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ON MANIPULATED SOCIOLOGICAL MODELS

In his interesting paper, Mr. Maybury-Lewis takes me to task on two grounds. In the first place, I am accused of misrepresenting the ethnographical data, in two major instances at least. Besides, my methodological approach is said to be "morally" wrong. As to the first point, I must disagree with my critic, in one case, simply because it is not so, and in the other case, because he mistakes a theoretical reconstruction for a description of actual facts. As to the second point, I can only try to clarify the line of reasoning which I have followed and which, I feel, Mr. M. L. has misunderstood completely.

Mr. M.L. starts his paper with a re-examination of what I shall call, for brevity's sake, "the Winnebago discrepancy". When describing the old village, the informants of the lower phratry, he says, "did not consider the dual division to be contextually relevant". But — and this is precisely my point — they could not do away with it unless they introduced in their description another dual division, admittedly not a social one, but not merely ecological either, since it emerges as a substitute for, and a transformation of, the other one.

Then, when in his fig. 3, Mr. M.L. puts one diagram on top of the other, and claims to have thus obtained a satisfactory picture of the village lay-out, he is doing the very thing which he so bitterly reproaches me for, i.e. manipulation of sociological models. What is actually given to us, on the empirical level, consists of two drawings of the Winnebago village, each of which bears a complementary relation to the other. These drawings not only present a fragmentary image of a total situation, so that all we have to do is to supplement one with the other: they also stand in opposition to each other, and this relation of opposition cannot be lightly dismissed since it is itself a part of the ethnographical data.

In other words, if the distinction between the cleared ground and the woods "is not relevant" in one diagram, why does it become so in the other? What we have here is nothing more than a curious fact, the true

interpretation of which probably will never be known. This gives us more reason to follow up each and every line of interpretation. I have taken up one, which offers at least the interest of not yet having been considered. What would be the theoretical consequences, if the social distinction between the two moieties, on the one hand, and on the other hand, the ecological-philosophical distinction between cleared ground (i.e. "culture"), and the wilderness beyond the timber line (i.e. "nature"), should be recognized as two different codes, used to carry the same message, but at the cost of complementary distortions?

Such being the working hypothesis, I cannot be reproached for confusing dual organization, dualist system, and dualism. As a matter of fact, I was avowedly trying to override these classical distinctions, with the aim of finding out if they could not be dealt with as open — and to some extent conflicting — expressions of a reality, to be looked for at a deeper level.

To fulfill this programme, I have proceeded in the following way. First, the "Winnebago discrepancy" reveals, in a tribe which is known to categorize dualistically on the social plane, the operation of another kind of dualism, which I proposed to call "concentric". This concentric dualism is then better analyzed, in reference to another population — the Trobriand — where it is known to exist independently from the other type (to object, as does Mr. M. L., that the Trobriand have no dual organization is begging the obvious). Having thus identified the phenomenon of concentric dualism, I now proceed to discuss societies where the two types co-exist: Bororo, Timbira, and so many Indonesian groups that I have found easier to start with a syncretic model rather than choose a particular example. The conclusions of the discussion are: 1) that there is a functional relation between the two types; 2) that the concentric pattern is logically more essential than the diametric one; 3) that this concentric pattern covers a ternary system which, consequently, can be said to underly the diametric dualism itself.

Therefore, I refuse to let myself be bound by an empirical opposition between a kind of dualism, mere "reflection of symbolic values" in the village lay-out, and a so-called true dualism "involving the segments of society". Is the latter not symbolic also? And does not the former entail, like the other, concrete rights and obligations? What I have tried to do is to transcend those partial views and to put to test a kind of common language, into which the two forms of dualism could be translated, thus enabling us to reach — not on the level of observation, of course — a "generalized" interpretation of all the phenomena of dualism. I hope

to have shown that this is not only possible, since all the considered instances of dualism can be treated, despite their obvious superficial heterogeneity, as the result of a combination of only five binary oppositions (p. 126 of my paper), but that this common language reveals an important and hitherto undetected fact: dualism does not exist *per se*, it entails and implies a triadic way of thinking, of which each individual case of dualism (taken in a wide sense, but including, among other forms, dual organizations) should be considered as a simplification and as a "limit".

Admittedly, this marks a departure from the thesis put forward in *Les Structures élémentaires de la parenté*, as I have myself taken care to explain (p. 117). However, this new position will not "do away with the distinction between dyadic and triadic systems" and make my earlier work invalid, as Mr. M.L. seems to fear, with a show of concern for which I feel flattered! For practical purposes, the distinction remains as useful as it was. I only believe, and mathematicians with whom the problem was discussed agreed, that by treating dyadic systems now as a special case of the triadic formula, the general theory of reciprocity becomes greatly simplified. On the other hand, this approach seems to be more convenient for the purpose of historical reconstruction, since there are cases where the triadic "core" appears to be not only logically more simple, but also older, than the dyadic "upper-crust".

I come now to the ethnographical distortions. First, the Bororo N-S axis. Mr. M.L. may rest assured that, should the contradiction with the description by the Salesian Fathers be as great as he thinks, I would have been the first one to suspect that I had misunderstood my informants (whose statements, incidentally, were quite clear in that respect). But, 1) the observation was made in a different part of the Bororo country, and it did not constitute the only discrepancy from the Salesian account, based on villages located in another valley; 2) there is something quite similar, recorded in Colbacchini's older material; 3) even in Albisetti's more recent descriptions, the N-S axis can be claimed to exist, and in two different ways; first, there is a N-S axis inside the men's house, dividing the sectors allocated to each moiety; next (and granted the fact that the order of clan subdivisions was accidentally reversed in the diagram p. 111), the very emphasis that Mr. M.L. puts on the correlation: lower-west, upper-east, shows that it can only exist if there is a N-S axis operating *inside of each clan*. Thus, the difference between the Albisetti account and my own account is that, in one case, the N-S axis has a positive existence inside the men's house, and

a relative one outside, while, in the other case, the N-S axis is granted an absolute existence, both in and out.

In order to see there an insuperable contradiction, it would be necessary to postulate two things: first, that the Bororo social structure was perfectly identical throughout a once enormous territory; second, that the absolute N-S axis, as recorded on the rio Vermelho, did serve to separate the clans according to status distinctions.

On the first point, everybody will admit that such an homogeneity is most unlikely. The Bororo once occupied a territory as big as half of France; and, in historical times, it was still a quarter of the earlier superficies. The rate of growth and extinction could not be the same for each clan and in each village, especially on account of continuous warfare with neighbouring groups. Each village was probably confronted with demographic problems of its own, and the number of clans, their distribution on the village circle, must have varied considerably. What we find in the Salesian accounts can hardly be a description of a situation which once existed everywhere; it did not even exist where they were working themselves, as their earlier, more empirical descriptions show. Rather, they gave us, after many years of patient work, a kind of ideal reconstruction, the theoretical pattern best suitable to account for numerous local variations. Therefore, one should not unduly worry if the actual pattern recorded in a village, at one time, is not identical with the pattern recorded at a different time, in villages belonging to a distant group. On the other hand, we all should be aware that, when reasoning on the invaluable account of Bororo social organization, which the Salesian Fathers have produced as a result of many years of painstaking reconstruction, we are not confronted with empirical data: we are rather manipulating a sociological model, notwithstanding Mr. M.I.'s indictment of such a procedure, in which he should realize that he indulges as much as I myself do.

In any case, Mr. M.L. might say that one should have chosen between the two variants, instead of using both of them despite the fact that they are mutually incompatible. This would only be true if the terms *xobbuguingue*, *xebbeguingue* were given the same meaning in the Salesian earlier and later descriptions, and in my own description. Should these terms always mean "superior" and "inferior", and denote differences of status, there would be a contradiction indeed, since, instead of having "inferior" and "superior" families inside each clan, the West clans would be absolutely inferior, the East clans absolutely superior, and it would be impossible to merge two systems into one.

But such was not the case on the rio Vermelho, and it was not the case either in Colbacchini's earlier account from the rio das Garças region. According to both of us, the native terms had a topographical meaning: "uphill", "downhill" for Colbacchini's informants, "upstream", "downstream" for mine. It just happens that, in Bororo as in many other languages, the same terms connote those three meanings. It was all the more easy for the Bororo to avoid equivocation when they had two other optional systems of oppositions, to express status distinctions: "great" and "small" on the one hand, "red" and "black" on the other.

"It is not correct to say that two clans in each moiety represent the two legendary heroes of the Bororo". I agree that "only two clans belonging to one moiety now do, and two clans belonging to the other moiety once did". However, if this merging of a "synchronic analysis" with a "diachronic analysis" is an ethnographical mistake, does not Mr. M.L. commit the same mistake, when, trying to explain the "Winnebago discrepancy", he puts forth a mythical account according to which the lower phratry "may once have held the chieftainship, or at least shared it", which, by the way, is exactly the same story as the one I was relying upon in the case of the Bororo?

There is, however, a more important point to make. In none of the two cases are we confronted with an opposition between synchronic and diachronic. Here, the past referred to is mythical, not historical. And, as a myth, the content is actually given to the native consciousness. When the Bororo myth tells us about a time when two Tugare clans, instead of two Cera clans, were connected with the cultural heroes, they may refer to past events. Of the truth of these events, we will remain for ever ignorant. But we are made quite sure that, in the present, some kind of connection is felt to exist, between the dispossessed clans and the cultural heroes.

Coming now to the Winnebago, Mr. M.L. misunderstood me, if he believed me to pretend that the Winnebago village actually comprised 12 clans distributed into 3 groups. This statement, as I made it, did not claim to be an ethnographical description of the Winnebago village as it existed. It merely described a theoretical diagram, purporting to re-organize ethnographical data which, at the observation level, do not clearly exhibit those hidden properties (or else, my undertaking would have been useless).

Thus, there is no claim that the Winnebago village was ever distributed into three groups of four clans each. What is being suggested here is quite different, namely, that on purely deductive

grounds, the three cases made the object of a comparison, may be treated as transformations one of the other under several conditions, one of them being that the Winnebago village be analyzed in such and such a way.

This inference being deductively drawn, it is highly gratifying that the ethnographical data should give it independent support. There is evidence in Radin's material that this interpretation of the village structure did not exist only in the anthropologist's mind, but also in the mind of the natives themselves. If it were imposed to us by the manifest content of the ethnographical data, there would be nothing to demonstrate, it would be sufficient to describe what one sees, or what one is being told. On the other hand, a theoretical hypothesis which deviates from the manifest content of the ethnographical data, is substantially upheld, if we are able to discover, in the latent content supplied by the myths, the religious representations, etc., some data showing a remarkable parallelism between the native categories, and those arrived at by the way of theoretical reconstruction. In the Winnebago case, the quotation from Radin 1923, p. 241, suffices to give proof of a latent ternary system, underlying the manifest dualism, which is far above anything could be expected, especially as it comes from a part of the world where the operation of such ternary systems had remained hitherto unsuspected.

The argument that, in any case, the triad "would be irrelevant in consideration of the Winnebago marriage system" appears, along these lines, out of order. The point made in the diagram is that, even if the system should be considered as ternary, this would not impair the dual marriage system. As a matter of fact, the usefulness of the diagram lies in its enabling us to "see" the social structure either as ternary (Sky, Water, Land) or as binary (Higher, Lower). Also, the NW-SE axis, "spatial referent of Winnebago dualism" is not "omitted", since the diagram makes clear that marriage possibilities are between Higher (= Sky) on the one hand, and Lower (= Water + Land) on the other.

As to the village circle, which Mr. M.L. claims to be "irrelevant in a diagram of marriage relations", there are two comments to be made. In the first place and contrary to what Mr. M.L. believes, the diagrams do not concern marriage relations alone. Rather, they are intended to show how marriage relations, social structure, village lay-out, religious representations, etc. are all part of a system, the difference being that, in each case, they are assigned different functions, or — to express it

in diagrammatic terms — they are permuted in different topological positions. To put it differently, what a given society “says” in terms of marriage relations, is being “said” by another society in terms of village lay-out, in terms of religious representations by a third, etc.

In the second place, and to limit myself to the Winnebago ethnographical material which I am reproached to have misinterpreted, may I refer Mr. M.L. to Radin’s enlightening comments on the relationship between village and clan structure? If there are Winnebago myths representing the whole tribe as having once consisted of one village, can the overall social structure be really thought of as independent of the residential unit? Wisely I believe, Radin raises the question whether the “band” or village, “setting off one group against another” is not an early form of social grouping, so that, as elsewhere in North America, village organization may have preceded the clan (see Radin 1923, p. 184—185).

I shall not dwell at length on the discussion of the other diagrams, since Mr. M.L. follows the same erroneous line of confusing a theoretical analysis of models, intended to explain the ethnographical data (by reducing them to common factors), with an actual description of the data, as they appear to the empirical observer. Models would be useless, if they did not tell us more, and differently from, the data. Surely, the fact that most S.E. Asia natives state that women circulate, not men, does not invalidate the truth (to be covered by a generalized model) that nothing would be changed in the formal properties of the structure, if the situation were described the other way around, as some tribes actually do. The “tripod” in fig. 7 only expresses the fact that, in an asymmetrical marriage system as well as in a symmetrical one, there is a rule of exogamy in operation, but that, in the first case, it creates a sociological opposition between the sexes, whatever group they belong to, while, in the second case, the opposition exists between the groups, whatever sexes they include.

Mr. M.L. is equally mistaken when he states that the E-W axis does not appear in the Bororo diagram. True enough, it does not appear where the natives put it, since it has been demonstrated that in so doing, they let themselves be mystified by their own system. For that reason, it has been represented, as it should, by the tripod each branch of which bissects the three otherwise fully endogamous groups. On the other hand, the hypothesis that the N-S axis provides the only unifying factor is admittedly “weak”, not because the existence of this axis results from an ethnographical error — a charge that I have already shown

to be gratuitous —, but because I have myself so qualified it, and explained that, before being accepted, it requires a careful testing in the field.

Obviously, the Bororo diagram is not exhaustive — none of the diagrams are and none are meant to be —, it nevertheless represents quite satisfactorily, “on the one hand, a pair of moieties, and on the other hand a triad of endogamous groups” (Mr. M.L. appears to acknowledge it reluctantly, and as the outcome of an afterthought, in the footnote appended to the sentence just quoted). But the diagram does not pretend to show everything, only the functions which are recurrent in all the cases diagrammatically exemplified, and despite the fact that these functions do not manifest themselves, each time, on the same level of social reality.

We reach here a major point of disagreement. Should the only result of my paper be that disparate elements drawn from different societies “can be represented in identical patterns”, I would not consider such a demonstration devoided of “sociological implications”. For we may have found an way of showing that what appears superficially disparate may not be so, and that behind the bewildering diversity of empirical data, there may exist a small number of recurrent, identical properties, appearing over and over again, although combined differently.

To sum up, I suspect that Mr. M.L. remains, to some extent, the prisoner of the naturalistic misconceptions which have so long pervaded the British school. He claims to be a structuralist, he even claims to defend structuralism against my reckless manner of handling it. But he is still a structuralist in Radcliffe-Brown's terms, namely, he believes the structure to lie at the level of empirical reality, and to be a part of it. Therefore, when he is presented a structural model which departs from empirical reality, he feels cheated in some devious way. To him, social structure is like a kind of jig-saw puzzle, and everything is achieved when one has discovered how the pieces fit together. But, if the pieces have been arbitrarily cut, there is no structure at all. On the other hand, if, as is sometimes done, the pieces were automatically cut in different shapes by a mechanical saw, the movements of which are regularly modified by a cam-shaft, the structure of the puzzle exists, not at the empirical level (since there are many ways of recognizing the pieces which fit together): its key lies in the mathematical formula expressing the shape of the cams and their speed of rotation; something very remote from the puzzle as it appears to the player, although it “explains” the puzzle in the one and only intelligible way.

But Mr. M.L. writes: "Social relations cannot be formally represented by symbols in the same way as mathematical relations can. Accordingly, sociological models are not manipulable in the sense that mathematical equations are". Should he not explain, first, what he means by "social relations"? If he refers to concrete social relations, as given to the empirical observer, we cannot but agree while remembering that, in primary school, we were already taught the impossibility of adding pears to apples. But, if a distinction is made between the level of observation and symbols to be substituted to it, I fail to see why an algebraic treatment of, let us say symbols for marriage rules, could not teach us, when aptly manipulated, something about the way a given marriage system actually works, and bring out properties not immediately apparent to the empirical observer.

Of course, the final word should rest with experiment. However, the experiment suggested and guided by deductive reasoning will not be the same as the unsophisticated ones with which the whole process had started. These will remain as alien as ever to the deeper analysis. The ultimate proof of the molecular structure of matter is provided by the electronic microscope, which enables us to see actual molecules. This achievement does not alter the fact that henceforth the molecule will not become any more visible to the naked eye. Similarly, it is hopeless to expect a structural analysis to change our way of perceiving concrete social relations. It will only explain them better. If the structure can be seen, it will not be at the earlier, empirical level, but at a deeper one, previously neglected; that of those unconscious categories which we may hope to reach, by bringing together domains which, at first sight, appear disconnected to the observer: on the one hand, the social system as it actually works, and on the other, the manner in which, through their myths, their rituals and their religious representations, men try to hide or to justify the discrepancies between their society and the ideal image of it which they harbour.

When Mr. M.L. gives an absolute value to the distinction between those two domains, it is he, not I, who begs the question. The problem originally raised in my paper can be phrased in the following terms: does the so-called dual organization belong to the domain of real social segments, or does it belong to that of symbolic representations? If the available descriptions of the Bororo social structure are correct, then — as I have shown elsewhere and as Mr. M.L. seems to recognize — dual organization among the Bororo belongs to the domain of symbolic representations, since its operative value is cancelled, so to speak, by

actual rules of endogamy. On the other hand, Bororo concentric dualism, opposing the profane circle to the sacred center, should be endowed with a higher coefficient of objective truth, since there is nothing in the system to contradict it, and since it is permitted to unfold all its consequences, on both the religious and the social levels.

But the same thing cannot be said of other examples of dual organization elsewhere. Hence the conclusion that "actual social segments" and "symbolic representations" may not be as heterogeneous as it seems, but that, to some extent, they may correspond to permutable codes. Here lies one's right to deal with social segments and symbolic representations as parts of an underlying system endowed with a better explanatory value, although — or rather, because — empirical observation will never apprehend it as such.

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