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Criticism? A rejoinder to professor Fischer (Zie nr. 1430)


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be only a fragile construction, built up by a weak datum of tribe A, which itself is supported by an absolutely dubious fact of tribe B, while both again have to prop up other questionable data found among still other peoples, the whole being tied up by the extremely weak yarns of the interpretation of myths.

H. TH. FISCHER.

CRITICISM?

A REJOINDER TO PROFESSOR FISCHER

Professor Fischer finds so many things wrong with my article on head-hunting that I hardly know how to begin answering him. Perhaps the simplest thing to do, however, would be to take up his criticisms one by one in the order in which he makes them.

Professor Fischer first objects to my rejecting earlier theories on head-hunting as inadequate without discussing them at any length. My only answer to this is that I was not writing a critique of past theories on the subject, but was presenting what I felt to be a new approach to it. Professor Fischer believes, however, that I have slighted other writers in so doing and asserts that what I have said of them “cannot possibly be maintained if one really knows what is written about the subject”. But since he doesn’t say which theories I have neglected or misrepresented I cannot very well argue with him on this point. My use of the word “rationalistic” was, perhaps, a little obscure, but all I meant by it was that the theories so characterized explain the custom on the basis of some property or other peculiar to heads which makes them desirable, assuming that the people who hunted heads were inspired by the same rationalistic motives. These theories would correspond, thus, to the “intellectual” interpretation of magic, as analyzed by Evans-Pritchard ¹).

Professor Fischer next finds that to speak of putting head-hunting into its proper perspective constitutes a “strange metaphor”, but why he does not say, nor can I imagine. However, more important to

¹) E. E. Evans-Pritchard, The intellectualist (English) interpretation of magic, Bulletin of the Faculty of Arts, Univ. of Egypt, 1 (1933), p. 282.
Professor Fischer is the fact that I begin by promising to examine the custom among “several Indonesian peoples” and after having discussed the Toradja say that I will do it with respect to “a few other peoples in Indonesia”. He apparently finds some significant difference between “several” and “few”, but since “several” is defined in this usage as meaning “a few, more than two but not many” (Concise Oxford Dictionary) I can hardly be accused of having tried to mislead any one on that score.

More important to me is Professor Fischer’s following objection, as it involves a question of method. He makes it seem as if I had made up my mind about head-hunting on the basis of the Toradja material and had then “merely” tacked on a few facts concerning other peoples which might give a semblance of credibility to my thesis while ignoring all contrary evidence as of no interest. I gather in the first place that Professor Fischer finds it unscientific that I started from an idea gained from my study of the Toradja instead of examining all the Indonesian data with a blank mind and letting the facts speak for themselves. It is hard to believe, however, that anybody could maintain such a position nowadays. If the examination of data pertaining to one people suggests an explanation of an activity widely shared by related peoples, what could be more legitimate than to try to see if that explanation were applicable in the cases of those other peoples and if so to consider it of general validity in the area concerned? Professor Fischer then asserts that I “consulted” only a single source on Borneo and only used Van Wouded’s book for Timor, and further on (p. 5) states that I didn’t consult “the actually available literature” on any of the tribes discussed. Does he really think that this is a fair statement? Does he really expect one to quote from all the literature that one has consulted on any subject one is treating? However, I shall bring this up again below in connection with the myths, as Professor Fischer’s attitude stems from his failure to understand, or at any rate to accept, the purpose of my paper as a whole. The reason why I devoted so much more space to the Toradja than to the other peoples is obvious; there is far more data available on them than on the others, and I wished to give a reasonably typical example of head-hunting in some detail so as to make clear its general character.

For Professor Fischer, however, I was not even writing about head-hunting, but “social and economic dualism”, and he even goes so far as to assert that I used head-hunting to prove the existence of this form of organization and then proceeded to explain the former on the basis
of the latter. And to prove the correctness of his view he quotes from the conclusion of my paper to the effect that the two are related. This interpretation of my article is patently false. Throughout my exposition I endeavored to show a connection between head-hunting and dualism, and only at the end, having demonstrated, as I thought, the likelihood of such a connection, did I suggest that on that basis one might hope to use head-hunting as an aid in reconstructing the earlier forms of political and social organization in Indonesia, only to reject it on the grounds of insufficient data. Dualism is, moreover, such a common theme of Indonesian social life and thinking \(^2\) that Professor Fischer is hardly justified in speaking of a "hypothetical form of society" in this connection without any further elucidation.

Turning next to my account of the Toradja Professor Fischer is dissatisfied with the selection made from Kruyt's material. I cannot discuss this point with Professor Fischer, however, as he gives no examples of facts which he thinks I should have included. Kruyt devoted one hundred and thirty-three large-sized pages to this subject, and obviously in summarizing it much had to be left out, but I attempted to give as balanced a picture as possible of the activity as a whole without favoring those data which supported my hypothesis, for the reason given above.

Professor Fischer now comes to my analysis of the myths. The interpretation of myths is always precarious, he says, and mine in this instance is not clear to him, but he "will not dispute about the interpretation of myths". If this is the way he feels about it I cannot think why he has spent so much time on my paper, as the myths were after all basic to my approach to the problem. It was my opinion that an interpretation of the myths would offer the best chance of discovering a unifying theme behind all the diversity of rationalizations offered by the peoples themselves for the taking of heads \(^3\). The mythological material available, however, particularly pertaining to this custom, is for the most part poor and unsystematic, which made it impossible for me to amass anything like the amount of evidence that I would like to have or to prove my hypothesis with respect to all the head-hunting

\(^2\) Cf., for ex., J. P. B. de Josselin de Jong, De Maleische Archipel als ethno-
logisch studieveld, Leiden, 1935; F. A. E. van Wouden, Sociale structuurtypen in
de Groote Oost, Leiden, 1935; J. M. van der Kroef, Dualism and symbolic anti-

\(^3\) Volmering, for ex., listed 24 reasons for it in his article on head-hunting in
Indonesia. Th. Volmering, Het koppensnellen bij de volken in den Oost-Indischen
Archipel, Koloniaal Tijdschrift, 3 (1914), p. 1153, 1319, 1461.
peoples of Indonesia, as Professor Fischer would seem to expect. I agree that the interpretation of myths is precarious, but this is hardly a reason for not attempting it. Indeed Professor Fischer apparently once thought differently about the matter himself and even thought it possible "... to find therein by analysis the traces of old social and religious systems which form the substratum of those still in force".

As for Professor Fischer's next objection, that I suggested that dual organization had once existed in Borneo and Celebes without offering evidence for it, I can only say that judging from the indications which I did mention and from a general impression of the literature on these islands, in particular with respect to religious and cosmological conceptions, it seems likely to me that such was the case. However, to examine all the evidence for this would take so much space that I will not argue the point. As for the word "here", which I used on page 55 of my article, I willingly withdraw it. I would suggest, though, that it would take a very suspicious mind indeed to think that in using it I was trying to suggest the existence of a well-defined form of social dualism among the Toradja when I had already denied it.

Professor Fischer now examines my evidence for dualism among the various other peoples. In connection with the Padjii-Demon groups he quotes me to the effect that there is no definite statement associating the Demon with the Upperworld, though presumably they were. I assume that he does this to show that I arbitrarily attributed dualism to these peoples, but on the next page he considers it established, so I am not sure of his position on this point. At any rate, since the dualistic relationship between the Demon and Padjii groups is so clear, it does not seem to me very rash to assume when one group is definitely associated with the Underworld that the other is probably linked with the Upperworld.

As for Ceram, Professor Fischer apparently finds my exposition ridiculous on the face of it, only adding that it "seems exceedingly improbable" that "the creation myth, familiar in many parts of Indonesia... would have anything to do with head-hunting". On what grounds? And does he mean to say that there is only one "creation myth" shared by many peoples in Indonesia? The possibility that head-hunting should have something to do with creation myths does not

seem at first glance so remote. Professor Fischer thinks my conclusion that the Toradja practised head-hunting as a ritual for ensuring health and fertility is neither "new, nor surprising", and head-hunting was found to be associated with the creation myth of the Ngadju-Dajak. Moreover, on Ceram the maro dance is linked by three authors 5) with the custom, and the creation myth, in which this dance plays a central role, is recorded by two 6).

Professor Fischer's remarks on Timor need no comment, but those on Savu and Sumba do. He asserts, namely, that I "cannot mention social dualism in either of both islands". I failed to do so, but this does not mean that I cannot. The information on Savu is scarce, and I can only refer to Van Wouden 7), who discusses the available literature on that point, but there can be no doubt that dualism forms, or at least formed, one of the principal features of Sumbanese society. The older literature, examined by Van Wouden 8), makes this quite clear for West Sumba, and Onvlee gives a detailed modern account of dualism in the kingdom of Mangili in East Sumba 9). Professor Fischer could more reasonably have objected that the feuds I mentioned did not take place between established moieties. The kingdoms of West Sumba were divided into halves, but the latter were not reported to have engaged in mutual hostilities (though one myth speaks of a traditional antagonism between the halves of the original populations of Laura, Wajewa, Lauli and Lamboja 10). There is very little information, however, on the traditional relationships between the various kingdoms, which formed the warring parties, though a tradition reported by Kruyt 11) indicates that relations of a religious order at least once existed between several of them. It seems, namely, that every so many years a girl without family was raised by the people of Lewata in Wajewa to serve as a sacrifice for several other kingdoms in western and central Sumba. She was waylaid by arrangement outside Lewata by men from three of these, beheaded, and various parts of her body

6) See my article, p. 55, note 45.
8) Ibid., p. 26ff., 142.
9) L. Onvlee, Naar aanleiding van de stuwdam in Mangili (opmerkingen over de sociale structuur van Oost-Soemba), this periodical, 105 (1949), p. 445.
11) A. C. Kruyt, De Soembanenzen, this periodical, 78 (1922), p. 541ff.
were distributed among the participating kingdoms, to be used there-
after in rituals devoted to the tribal ancestors (marapu).

Professor Fischer, however, not only denies dualism to the Savunese
and Sumbanese, but to most of the peoples whose mock combats
I described, and in support of this applies to me remarks made by
Lowie in criticizing Smith and Perry. Perry, in his *Children of the
Sun*, began with the dualist organization as found in Egypt and every
trace of duality he found elsewhere he took as evidence of the earlier
existence there of the original form, which, he assumed, must have
spread from Egypt. Lowie's criticism was, thus, in this instance
justified, but I do not equate dual organization with "the division of
a society into intermarrying halves", nor, for that matter, with any
specific institution whose earlier existence in Indonesia I was trying
to prove. I gather, however, that Professor Fischer does conceive of it
in that light, which would account for his extreme reluctance to accept
its occurrence in Indonesia. Dual organization can best be regarded,
I believe, as Lévi-Strauss has recently interpreted it 12):

"Mais si l’organisation dualiste n’atteint qu’exceptionnellement le
stade de l’institution, elle tient cependant aux mêmes racines psycholo-
ques et logiques que toutes ces formes frustes ou partielles, simples
ébauches parfois, qui sont, au même titre qu’elle (bien que pas toujours
aussi systématiquement), des mises en forme du principe de réciprocité.
L’organisation dualiste n’est donc pas, d’abord, une institution; si nous
voulions l’interpréter de la sorte, nous serions condamnés à rechercher
sans espoir où elle commence et où elle finit, et nous risquerions d’être
rejetés vers l’atomisme et le nominalisme de Lowie. C’est, avant tout,
un principe d’organisation, susceptible de recevoir des applications très
diverses, et surtout plus ou moins poussées”.

In conclusion I must express my disappointment at Professor
Fischer’s reaction to my article. From one whose knowledge of Indo-
nesian ethnography is as wide as his I had hoped for some discussion
of the hypothesis I had suggested with reference to ethnographic data.
Instead he has refused to discuss it or the method it entailed.

R. E. DOWNS.

Discussie gesloten. — Redactie.

12) C. Lévi-Strauss, Les structures élémentaires de la parenté, Paris, 1949,
p. 95.