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Tribal Kinship in Central India

A Reply Article

Robert Parkin

Professor Georg Pfeffer has recently (2004) returned to his ideas concerning the nature of kinship systems of peoples in central India who are usually

characterized as “tribes” by both academic and legal convention. His basic aim in this new article is to reaffirm his longstanding claim (see Pfeffer 1982, 1983, 1985) that these systems are best interpreted as four-line systems of affinal alliance, for which the Aranda (or Arunta) system in Australia has long been the textbook example. Here he is seeking to reinforce this argument in a number of ways: 1) by presenting considerable kin-term data from twelve of these tribes; 2) by offering ostensibly new ways of diagramming these systems; 3) by directly comparing one of them, that of the Juang, with the Aranda as described by Spencer and Gillen nearly eighty years ago (1927); and 4) by referring to the as yet largely unpublished findings of a cohort of students and colleagues who have recently been working in the area. Unfortunately, this new cornucopia of evidence and presentational techniques does no more than expose even more graphically than before the impossibility of Pfeffer’s basic hypothesis. In the brief discussion below, I shall restrict myself to the first, second, and third of these points, before introducing some field data of my own which points in a totally different direction for the tribe that has been most central to this debate, namely the Juang. I shall mostly refrain from simply regurgitating arguments I have made before in this context (but cf. Parkin 1993a, 1993b), since they are not substantially affected by Pfeffer’s latest article.

Throughout the article, if intermittently, Pfeffer appears to treat me as his main antagonist – not without reason. Indeed, the reader should be alerted to the fact that the ongoing exchange between Pfeffer and myself has as its background the breakdown in our personal and professional relationship that he himself alludes to (2004: 388, n. 30). Given Pfeffer’s decision to reopen the dispute, I beg the reader’s indulgence to reply to this aspect of it here too. Pfeffer’s account of the circumstances of this breach is broadly correct, with the addition that it was brought about means of a personal letter he wrote to me accusing me of a variety of professional misdemeanours, including most particularly plagiarism, that I had allegedly committed in a paper I gave to a conference on transformations in kinship in Moscow in spring 1992. Since the letter did not make it at all clear whom I was supposed to have plagiarised, at the time I assumed it was himself. Now for the first time I infer that it was actually Sasanka Sarkar, an obscure but worthy Indian anthropologist writing in the 1930s. In the conference paper that Pfeffer mentions, I had presented a general argument backed up by specific but minimal data, including Sarkar’s, and with a

minimum of references. It was generally but not universally known at this very small conference – including by Pfeffer himself – that I had not done fieldwork in India at the time and, therefore, could not possibly have collected the data myself, nor did I ever claim that I had. My decision to publish the paper subsequently in unrevised form (Parkin 1992a), thus giving Pfeffer an opportunity to reply in print (Pfeffer 1993a, 1993b), was stimulated by the accusations made against me, coupled with the desire not to deceive him by altering the original conference paper. I decided to go on and defend my scholarly reputation by making the interpretative differences between us on these topics crystal clear in two articles devoted to critiques of his work on kinship in central India (Parkin 1993a) and Germanic languages (Parkin 1993b) respectively. That was more or less the end of the matter until Pfeffer's recent article (2004).

I start with Pfeffer's new diagrams. As in earlier versions, those relating to Indian tribes all lack a full set of terms for the hypothetical third and fourth lines of the terminologies they represent. Those terms that are in these lines do not belong there, since 1) they mostly also appear in lines one and two, quite unlike the Aranda model; and 2) the genealogical specifications for them would mostly actually appear in lines one and two only in the same model. In short, the distribution of terms is again totally unlike the Aranda model in its crucial diagnostic aspects, and they do not sort themselves into the appropriate terminological or descent lines.¹ Although the diagrams look novel, therefore, they do nothing to support Pfeffer's basic argument.

If we now turn to the kin terms listed in Pfeffer's appendix (2004: 405–407), most of which have been collected by Pfeffer himself – though the Juang are not represented – even a cursory examination reveals none of the distinctive equations or distinctions of an Aranda four-line scheme. In particular, there are no terms for second cousins, cross or parallel, of ego's own generation, even disguised by their theoretical affinal equivalents. One would certainly expect this if this were truly a four-line scheme as conventionally defined, since it is precisely these categories whom the model envisages ego marrying (cf. here Korn 1973: chapters 4, 6). Nor is the crucial distinction between such cousins and first cross-cousins made any-

where.² Given Pfeffer's now extensive experience of collecting kin terms from Indian tribes, I find it impossible to believe that he or his students and colleagues would not have uncovered specific terms for second cross-cousins had they been present there. How, therefore, can he generate an Aranda system in their absence?

In principle, Pfeffer may have some other four-line scheme in mind, but this is surely ruled out by the very comparison between Juang and Aranda that he presents, quite deliberately, as evidence. Another possibility is that, like Lévi-Strauss frequently before him, he is not clearly distinguishing between terminology and actual or ideal patterns of alliances. But not only is there no evidence of such practices among any of these tribes as a regular feature, it has long been conventional to define prescriptive systems on the basis of the terminologies (cf. Needham 1973). On this basis, there is nothing specifically "Aranda-like" here.

A further consideration, one I myself missed in my previous exchanges with Pfeffer, is that there is structural regularity about the Aranda model which is generally absent in central Indian systems. The Aranda model is one in which each terminological line conducts alliances with two other lines in strict rotation, generation after generation. The necessity for four lines is, of course, derived from the symmetry of the system: the line that ego's line is avoiding in marriage in any particular generation must have another line to marry into in that same generation. This does not seem to be the case in central India, where the usual requirement instead is simply to *avoid* repeating the alliances of any one generation in the succeeding one. This is commonly accompanied by the possibility of repeating or intensifying alliances between groups of siblings in the same generation. In this respect this resembles cross-cousin marriage, but without the repetition of such exchanges generation after generation, and without the genealogical links with previous generations that appear in such a model. This necessitates the dispersal of the alliances of any one spouse-exchange group among a number of such groups.

This is nonetheless what seems to have led Pfeffer to think of a four-line scheme in the first place. In reality the tribal system appears to reflect the impact of upper-caste values in north and central India, which are opposed to any form of cousin

1 Pfeffer briefly discusses this concept (2004: 383 and n. 8).

Like him, I realise that these lines have no necessary connection with actual descent modes in the societies concerned.

2 In fact, all these terminologies seem to fit patterns that are already familiar in this part of India: there is nothing novel about them in this ethnographic context.

marriage, cross as well as parallel. The specifications that McDougal gives for ego's possible marriage partners in the Juang system are elder brother's wife's younger sister and elder's sister's husband's younger sister (for a male ego; there are other structurally equivalent possibilities). This is found elsewhere in the region too and can be generalized to sibling's spouse's sibling (GEG) categories.³

However, it is rare for such categories to be identified terminologically as such in central India, let alone prescribed as marriage partners. Even where they are, as may the case in some tribes, including the Juang, this is very different from the Aranda prescription for the category that includes second cross-cousins (MMBDC, MFZDC, FMBSC, FFZSC) and cannot be reduced to them. Although I agree with Pfeffer that the central Indian systems should be treated as fundamentally *sui generis*, especially historically, as already noted I would attribute this particular feature to the cultural influence of upper-caste north Indian norms against any form of cousin marriage: as I have long argued, central India is clearly a transitional area between south and north India when it comes to kinship. In addition, in some north Indian castes, this dislike produces a similarly irregular dispersal of the alliances of any one spouse-exchange group (here, one must often talk rather of individual families), and again with some tendency towards the repetition of the marriages of one's siblings in the same generation. This is coupled with an additional dislike of marrying complete strangers, which often leads one back to previous affines in the long term, but not those of the immediately previous generation, in seeking alliances – something that is also found in many tribes. But there is nothing prescriptive about this in north Indian caste society, and certainly none of the regularity of the formal Aranda model.⁴

In fact, however, both Pfeffer and I may be barking up the wrong tree, at least as far as the Juang are concerned – the central example of central Indian tribal kinship for both of us. In discussing this tribe, we have both been relying almost exclusively on McDougal's thesis (1963a), which presents the Juang system of affinal alliance as involving the exchange of spouses between groups of siblings as described above. More re-

cently (1998), however, in a brief visit to the same general area that McDougal worked in, my research assistants and I were told in village after village that, while one could marry GEG categories, this was quite rare, much rarer than marrying MBD (the matrilateral cross-cousin). McDougal, by contrast, placed cross-cousin marriage low in the scale of Juang preferences, though in a contemporaneous article he said that the Juang pursued "prescriptive symmetric alliance" (1963b). We could find no term isolating MBC, who are equated with elder siblings among the Juang (cf. Pfeffer 2004: 398, n. 56). My trip was brief and the conditions for data collection not ideal,⁵ but the consistency of replies from village to village was impressive.

Asymmetric preferences of this sort have been reported from two other tribes in this general area, namely the Santal of Santal Parganas and the Ho,⁶ both for second cousins through matrilateral ties (for a male ego): again, in neither case is a term reported identifying, much less isolating these categories. In addition, in neither case are the genealogically defined preferences those associated with four-line prescriptive systems, namely MMBDCs, MFZDC, FMBSC and FFZSC, who are all structurally equivalent in the sense that they occupy the same category in the model of such a system (see Parkin 1997: 87 ff.). In the Ho case, the specification is MFBSD marrying FFBDS, in the Santal case MMBSD marrying FFZDS (in the Aranda system, the Santal preferences would marry not ego, but ego's first cross-cousins). Finally, both preferences appear to be part of a more general preference for matrilateral cross-cousins, basically in line with what I was told by the Juang in 1998.

If these reports on the Ho and Santal and my own field data are correct, then two things follow.

5 In particular, the data were collected in Oriya from Juang-speaking informants by my English-speaking assistants. Assuming the latter were not deceived, or consistently mistranslating the data, then either McDougal was wrong or the system has changed in the near half century since he was in the area. Why it should have changed is not at present clear. In the Santal case discussed by me next in the text, Gautam says "the social standing of such marriages is also enhanced by the fact that they are not disliked by the neighbouring Hindus" (quoted in Parkin 1992b: 161), presumably because of the status difference between wife-givers and wife-takers. However, the superior status of wife-givers in most asymmetric systems of affinal alliance is rare in India outside the northeast and the Himalayas – quite the reverse is the case, in fact, given the well-known ideology of hypergamy (cf. Parkin 1990).

6 Discussion and references at Parkin (1992b: 153–161).

3 See Parkin (1992b: ch. 8) for a more extended discussion of and references for the issues discussed in this and the next two paragraphs.

4 An overview of kinship in the caste society in north India, including these aspects, can be found at Parkin (2001: 135 ff.).

First, there is considerable slippage between terminology and marriage practice among these tribes, with an asymmetric preference but a basically symmetric kinship terminology. Secondly, there is even less of an Aranda four-line scheme in evidence here. If followed regularly, MBC marriage would allow the repetition of a spouse-exchange group's alliances in the immediately following generation, and indeed would formally require it at the model level. There would be no formal need for the dispersal of a group's alliances among several other groups (although this might happen), and while there would be a third terminological line, there would be no need for a fourth.

It will be interesting to review the new data on central Indian kin terms that Pfeffer promises us, courtesy of his students and colleagues. Until then I see no reason to revise my view that terminologies like the Juang represent a modified form of two-line symmetric prescriptive, and that there is still no evidence at all for Pfeffer's interpretation of them as four-line symmetric prescriptive.

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History of Anthropology in The Netherlands

A Review Article

Martin Ramstedt

The narratives contained in the two volumes of "Tales from Academia. History of Anthropology in The Netherlands"¹ are heterogeneous, yet complementary, "tales from Dutch academia" as regards the development of anthropology – and non-Western sociology, for that matter – in The Netherlands. The institutional histories of these two disciplines in The Netherlands have been closely intertwined, due to the specific sociopolitical context in which they have evolved, and because of a strong overlap in research focus. Unfortunately, only anthropology is mentioned in the title. For the insider, this neglect confirms the recent marginalization of non-Western sociology in The Netherlands to the benefit of anthropology. Yet, it is the institutional and intellectual entanglement of anthropology and non-Western sociology in The Netherlands that – to my mind – justifies the publication of the two volumes with the Verlag für Entwicklungspolitik in Saarbrücken, specializing in development studies.

The first volume, Part 1, presents "Trends and Traditions" within the institutional and intellectual

¹ Vermeulen, Han, and Jean Kommers (eds.): Tales from Academia. History of Anthropology in the Netherlands; 2 parts. Saarbrücken: Verlag für Entwicklungspolitik, 2002. 1132 pp. ISBN 3-88156-763-1; 3-88156-764-X. (Nijmegen Studies in Development and Cultural Change, 39/40) Price: € 39,00; € 38,00.