

## SECOND REPLY TO PFEFFER

Nothing in Pfeffer's latest set of remarks leads me to withdraw or change what I have said previously, though obviously some further clarification is called for. This is partly to deal with the extra points he has made, and partly because his attempts 'to avoid all ambiguities' now enable us to see a little more clearly the reasoning behind some of his objections and the slender basis of them all.

*Dumont.* It is astonishing that Pfeffer goes on talking about misrepresentation in the face of the passages from Dumont's text I had cited. His own new quotation makes no difference: it has long been clear to me that Dumont was not arguing for the identity of north and south India but simply searching for what similarities there were between the two areas; a legitimate exercise, though one bristling with difficulties. The tension this involved is revealed on the very first page of Dumont's article: 'I shall not be able to define a North Indian kinship system, *but I shall try to express the similarities between North and South as a common, pan-Indian pattern*' (1966: 90; my emphasis). There are many similar passages in the same paper. In the conclusion (*ibid.*: 113), one key point is partly italicized: 'it [the common pattern] consists essentially, if not perhaps exclusively, in the *valuation, and in the consequent elaboration and ordering or patterning, of affinal relationships*'. Far from being withdrawn in the paper of 1975, this phrase is quoted as something which 'is now widely acknowledged' (Dumont 1975: 197). Although Sylvia Vatuk had, in Dumont's own words, 'made light of my differences between north and south' (*ibid.*: 198), what Dumont identifies as his 'radical mistake' (*ibid.*) is not, as Pfeffer would have it, his 1966 comparison of these two regions but a detail of his analysis, dating originally from 1962, of the Gorakhpur terminology. One might infer from this that the former too was faulty, but this is certainly not made explicit. In fact, it is only much later (1983: 23) that Dumont finally appears to feel that this invalidates the comparison he had attempted. But only in part, it seems, since even this withdrawal is restricted to the terminologies, i.e. to just one aspect of the original comparison.

Pfeffer cannot have it both ways: either I have misrepresented Dumont by attributing to him what he did not say, or there really was something amiss; otherwise, what was the point of Dumont's withdrawal, however partial, which Pfeffer is at such pains to stress? I should add that I have no hostility towards Dumont himself, nor towards his work on India; on the contrary, I think he has done more for the anthropology of the area than any other scholar. But this is one respect in which I cannot follow him, nor it seems would he himself any longer entirely desire one to.

*Juang.* Although I did not give the Juang marriage rules as reported by McDougal in my first reply, they appear in my original *JASO* article (p. 256), where I properly record *saliray* (*inter alia* eGEyZ) as the main category from which a wife should be taken. Although it is not the only such category, McDougal makes clear its priority in terms of affinal alliance, both through explicit statements and statistically (1963: 157, 162; 1964: 331). Pfeffer, on the other

hand, prefers to emphasize different terms, *na* and *bokosini*, especially their second-cousin specifications (p. 154, above), glossing over the fact (despite his reference to the +2 level) that they are also PM and CD respectively (McDougal 1963: 162). Focusing on them supports Pfeffer's own hypothesis that the Juang have a four-line, Aranda-type system (criticized briefly in my earlier reply, pp. 60-61). However, as McDougal also makes clear (*ibid.*; 1964: 331), the statistical significance of such specifications as potential marriage partners is actually very low. This far from exhausts the problems with Pfeffer's hypothesis, a detailed discussion of which is now available (Parkin 1993), but the passage above is particularly tendentious.

It should be obvious that my use of the word 'reflected', which also appears in the sentence following that cited by Pfeffer, is not accorded any explanatory force but is used normatively in both cases. I have never associated myself with the view that terminologies are dependent for their form on other factors of a social-structural kind.

As for Pfeffer's main objection, the three-generation rule is, as he admits, described in the literature on a number of groups, namely the Munda, Santal, Korcu, Maler, Mal Pahariya, Hill Bhuiya and Hill Kharia (discussed with references in Parkin 1992: ch. 8, appendix II), to which we can now add the Sora (Vitebsky 1993: 38, 43, 48, 185). The failure of ethnographers working on other groups to report such rules certainly has to be treated with caution, but I eventually came to realize that many unclarities in their work could only be explained by taking the rules into account. This also applies to the Juang, though to a lesser extent, thanks to McDougal's very full and detailed ethnography.

What seems to happen in most of these groups is that alliances should not be repeated before the passage of three generations, which we might describe as a negative rule. Only in one case, the Munda, is there any suggestion that alliances should be repeated after three generations have elapsed, i.e. that they have a positive rule (see Yamada 1970: 385). The connection of the latter with positive marriage rules in the usual sense of the term is unclear, but I should be inclined to consider systems with a positive rule, such as that indicated by Yamada, as closer to prescription than those with a purely negative one. In some other cases—and the Juang is one of them—evidence that alliances are directed by kin term may indicate the existence of a positive rule of delay, but we cannot be certain. However, we can be considerably more confident about the existence of negative rules, among the Juang and elsewhere. I certainly agree with Pfeffer that McDougal does not use the word 'rule' himself (despite what Pfeffer appears to think, I have never said that McDougal does). However, I do not feel this affects the issue, given the weight of other evidence he produces. Here yet again, therefore, is a brief review of that evidence as I see it, which for the sake of variety I present in terms slightly different from those I have used before.

McDougal himself makes it clear that, at least when seen from the point of view of lower-order segments, generational delays in the immediate repetition of alliances are in operation here (e.g. 1963: 158, 168-9). Terminological conflicts

arise if alliances repeat those of the previous generation (*ibid.*: 159). McDougal is not specific about the number of generations. In a table in my book (1992: 176, table 2), I actually left open the possibility that it was *one*, which would suggest repetition of the marriages of the +2, i.e. grandparental generation. However, I expressed a preference for *three* in the text (*ibid.*: 171), a preference I continue to hold for the following reasons.

First, there are clear statements by McDougal himself (1963: 159 n.1) that the delay of a single generation is in no sense a norm, and indeed that it is actually usually more:

there is no special propensity for marriage choices to be related to [i.e. to repeat] those of close agnates belonging to the second ascending generation.... The marriage choices of persons in any generation are inclined to be related to one another, but unrelated to those formed by close agnates of all previous generations.

Secondly, only 10% of marriages in McDougal's sample repeated those of the FF (*ibid.*: 160). For these two reasons alone, therefore, the indications are that the normal delay is longer than one generation. Thirdly, however, a two-generation delay, i.e. the repetition of the marriages of the FFF, would entail a six-section system with marriages between adjacent generations (the so-called 'Ambyn' system; cf. Dumont 1983: 206-8). Whatever its feasibility in a general sense, this would offend against at least one norm of the Juang system, which is that marriages between adjacent generations should not take place; besides, the Juang do not have six nor any other number of sections. This makes four generations or any other even number equally unlikely. Fourthly, a greater number than three would require greater local knowledge about previous alliances than is likely to be possible. Fifthly, the comparative ethnography of the area suggests three as the upper limit. For all these reasons, three is indicated as the strongest possibility in the Juang case.

Pfeffer's reluctance to accept this may be connected with the fact that his own hypothesis of a four-line, 'Aranda' system requires a *one*-generation rule of delay. A delay of any greater number of generations, whether norm or rule, would have the disadvantage of further undermining a hypothesis that is already quite unacceptable on terminological grounds (Parkin 1993: 326-9). Yet it is precisely a delay of this magnitude that McDougal's evidence indicates. Neither his failure to write down the word 'rule' nor the fact that all rules, including Juang ones, are broken on occasion can be considered material objections here.

It should also be made clear that, when it comes to the evolutionary hypotheses I have been putting forward, it is the existence of a delay in renewing alliances, not the actual number of generations involved or the existence of a positive rule stipulating precisely when they should be renewed, that is significant in distinguishing systems like the Juang from prescriptive South Indian on the one hand and non-prescriptive North Indian on the other. In the last resort, my evolutionary hypothesis does not even depend on the Juang—all the necessary

evidence is available from other groups. Even if Pfeffer does return from his forthcoming field trip with the information that the Juang have no positive rule of this sort, nothing material would change in this respect. In fact, I should be inclined to welcome such information, since it would provide further if minor support for my longstanding contention that the affinal alliance systems of many of these groups are transitional between prescriptive and non-prescriptive, and therefore that they provide a historical link between south and north India.

Conversely, Pfeffer's own hypothesis that the Juang have a four-line, 'Aranda' system would benefit from a positive rule of some sort, since this would provide at least a marginal indication that this was a fully prescriptive system, as the hypothesis requires. Whether this would affect my misgivings concerning that hypothesis is another matter, given that the terminology as recorded by McDougal has no diagnostic features of a four-line system (Parkin 1993: 326-9). Alternatively, Pfeffer might find that there is no rule or norm of delay, even in the negative sense. This could mean one of two things: either that there is no direction at all to the formation of alliances; or that one is expected to renew alliances with the same alliance group in the immediately following generation, which if followed consistently would produce a system based on first cross-cousin marriage. Either outcome would make everyone look silly, since neither bears any relation to existing Juang ethnography, whether McDougal's, Pfeffer's or anyone else's (and by extension, of course, nor to my attempts at interpreting it).

*Munda.* The term 'Munda' is of Sanskrit origin and therefore not original in any sense to Austroasiatic speakers, although it has come to be used by one tribe as an alternative to their own term 'Horo' (i.e. Roy's group; cf. Pfeffer above, p. 154; also Parkin 1990: 17, 23). Having been applied first by administrators and then by early ethnographers, it was taken over by linguists as a designation for the whole branch of Austroasiatic to which the language of this tribe belongs. These are quite radical shifts in meaning, especially the second, though it is generally accepted even by anthropologists, including, it seems, Pfeffer himself. My use of the term simply entails a further extension, which I carefully described and justified in my main comparative work, together with other modifications made in the interests of avoiding confusion between the various possible uses of the term *as they already existed* (Parkin *ibid.*).

Whatever objections might be raised against such well-meaning modifications, given my numerous explicit disclaimers (cf. my previous reply, p. 56), the suggestion that I have been guilty of reification or linguistic determinism as a result is absurd. On the contrary, differences as well as similarities between Munda groups are recognized where appropriate in all my work, however condensed its presentation. Pfeffer's objection depends on eliding the various meanings of the word 'group', though I should have thought a phrase like 'a group of tribes' could not possibly be confused with the idea of a socially bounded whole. What Pfeffer dismisses as 'Parkin's group' (p. 156, above) is clearly the former, as the sentence of mine he quotes in his first paragraph makes clear. In the latter sense, I intend it simply as shorthand for 'ethnic group', a formulation

I actually chose—I did not make this clear at the time—because it had the advantage of avoiding another potential misunderstanding, namely the sometimes derogatory connotations, elsewhere if not in India, of the term ‘tribe’.

There are, of course, cogent reasons for concentrating on a particular language family when comparing kinship terminologies in a historical sense, given that these are aspects of language and that linguistic relationships have a historical aspect. This is one of the things I am interested in, and it explains the way I chose to present this material in the relevant chapter of my book (1992: ch. 7; cf. Pfeffer’s objection above, p. 153 n.1). To pursue the matter of my forms of presentation further, while I sometimes produce composite descriptions to save space, I do not indulge in ‘mixing ethnographic bits and pieces’ (p. 153 above). On the contrary, in analysis, where it really matters, I am careful to treat each group separately to begin with, only then seeing what correlations (and, of course, differences) might emerge. Chapters 4, 8 and 10 of my book (1992), dealing with respectively descent, affinal alliance and reincarnation, are exemplary in this regard.

*Malto.* I am the author of the phrase ‘terminology poised between the last stages of prescription and individualizing North Indian’, as should have been clear from the original context. The reason for concentrating on just the +1 level of the Malto terminology has already been given in my earlier reply (p. 56). As to the only other real query of Pfeffer’s here, the fact that Sarkar gives terminologies from six different villages, with considerable variation in form, is, I think, a not unreasonable basis for assuming that there may be some dialectal variation, to which unwritten languages are particularly prone. However, it is not something I am going to insist on, especially since it would actually strengthen the argument from redundancy if purely dialectal variations could be eliminated as an alternative explanation.

For the rest, Pfeffer’s basic concern seems to be to heap praise on Sarkar for his abilities as an ethnographer—which I do not remember ever having doubted—and to complain that he personally has been put to an unnecessary degree of inconvenience in tracking down my original sources. I fail to see why it should have been quite so difficult: the two earlier articles of mine that discuss this matter (Parkin 1990: 74; 1992: 276 n.2) contain all the information necessary for my sources to be traced. At all events, Pfeffer obviously managed to locate them in the end. Yet to what purpose? No material challenge to my hypothesis has resulted from all this trouble.

*Jat.* The significance of Tiemann’s article on the Jat (1970) is that he shows how the four-*got* rule, found there and elsewhere in north India, actually works. It was never part of my purpose in referring to it to suggest either that the rule was restricted to the Jat (hence the use of the phrase ‘the Jat and other groups of north India’ in my original article (p. 258)), or that the statistical tendency for lineages to renew alliances with one another in the longer term was a feature of north India generally, a conclusion which would certainly be premature.

*Companionship.* It is clear from the original context that the phrase about changes from prescriptive to cognatic (the typology is Needham’s) is intended to

be a general point on the evolution of terminological patterns. FB = MB equations are, of course, cognatic, and I was not suggesting that they were to be associated with South Asia. I pointed out at the end of the previous paragraph that north Indian terminologies are non-cognatic, in which respect, if not in others, they resemble their South Indian counterparts. Perhaps Pfeffer would none the less be prepared to accept a South Asian example of 'terminological companionship' between equivalent female specifications, FeZ and MeZ. There is at least one example in the literature, for which we must return to the Malto terminology. My immediate source is actually Pfeffer himself (1982: 90), though he has taken it from a homegrown ethnography. And the author of that turns out to be none other than that 'meticulous reporter' Sarkar.

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