknown.

Thus we find a profound difference between the *system of terminology* and the *system of attitudes,* and we have to disagree with A. R. Radcliffe-Brown if he really believed, as has been said of him, that attitudes are nothing but the expression or transposition of terms on the affective level. The last few years have provided numerous examples of groups whose chart of kinship terms does not accurately reflect family attitudes, and vice versa. It would be incorrect to assume that the kinship system constitutes the principal means of regulating

interpersonal relationships in all societies. Even in societies where the kinship system does function as such, it does not fulfil that role everywhere to the same extent. Furthermore, it is always necessary to distinguish between two types of attitudes: first, the diffuse, uncrystallised, and non-institutionalised attitudes, which we may consider as the reflection or transposition of the terminology on the psychological level; and second, along with, or in addition to, the preceding ones, those attitudes which are stylised, prescribed, and sanctioned by taboos or privileges and expressed through a fixed ritual. These attitudes, far from automatically reflecting the nomenclature, often appear as secondary elaborations, which serve to resolve the contradictions and overcome the deficiencies inherent in the terminological system. This synthetic character is strikingly apparent among the Wik Munkan of Australia. In this group, joking privileges sanction a contradiction between the kinship relations which link two unmarried men and the theoretical relationship which must be assumed to exist between them in order to account for their later marriages to two women who do not stand themselves in the corresponding relationship. There is a contradiction between two possible systems of nomenclature, and the emphasis placed on attitudes represents an attempt to integrate or transcend this contradiction. We can easily agree with Radcliffe-Brown and assert the existence of real relations of interdependence between the terminology and the rest of the system. Some of his critics made the mistake of inferring from the absence of a rigorous parallelism between attitudes and nomenclature, that the two systems were mutually independent. But this relationship of interdependence does not imply a one-to-one correlation. The system of attitudes constitutes, rather, a dynamic integration of the system of terminology.

Granted the hypothesis (to which we wholeheartedly subscribe) of a functional relationship between the two systems, we are nevertheless entitled, for methodological reasons, to treat independently the problems pertaining to each system. This is what we propose to do here for a problem which is rightly considered the point of departure for any theory of attitudes - that of the maternal uncle. We shall attempt to show how a formal transposition of the method of structural linguistics allows us to shed new light upon this problem. Because the relationship between nephew and maternal uncle appears to have been the focus of significant elaboration in a great many primitive societies, anthropologists have devoted special attention to it. It is not enough to note the frequency of this theme; we must also account for it. ...

Chapter XII
Structure and Dialectics

From Lang to Malinowski, through Durkheim, Lévy-Bruhl, and van der Leeuw, sociologists and anthropologists who were interested in the interrelations between myth and ritual have considered them as mutually redundant. Some of these thinkers see in each myth the ideological projection of a rite, the purpose of the myth being to provide a foundation for the

rite. Others reverse the relationship and regard ritual as a kind of dramatised illustration of the myth. Regardless of whether the myth or the ritual is the original, they replicate each other; the myth exists on the conceptual level and the ritual on the level of action. In both cases, one assumes an orderly correspondence between the two, in other words, a homology. Curiously enough, this homology is demonstrable in only a small number of cases. It remains to be seen why all myths do not correspond to rites and vice versa, and most important, why there should be such a curious replication in the first place.

I intend to show by means of a concrete example that this homology does not always exist; or, more specifically, that when we do find such a homology, it might very well constitute a particular illustration of a more generalised relationship between myth and ritual and between the rites themselves. Such a generalised relationship would imply a one-to-one correspondence between the elements of rites which seem to differ, or between the elements of any one rite and any one myth. Such a correspondence could not, however, be considered a homology. In the example to be discussed here, the reconstruction of the correspondence requires a series of preliminary operations. - that is, permutations or transformations which may furnish the key to the correspondence. If this hypothesis is correct, we shall have to give up mechanical causality as an explanation and, instead, conceive of the relationship between myth and ritual as dialectical, accessible only if both have first been reduced to their structural elements. ...

Chapter XV
Social Structure

THE 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 formalisation 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 *Anthropology:*

> "Structure" appears to be just a yielding to a word that has 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.'

Although this passage concerns more particularly the notion of "basic personality structure," it has devastating implications as regards the generalised 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 difference between two concepts which are so close to each that they have often been confused, namely, those of *social structure* and of *social relations.* It will be enough to state at this 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. 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.

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 realise that these features belong to two entirely different levels, or rather to two stages of the same process. On the observational level, in the main one could almost say the only 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 in relation to psycho-physiological 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. 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.

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 organisation 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 character 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 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 - 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.

Structure and Measure.

It is often believed that one of the main interests of the notion of structure is to permit the introduction of measurement in social anthropology. This view has been favoured by the frequent appearance of mathematical or semi-mathematical 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, 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 utilised by social scientist e to be found in J. von Neumann and O. Morgenstern, *Theory of Games and Economic Behaviour; N. Wiener, Cybernetics;* and C.

Shannon and W. Weaver, *The Mathematical Theory of Communication*. ...

Chapter XVI ...

I do not postulate a kind of pre-existent harmony between different levels of structure. They may be - and often are - completely contradictory, but the modes of contradiction all belong the same type. Indeed, according to dialectic materialism it should always be possible to proceed, by transformation, from economic or social structure to the structure of law, art, or religion. But Marx never claimed that there was only one type of transformation - for example, that ideology was simply a "mirror image" of social relations. In his view, these transformations were dialectic, and in some cases he went to great lengths to discover the crucial transformation which at first sight seemed to defy analysis.

If we grant, following Marxian thought, that infrastructures and superstructures are made up of multiple levels and that there various types of transformations from one level to another, it becomes possible - in the final analysis, and on the condition that we disregard content - to characterise different types in terms of the types of transformations which occur within them. These types of transformations amount to formulas showing the number, magnitude, direction, and order of the convolutions that must be unravelled, so to speak, in order to uncover (logically, not normatively) an ideal homologous relationship between the different structural levels.

Now, this reduction to an ideal homologous relationship is at the same time a critique. By replacing a complex model with a simple model that has greater logical value, the anthropologist reveals the detours and manoeuvres, conscious and unconscious, that each society uses in an effort to resolve its inherent contradictions - or at any rate to conceal them.

This clarification, already furnished by my previous studies, which Gurvitch should have taken into consideration, may expose me to still another criticism. If every society has the same flaw, manifested by the two-fold problem - of logical disharmony and social inequality, why should its more thoughtful members endeavour to change it? Change would mean only the replacement of one social form by another; and if one is no better than the other, why bother?

In support of this argument, Rodinson cites a passage from *Tristes Tropiques:* "No human society is fundamentally good, but neither is any of them fundamentally bad; all offer their members certain advantages, though we must bear in mind a residue of iniquity, apparently more or less constant in its importance. . . .

But here Rodinson isolates, in biased fashion, one step in a reasoning process by which I tried to resolve the apparent conflict between thought and action. Actually:

(1) In the passage criticised by Rodinson, the relativistic argument serves only

> to oppose any attempt at classifying, *in relation to one another,* societies remote from that of the observer - for instance, from our point of view, a Melanesian group and a North American tribe. I hold that we have no conceptual framework available that can be legitimately applied to societies located opposite poles of the sociological world and considered in their mutual relationships.
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> (2) On the other hand, I carefully distinguished this first frame from a very different one, which would consist in comparing remote societies, but two historically related stages in the development of our own society - or, to generalise, of the observer's society. When the frame of reference is thus "internalised," everything changes. This second phase permits us, without retaining anything from any particular society,

. . . to make use of one and all of them in order to distinguish those principles of social life which may be applied to the reform of our own customs, and not of those of societies foreign to our own. That is to say, in relation to our own society we stand in a position of privilege which is exactly contrary to that which I have just described; for our own society is the only one that we can transform and yet not destroy, since the changes we should introduce would come from within.

Far from being satisfied, then, with a static relativism - as are certain American anthropologists justly criticised by Rodinson (but with whom he wrongly identifies me) - I denounce it as a danger ever-present on the anthropologist's path. My solution is constructive, since it derives from the same principles, two apparently contradictory attitudes, namely, respect for societies very different from ours, and active participation in the transformation of our own society.

Is there any reason here, as Rodinson claims, "to reduce Billancourt to desperation"? Billancourt would deserve little consideration if cannibalism in its own way (and more seriously so than primitive man-eaters, for its cannibalism would be *spiritual*), should feel it necessary to its intellectual and moral security that the Papuans become nothing but proletarians. Fortunately, anthropological theory does not play such an important role in trade union demands. On the other hand, I am surprised that a scientist with advanced ideas should present an argument already formulated by thinkers of an entirely different orientation.

Neither in *Race and History* nor in *Tristes Tropiques* did I intend to disparage the idea of progress; rather, I should like to see progress transferred from the rank of a universal category of human development to that of a particular mode of existence, characteristic of our own society - and perhaps of several others - whenever that society reaches the stage of self-awareness.

To say that this concept of progress - progress considered as an internal property of a

given society and devoid of a transcendent meaning outside it - would lead men to discouragement, seems to me to be a transposition in the historical idiom and on the level of collective life, of the familiar argument that all morality would be jeopardised if the individual ceased to believe in the immortality of his soul. For centuries, this argument, so much like Rodinson's, was raised to oppose atheism. Atheism would "reduce men to desperation" - most particularly the working classes, who, it was feared, would lose their motivation for work if there were no punishments or rewards promised in the hereafter.

Nevertheless, there are many men (especially in Billancourt) who accept the idea of a personal existence confined to the duration of their earthly life; they have not for this reason abandoned their sense of morality or their willingness to work for the improvement of their lot and that of their descendants.

Is what is true of individuals less true of groups? A society can live, act, and be transformed, and still avoid becoming intoxicated with the conviction that all the societies which preceded it during tens of millenniums did nothing more than prepare the ground for *its* advent, that all its contemporaries - even those at the antipodes - are diligently striving to overtake it, and that the societies which will succeed it until the end of time ought to be mainly concerned with following in its path. This attitude is as naive as maintaining that the earth occupies the center of the universe and that man is the summit of creation. When it is professed today in support of our particular society, it is odious.

What is more, Rodinson attacks me in the name of Marxism, whereas my conception is infinitely closer to Marx's position than his. I wish to point out, first, that the distinctions developed in *Race and History* among stationary history, fluctuating history and cumulative history can be derived from Marx himself:

> The simplicity of the organisation for production in those, self-sufficing communities that constantly reproduce themselves in the same form and, when accidentally destroyed, spring again on the spot and with the same name - this simplicity supplies the key to the secret of the unchangeableness of Asiatic Societies, an unchangeableness in such striking contrast with constant dissolution and refounding of Asiatic states, and never-ceasing changes of dynasty.

Actually, Marx and Engels frequently express the idea that primitive, or allegedly primitive, societies are governed by "blood ties" (which, today, we call kinship systems) and not by economic relationships. If these societies were not destroyed from without, they might endure indefinitely. The temporal category applicable to them has nothing to do with the one we employ to understand, the development of our own society.

Nor does this conception contradict in the least the famous dictum of the *Communist Manifesto* that "the history of all hitherto existing society is the history of class struggles." In the light of Hegel's philosophy of the State, this dictum does not mean that the class struggle is co-extensive with humanity, but that the ideas of history and society can be applied, in the full sense which Marx gives them, only from the time when the class struggle first appeared. The letter to Weydemeyer clearly supports this: "What I did that was new," Marx wrote,

"was prove . . . that the *existence of classes* is only bound up with *particular historical phases in the development of production. . . .*"

Rodinson should, therefore, ponder the following comment by Marx in his posthumously published introduction to *A Contribution to the Critique of Political Economy:*

> The so-called historical development amounts in the last analysis to this, that the last form considers its predecessors as stages leading up to itself and perceives them always one-sidedly, since it is very seldom and only under certain conditions that it is capable of self-criticism . . .

This chapter had already been written when Jean-François Revel published his lively, provocative, but often unfair study.

Since part of his chapter VIII concerns my work, I shall briefly reply -

Revel criticises me, but not without misgivings. If he recognised me for what I am - an anthropologist who has conducted field work and who, having presented his findings, has re-examined the theoretical principles of his discipline on the basis of these specific findings and the findings of his colleagues - Revel would, according to his own principles, refrain from discussing my work. But he begins by changing me into a sociologist, after which he insinuates that, because of my philosophical training, my sociology is nothing but disguised philosophy. From then on we are among colleagues, and Revel can freely tread on my reserves, without realising that he is behaving toward anthropology exactly as, throughout his book, he upbraids philosophers for behaving toward the other empirical sciences.

But I am not a sociologist, and my interest in our own society is only a secondary one. Those societies which I seek first to understand are the so-called primitive societies with which anthropologists are concerned. When, to Revel's great displeasure, I interpret the exchange of wine in the restaurants of southern France in terms of social prestations, my primary aim is not to explain contemporary customs by means of archaic institutions but to help the reader, a member of a contemporary society, to rediscover, in his own experience and on the basis of either vestigial or embryonic practices, institutions that would otherwise remain unintelligible to him. The question, then, is not whether the exchange of wine is a survival of the *potlatch,* but whether, by means of this comparison we succeed better in grasping the feelings, intentions, and attitudes of the native involved in a cycle of prestations. The ethnographer who has lived among natives and has experienced such ceremonies as either a spectator or a participant, is entitled to an opinion on this question; Revel is not.

Moreover, by a curious contradiction, Revel refuses to admit that the categories of primitive societies may be applied to our own society, although he insists upon applying our categories to primitive societies. "It is absolutely certain," he says, that prestations "in which the goods of a society are finally used up . . . correspond to the specific conditions of a mode of production and a social structure." And he further declares that "it is even probable - an exception unique in history, which would then have to be explained - that prestations mask the economic exploitation of certain members of each society of this type by others."

How can Revel be "absolutely certain"? And how does he know that the exception would be "unique in history"? Has he studied Melanesian and Amerindian institutions in the field? Has, he so much as analysed the numerous works dealing with the *kula* and its evolution from 1910 to 1950, or with the *potlatch* from the beginning of the nineteenth century until the twentieth? If he had, he would know, first of all, that it is absurd to think that all the goods of a society are used up in these exchanges. And he would have more precise ideas of the proportions and the kinds of goods involved in certain cases and in certain periods. Finally, and above all, he would be aware that, from the particular viewpoint that interests him - namely, the economic exploitation of man by man - the two culture areas to which he refers cannot be compared. In one of them, this exploitation presents characteristics which we might at best call pre-capitalistic. Even in Alaska and British Columbia, however, this exploitation is an external factor: It acts only to give greater scope to institutions which can exist without it, and whose general character must be defined in other terms.

Should Revel hasten to protest, let me add that I am only paraphrasing Engels, who by chance expressed his opinion on this problem, and with respect to the same societies which Revel has in mind. Engels wrote:

In order finally to get clear about the parallel between the Germans of Tacitus and the American Redskins I have made some gentle extractions from the first volume of your Bancroft [*The Native Races of the Pacific States,* etc.]. The similarity is indeed all the more surprising because the method of production is so fundamentally different - here hunters and fishers without cattle-raising or agriculture, there nomadic cattle-raising passing into agriculture. It just proves how at this stage the type of production is less decisive than the degree in which the old blood bonds and the old mutual community of the sexes within the tribe have been dissolved. Otherwise the Tlingit in the former Russian America could not be the exact counterpart of the Germanic tribes . . . .

It remained for Marcel Mauss, in *Essai sur le Don* (which Revel criticises quite inappropriately) to justify and develop Engels' hypothesis that there is a striking parallelism between certain Germanic and Celtic institutions and those of societies having the *potlatch*. He did this with no concern about uncovering the "specific conditions of a mode of production," which, as Engels had already understood, would be useless. But then Marx and Engels knew incomparably more anthropology almost a hundred years ago than Revel knows today.

I am, on the other hand, in full agreement with Revel when he writes, "Perhaps the most serious defect which philosophy has transmitted to sociology is . . . the obsession with creating in one stroke holistic explanations." He has here laid down his own indictment. He rebukes me because I have not proposed explanations and because I have acted as if I believed "that there is fundamentally no reason why one society adopts one set of institutions and another society other institutions." He requires anthropologists to answer questions such as: "Why are societies structured along different lines? Why does each structure evolve? . . . *Why are there differences* [Revel's italics] between institutions and between societies, and

what responses to what conditions do these differences imply . . . ?" These questions are highly pertinent, and we should like to be able to answer them. In our present state of knowledge, however, we are in a position to provide answers only for specific and limited cases, and even here our interpretations remain fragmentary and isolated. Revel can believe that the task is easy, since for him "it is absolutely certain" that ever since the social evolution of man began, approximately 500,000 years ago, economic exploitation can explain everything.

As we noted, this was not the opinion of Marx and Engels. According to their view, in the non- or pre-capitalistic societies kinship ties played a more important role than class relations. I do not believe that I am being unfaithful to their teachings by trying, seventy years after Lewis H. Morgan, whom they admired so greatly, to resume Morgan's endeavour - that is, to work out a new typology of kinship systems in the light of knowledge acquired in the field since then, by myself and others."

I ask to be judged on the basis of this typology, and not on that of the psychological or sociological hypotheses which Revel seizes upon; these hypotheses are only a kind of mental scaffolding, momentarily useful to the anthropologist as a means of organising his observations, building his classifications, and arranging his types in some sort of order. If one of my colleagues were to come to me and say that my theoretical analysis of Murngin or Gilyak kinship systems was inconsistent with his observations, or that while was in the field I misinterpreted chieftainship among the Nambicuara, the place of art in Caduveo society, the social structure of the Bororo, or the nature of clans among the Tupi-Cawahib, I should listen to him with deference and attention. But Revel, who could not care less about patrilineal descent, bilateral marriage, dual organisation, or dysharmonic systems, attacks me - without even understanding that I seek only to describe and analyse certain aspects of the objective world - for "flattening out social reality," For him everything is flat that cannot be instantaneously expressed in a, language which he may perhaps use correctly in reference to Western civilisation, but to which its inventors explicitly denied any other application. Now it is my turn to exclaim: Indeed, "what is the use of philosophers?"

Reasoning in the fashion of Revel and Rodinson would mean surrendering the social sciences to obscurantism. What would we think of building contractors and architects who condemned cosmic physics in the name of the law of gravity and under the argument that a geometry based on curved spaces would render obsolete the traditional techniques for demolishing or building houses? The house-wrecker and the architect are right to believe only in Euclidean geometry, but they do not try to force it upon the astronomer. And if the help of the astronomer is required in remodelling his house, the categories he uses to understand the universe do not automatically prevent him from handling the pick-axe and plumb-line.

Further Reading:

Biography | Durkheim | Saussure | Jakobson | Parsons | Marx | Althusser

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