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Structuralism versus comparative functionalism; some comments

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STRUCTURALISM VERSUS COMPARATIVE FUNCTIONALISM; SOME COMMENTS

Dr. Pouwer has done me the honour of devoting a rather lengthy discussion to some papers of mine. He opposes their methodology, as being comparative functionalistic, to the structuralism of which he himself is an adherent. About three points in particular I should like to make some observations.

First point

According to Pouwer, comparative functionalism looks only for relations whereas structuralism seeks to discover arrangement (configurations, structures). I disagree. As an example of the structuralist method Pouwer recommends the study by J. P. B. de Josselin de Jong about the couvade. The latter mentions three main themes in the couvade which occur in different combinations and varying emphasis in the various societies. Compare with this my article on prophetic movements which Pouwer discusses (Köbben, 1964: 95-96, 102, 115, 120, 128, 131, 135); I list six main themes which are found with varying emphasis and in different arrangements (one or more of these themes may, however, be absent in certain movements). I content that this is precisely the same procedure. As to my discussion of cargo cults he suggests (p. 137) that it is because of methodological shortcomings that I cannot indicate what specific configuration makes the situation become "critical". This, however, is not so. I think nobody, from whatever specific point of view he works, is able to make this out!

Does the foregoing mean that I am satisfied with the way arrangement is taken into account in these and similar studies? No, for, when I say that a certain theme preponderates in a particular prophetic movement, my statement is largely based on unprovable impressions. I would be glad to have objective standards at my disposal by which to measure the "intensity" of such a theme, but so far I have not succeeded in finding such standards.¹ Poor comfort, then, to realize

¹ Cf also Köbben (forthcoming).

that the structuralists are no better off . . . although it seems to worry them less than it does me.

Second point

The comparative functionalist method, says Pouwer (p. 140) emphasises behaviour and situation; the structuralist method is especially concerned with representations. Let me observe in the first place that "my" method does investigate representations as well. This may be illustrated again from my paper on prophetic movements: my classification of prophetic movements into types is based on differences in representations (of the better world the prophets try to bring about through their movements).

But it is true there is a difference in emphasis here: Pouwer attaches more significance to representations than I do, at least judging by p. 142 where he speaks about representations as being "the agents of actions and situations". I wonder, incidentally, whether this almost Hegelian point of view is shared by all structuralists. Personally, I feel no need to proclaim the priority either of representations or of "situations" (social relations). I even consider it dangerous to do so.

Representations do interest me . . . but only, I may add, if I can be sure they really exist in the minds of the people I study and are not merely pictures in the head of the anthropologist. And I cannot say I am quite easy about what Pouwer says on this score. If we believe that all human beings have the same logic, he argues (see p. 135-6), we may infer that certain representations are present in their minds (in this case about lines of descent) even if they are not manifest.² Because the anthropologist sees these lines of descent, he seems to think, those people must see them too. It may be objected straightaway that even if the *processes* of the mind are the same for all human groups the *products* of the mind are not necessarily also the same. But more important is that Pouwer's reasoning takes him in a bold leap outside the sphere of science. Empirical science, according to the agreed notions, consists of *making statements that are, in principle, susceptible to testing*. Thus: "I postulate that where A is present, B is also present. I then determine empirically whether this is true. If it is so, I may maintain my thesis, if it is not, it must be rejected or at least altered or supplemented".

² I have taken due note of Pouwer's confession that he is not sure all human beings think in the same way. For the purposes of my argument, however, this is irrelevant. If they do think in the same way, so Pouwer evidently believes, they cannot but see these lines of descent.

But Pouwer argues thus: "I postulate that A and B occur together. And even if I find no sign of B, I still insist that B is there . . . though only non-manifestly" (whatever that may be). This standpoint opens the door wide to arbitrary reasoning. I am, therefore, happier about what P.E. de Josselin de Jong says on this subject (quoted by Pouwer on p. 135): "let us consider whether these *latent* lines become *manifest* in some way or other". That they do not become manifest is just what constitutes Kloos' objection. Unlike Pouwer, therefore, I am not so sure that Kloos and de Josselin de Jong misunderstood one another.

In this connection I also object to Pouwer's representation of the discussion between Lévi-Strauss and Homans and Schneider. According to Lévi-Strauss' theory about unilateral cross-cousin marriage, there is no necessary relation between this phenomenon and linearity. About Homans and Schneider Pouwer says: (p. 134) "They reason in the opposite direction. Placing themselves on the plane of empirical reality they establish a statistically significant link between matrilinear cross-cousin marriage/patrilinearity and patrilinear cross-cousin marriage/matrilinearity".

In actual fact Homans and Schneider also start off with a theory about unilateral cross-cousin marriage, only theirs is a different theory. Then they argue as follows: if Lévi-Strauss is right, there will be no statistically significant correlation between the two phenomena in question, if we are right, there will be such a correlation. With this postulate they turn towards empiricism for a test. Note that they do so *after* having developed their theory: real induction does not exist. It then appears that their results tally with their theory and not with Lévi-Strauss.

Does this mean that their theory should be accepted wholesale? Certainly not, but anyone who wishes to oppose Homans and Schneider will have to do one of three things: a. demonstrate that their classification of data is erroneous on a large scale; b. show, by a large number of new cases, that their results are due to chance; c. provide a new theory offering an explanation for Homans and Schneider's statistical results that is at least as adequate as their own theory. But the opponent cannot simply disregard their results. If he does, he merely places himself outside the sphere of science.

Pouwer points out that Lévi-Strauss does give an explanation for the statistical relation between the two phenomena in question. An explanation in fact, which saves his theory. Unfortunately, however,

that explanation is due to a logical error³ — even Homer nods sometimes.

To be frank I must add I agree with Pouwer that *factually*, with respect to the ethnographic data, there is quite a bit to be said against Homans and Schneider; *theoretically*, too: their theory is based on a rather shallow sort of psychology. But *methodologically* no objection can be made!

Third point

The structuralists claim they can characterize a structure (a culture) by a restricted number of “fundamental principles”, or an arrangement (system, configuration) (Pouwer p. 138). Except perhaps for some micro-societies, however, it seems to me this claim cannot be sustained. If someone wishes to regard this point of view as an essential difference between the comparative functionalist method and that of the structuralists, I agree.

Unintentionally, Pouwer himself shows that such a “total” approach is not really possible (p. 138). He points out that irrigation-societies *à la* Wittfogel are found both in dualistic and in non-dualistic cultures. Quite true! We might add that dualistic systems, in turn, are found in societies *with* as well as in societies *without* irrigation. In other words, if I say: “this society has irrigation (on a large scale)” I give an important piece of information about that society, but I do not thereby characterize it adequately. And similarly if I say: “this society

³ Homans and Schneider’s (1955: 34) results are as follows:

	Patrilineal	Matrilineal
Matrilat. cc. marriage preferred	22	4
Patrilat. cc. marriage preferred	2	5

Lévi-Strauss (1958: 344) explains this result in this way: patrilinearity occurs more frequently than matrilinearity; matrilinear more frequently than patrilinear cross-cousin marriage. No wonder, then, that the combination of patrilinearity and matrilinear cross-cousin marriage also occurs most frequently. It is true that in his next paragraph he suggests a different explanation that is at least not contrary to simple statistical theory.

has a dualistic system" I also give an important piece of information about the society without again characterizing it adequately.

At this point Pouwer will perhaps object that he is not concerned with just any element, but with the *fundamental principles* of a society. What, however, should be called fundamental? This brings us to a weak point in the approach of some structuralists: they *call* fundamental what they *believe* to be fundamental, and their beliefs are rarely founded on objective grounds. In the example given above: why should the fact that a society has largescale irrigation, with all its economic and social consequences, be less "fundamental" than a dualistic structure? Or vice-versa for that matter. There are certain facts that should induce us to caution in such questions. The society of Bali has mostly been described, by Dutch anthropologists, as a dualistic system. When the American anthropologist Geertz went to Bali he did in fact see elements that pointed towards something dualistic, but he felt no compulsion to describe this society in terms of dualism. He even considers that this notion has been detrimental to our understanding of Bali culture. (Geertz 1961: 498-502). That is to say, he does not consider fundamental what his predecessors believed to be so.

This is also the purport of my remark against R. Benedict (Köbben, 1964: 22-23). Pouwer says that I misinterpret her, but I think not. I quite realize that her "total" does not signify "all", but surely it does mean what is fundamental, determinative in a society. Otherwise the word has no sense at all. In my opinion, however, the term "Dionysian" is no such determinative characterization. Therefore, when she compares the "Dionysian" character of the Kwakiutl with the "Apollonian" character of the Zuni, she is comparing elements and not total structures — assuming that the terms in question are at all usable.⁴

A final remark. In my opinion, then, the differences between comparative functionalists and structuralists, as postulated by Pouwer, are partly at least, only apparent differences.⁵ Partly, too, they are indeed real. Nevertheless in practice collaboration between adherents of the

⁴ Which does not mean, of course, that I wish to deny the presence in a society of certain themes which may manifest themselves in quite different aspects of that society.

⁵ There are other alleged differences which I pass over in silence, so Pouwer's contention (p. 139) that comparative functionalism involves only inter- and no intra-cultural comparisons.

two 'schools' has proved possible and fertile. In the social sciences, we have arrived past the stage where the different schools could do no more than oppose each other; it seems we are growing adult.⁶

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⁶ Translation: Mrs. M. J. van de Vathorst.