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Referential and inferential reality. A rejoinder

In: Bijdragen tot de Taal-, Land- en Volkenkunde 122 (1966), no: 1, Leiden, 151-157

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REFERENTIAL AND INFERENTIAL REALITY¹

A REJOINDER

I am indebted to my colleague, Prof. Köbben, for his comment which, by pointing up the comparison between them, serves to throw his own and others' scientific preconceptions into greater relief. He reveals himself (both in speaking and in writing) as a somewhat agnostic empiricist, and prefers (for the time being?) not to go into the question on whether something called a social order can be said to exist or not. He creates one for himself by defining, classifying and comparing those social phenomena that in principle can be tested by empirical methods and that are relevant to a specific question *as though they were things*. The consequences of the "choses sociales" postulate, which has governed so many scientific minds since Durkheim's day, are far-reaching. It causes the scientist to invest social phenomena, just like other things such as organisms, natural phenomena and chemical substances, with functions and a certain continuity and consistency. These three characteristics of the "thing" are prerequisites for the existence of more or less fixed functional relationships between phenomena. In so doing, the researcher arrives at the following type of conclusion: where A, there B; and, similarly, where not-A, there not-B. The analogy of this reasoning with the method used in the natural sciences — one which is just as much the result of a certain type of approach — is immediately apparent. For Köbben, as for his spiritual father, Radcliffe-Brown, who even went so far as to regard social anthropology as a branch of the natural sciences, there is only *one* scientific method. There are of course, as Köbben recognises too, gradual differences between the method used in the social sciences and that used in the natural sciences. The "as though" situation is even more evident in the study of social phenomena than in the natural sciences. Things do not coincide with their definition. Whether marriage, economics, law and religion are

¹ The translation is by Mrs. E. Wentholt-Haig.

universally found phenomena is to no small extent a question of definition. Moreover, human beings, unlike other "things", can act of their own volition.

It is these and similar differences which find concise expression in the familiar saying that though man is an animal, he is a symbolizing one. This should prompt the cultural anthropologist to see his profession in terms of the sign, the symbol, rather than in terms of the thing. Man transforms the things of his "Umwelt" into signs; the "thing an sich" is thus not enough to go by. Köbben will perhaps retort that a symbol can also be regarded as a thing. I agree, but in the sense that it can only be a thing with a "false bottom". Signs and, *a fortiori*, symbols are, after all, things representing other things. Socio-cultural "things" occur on two levels of reality, an observable, a referential, one and a non-observable, an inferential, one. The study of these "things", which I prefer to call systems of symbols, therefore requires a special bi-dimensional method.

In view of Köbben's empiricism, it is not to be wondered at that his comments on my article are all variations on the theme of empirical testability. He is even of the opinion that I exceed the bounds of science when I make a conditional statement that is not corroborated by empirical testing. (What, then, are we to think of not empirically tested or testable statements of theologians, philosophers, mathematicians and astronomers? What is the test of the test?) The characterization of Maybury-Lewis given by Lévi-Strauss in response to the former's criticism is equally applicable to Köbben:

"... Mr. Maybury-Lewis remains, to some extent, the prisoner of the naturalistic misconceptions which have so long pervaded the British school... he is still a structuralist in Radcliffe-Brown's terms (all labels, we note, are of relative value, P), 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 with a structural model which departs from empirical reality, he feels cheated in some devious way..." (1960, p. 52, as cited by Schneider, 1965, pp. 25-26).

It will occasion no surprise when I say that what I most strongly object to in Köbben's comments, which are more the words of the well trained artisan than of the architect, is the empiricism and the closely related postulate of the "choses sociales", both of which I reject. His reasoning, which seems plausible enough at first, is implicitly based on both postulates and is completely pervaded by them.

I have, in addition, a serious methodological objection: he measures the constructions of others against standards derived from his own

constructions. In my opinion, criticism of a construction can only be pertinent:

- (a) if its postulates are not accepted, by showing them to be untenable and/or sterile;
- (b) if its postulates are accepted, by showing flaws in the construction built on them.

Köbben, however, tries to demonstrate flaws in the structural approach and its application on the basis of his own postulates and by the use of his own criteria. That is why he sees similarities that do not exist and why his criticism only grazes without scoring any direct hits. He states, for instance, that J. P. B. de Josselin de Jong, in his comparative study of the *couvade*, uses "exactly the same" methods as he himself used in his comparison of prophetic movements, thereby overlooking the fact that de Josselin de Jong's "ideational themes" are a reality behind the empirical reality. For de Josselin de Jong states that those patterns of belief manifest themselves in all uncivilized societies; they belong to the mental reality of the "uncivilized". In his early work, he reveals a certain affinity with the views of Lévy-Bruhl. (See, for instance, his dissertation.) It is interesting to compare this statement with that other, cautiously formulated one made after the war which I cited in my article (p. 141-142) and which relates to grouping tendencies . . . which are not *engendered* by specific psychological or historical situations, but which, on the contrary, themselves *give rise* to such situations.

Köbben, on the other hand, places his "main themes" ("hoofdmotieven") in the empirical reality and refers to them in his book (1964, p. 103), in contrast to his comments, not as themes but as *elements* which are present in a *specific movement* (at the empirical level, P.) and which largely characterize that movement. His "themes" stand primarily in the service of a typology, and those of de Josselin de Jong primarily in the service of an explanation.

Köbben asserts that I attach greater importance to 'representations' than he does. Yes and no. I am in complete agreement with him when he says that (*at the empirical level*) it is dangerous to claim the priority of representations over actions, or vice versa. The senseless struggle over whether priority is to be accorded to myth or to rite serves as a warning example. At the *supra-empirical level* of structure, I do postulate the priority of the human mind. In last analysis, culture and society are products of the human mind and it is in this sense that I regard the representations emanating from it as the prompters of

action. From this *general* point of view, I analyse and compare all *specific* cultures and societies as though they were complexes of symbol systems. This point of view need not coincide in any way at all with that of Hegel or philosophical "idealism". The structuralist employs the empirical test of the total social fact and of situational analysis. Neither Hegel nor any idealist ever did that.

Köbben is wide off the mark when he attributes to me the opinion that lines of descent occurring in a research model, in case of structures held to be universally valid must be *perceived* by the participants as well. Every research worker is in a position to know that he sometimes sees "things" of which the participant in a culture is not consciously aware, and vice versa. Köbben obviously finds the concept of latent representations a difficult one. "Latent representations" may be defined as what are to the participant of a culture self-evident expressions of the activity of the human mind of which he is not aware but which nevertheless influence his ideas and behaviour as a member of a society. Their existence is apparent from the undeniable fact that the speaker of a language — that cultural phenomenon par excellence — structures the sounds which he produces even though he can scarcely be said to be aware of either his own action or the actual structure of the language. I presume that in communicating with his "Umwelt" the participant in a culture is decisively influenced by representations emanating from his mind and by processes of his mind of which he is not aware. It is thus possible for a fixed connubium to exist between groups without the participants of the culture being aware of either the norm or its application. (For a striking example see P. E. de Josselin de Jong, 1956, pp. 148-168.)

I would, however, strongly emphasize the fact that representations included in a research model on the strength of the research worker's belief that they are latent representations may only be used to explain an empirical constellation if the test of the total social fact and a situational analysis clearly reveal their effects. I agree with Köbben on the point that working with the concept of latent representations contains a danger of arbitrariness. For while the participant can give *a posteriori* explanations of latent representations and their related behaviour, the observer can, in a manner of speaking, fall into the error of *a priori* explanations. Indeed, it is a danger which some "Leyden professional brothers" and, for that matter, Lévi-Strauss in his weaker moments have not always managed to avoid, to put it mildly.

Köbben's criticism of Lévi-Strauss's theory relating to cross-cousin

marriage misses the point altogether. Taking his own postulates as his starting point, he thinks he has caught out Lévi-Strauss in a vulgar error of thought, accusing him of dismissing the correlation between patrilineality and the matrilinear cross-cousin marriage posited by Homans and Schneider, as a statistical coincidence. Such an interpretation of their findings would indeed be incorrect, since it is precisely this factor of coincidence that they try to exclude by also subjecting the complement of the correlation concerned, viz. a connection between matrilineality and the patrilinear cross-cousin marriage, to a statistical test.

The truth of the matter is that, in the footnote Köbben refers to, Lévi-Strauss most decidedly does not confuse correlation with coincidence. On the contrary, he even considers it likely that *at the empirical level* each of the two modes of marriage correlates with one of the two modes of descent. He says that both correlations, if they can clearly be shown to exist, require an explanation, and he proceeds to give a hypothetical one. In a somewhat condensed argument, one which is therefore difficult to follow, he nevertheless urges that care be taken in proclaiming the existence of these correlations, for they can easily be obscured by the coincidence factor. The presence of this factor could mean that "la corrélation invoquée par mes critiques *serait vide de signification*" (my italics). In this connection he also draws attention to the fact that Murdock, in his much more comprehensive study (564 communities) of the correlations concerned, questions their theoretical interpretation on statistical grounds. Köbben does not seem to have given sufficient thought to why Lévi-Strauss establishes *no* (logical) connection between unilateral cross-cousin marriage and modes of unilineal descent while at the same time he *does* find it likely that there is a (statistical) connection between the two phenomena. Lévi-Strauss does not put forward the latter in order to rescue the former from Homans and Schneider, as Köbben incorrectly surmises. He does it because he realises that norms and a logical connection between norms cannot be explained by their (degree of) application. Moreover, he knows that discontinuity and discrepancy can occur between the structuring activities of the human mind and the empirical reality in which those activities manifest themselves.

So empirical data yield at most an indication of the direction in which we should seek an explanation for the occurrence of a phenomenon and are also useful in tracing *empirical* (not to be confused with logical) connections between phenomena. It should be borne in mind,

however, that correlations can also lead the investigator on to the wrong track in his search for an explanation, as is apparent from the error made by Homans and Schneider. It is no accident that they, too, finally reach for the reality behind the reality for their explanation of the preference for unilateral cross-cousin marriage, though I am entirely in agreement with Köbben when he says that their motivation is rooted in a somewhat rudimentary psychology.

As I have already made clear in a wider context, I am fully aware of the fact that Homans and Schneider also operate with a theory. I stated then that the difference between them and Lévi-Strauss is not analogous with that between induction and deduction.

Finally, Köbben is of the opinion that the structuralists claim "to be able to characterize a structure (culture) on the basis of one or a few 'fundamental principles', i.e. one arrangement (system, configuration)". While rejecting some of the differences I pointed out, he sees in this claim a basic contrast with his own method. But the quotation reveals a caricatural view of the argument; it is as though the structural approach were the spiritual child of a (mé)salliance between Ruth Benedict and Claude Lévi-Strauss. Structuralists do not believe that a society or a culture, even a primitive one, has one single structure which can be characterized with the aid of one simple arrangement. Using separate models, Lévi-Strauss even goes to a great deal of trouble to trace the structuring of various spheres of life, such as marriage, kinship, myth and religion. In his "Social Structure" (1953) he differentiates between a number of structures within a society and speaks of "the order of orders" (p. 547). Moreover, the structuralist does not seek a characterization or explanation of a society, but tries to find an explanation for structuring phenomena in a society.

The way in which Köbben illustrates the so-called claim of the structuralists shows, I am sorry to say, an equal lack of understanding of structural analysis, comparison and explanation. Though he is right when he says that a given society cannot adequately be typified on the grounds of its possessing either an irrigation system or a dualistic system, I would repeat that the structuralist is *not* concerned to arrive at such a characterization. Furthermore, the two systems are not at the same level of abstraction. A total social fact like dualism, which is found in a wide variety of concrete forms throughout the whole of a culture, is closer to the level of structuring principles under-

lying a socio-cultural system and is indeed, in this respect, more fundamental than an irrigation system, which, at least in its intrinsic characteristics, is more down to earth, more limited. But even here, it might be added, undeniably concrete as an irrigation system would seem to be, human inventiveness can sometimes prepare surprises for the student by converting the hard facts of irrigation into symbols. For a striking example of this see *Onvlee's* study (1949) of an irrigation system on East Sumba that is modelled on the system of . . . dualism and circulating connubium, both of which total social facts lend significance and arrangement to the society, the culture, concerned.

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