

COMMENT

PARKIN'S EVIDENCE

These further and final remarks on Robert Parkin's *JASO* article (Vol. XXIII, no. 3, pp. 253-62) and on his reply (Vol. XXIV, no. 1, pp. 54-63) to my comment (Vol. XXIV, no. 1, pp. 49-54) will only reinforce my previous points about his form of evidence and add two further short points. More examples of Parkin's methods can be found in his original article and in his recent book (Parkin 1992). Having been accused by Parkin of unclear wording ('Reply', p. 55n.), I shall try to avoid all ambiguities.

1. The passage in Parkin's original article that reads, 'comparing like with unlike, for example northern address terminologies with southern reference terminologies in Dumont's attempt (1966) to prove that the former were as classificatory as the latter' (p. 253) is a misrepresentation. Implicitly and explicitly—'the vocabulary...has no "structure" in the strict sense'; 'we shall...call our system descriptive'—the very opposite was elaborated by Dumont (1966: 96), who anyway withdrew this contribution—'I came to acknowledge that I had made a radical mistake' (Dumont 1975: 198)—seventeen years before Parkin chose to criticize it.

2. Parkin's thesis depends essentially upon ethnographic data on the Juang marriage system. He writes of 'the rule of delay of three generations' (p. 256). This 'rule' is Parkin's invention. Several ethnographers, besides myself in my own primary ethnographic research, have discovered such a rule among *other* tribes *but not among the Juang*. Parkin's constant mixing of ethnographic bits and pieces relating to several quite different tribes does encourage confusion,¹ but in the present case he has explicitly named McDougal (1963, 1964) as his source, even though he carefully avoided a more detailed reference in the original article. None of the references in Parkin's reply (p. 55) indicates a 'rule of delay of three generations', which is supposed to constitute Parkin's 'Juang-type system' in his crucial 'third stage' of kinship evolution in South Asia. What exactly did McDougal's dissertation report?

The fifth chapter, on 'The Marriage System', has an initial section headed 'Marriage Rules and Preferences', which Parkin has chosen not to consult for his reply. It informs us of two—and only two—such rules: 'the first rule is that a man must marry a female belonging to a group which his own classifies as *bondhu*', i.e. as hereditary affines; 'the second rule divides the *bondhu* category into halves, one prescribed and the other prohibited. A man must marry a female belonging to his generation-set—his own generation or an even-numbered one' (McDougal 1963:

1. As far as I can see, none of Parkin's publications contains a formal analysis of a single terminological system as a whole that would be applicable to a particular middle Indian tribe.

155). As examples, the ethnographer explicitly names 'five...kin-types from which a spouse may be taken', and among these are 'na (e.g. FMBSD) and bokosini (e.g. FFZSD)' (ibid.: 157), i.e. members of the affinal group already allied with ego's in the *second ascending*, the grandparental generation. I am not sure if marriage 'rules of delay' could be stated in any clearer wording, and I fail to understand why Parkin continues to defend his contrafactual account. In the present context it is unimportant whether Juang marriage rules are 'therefore reflected in the terminology', as Parkin (p. 256) pointed out, even though in his reply he could not conceive 'how any sort of reflectionism' could be attributed to him (p. 57).

The Juang do not just exist in written sources, the people themselves can provide evidence of their rules. They can be reached from any European airport within forty-eight hours. The passage and a two-month stay in a Juang village costs less than required by the most modest standard of living in Western Europe. Altogether I have spent eighteen months in the middle Indian hills visiting most of the tribal areas in excursions (1972, 1978, 1980/81, 1982, 1983, 1987, 1988, 1990) lasting between two weeks and six months. Most of this time has been spent in the Phulbani and Koraput Districts of Orissa. On my next middle Indian excursion this coming winter I could visit the Juang in the company of established anthropologists from Bihar, West Bengal or Orissa in order to re-examine—for the third time—the Juang marriage rules as described by McDougal. Parkin must have had good reasons for avoiding travelling to South Asia so far, but he could be very helpful by suggesting the conditions under which he would accept ethnographic data from the primary accounts of other authors.

3. On many occasions Parkin has reified the linguistic category 'Munda' into a social group named 'Munda' (subdivided into such similar subgroups as 'the North Munda' and 'the Koraput Munda'). In 1985 he defined 'the Munda' as 'a group of tribes living principally in southern Bihar and Orissa, and speaking languages which are unrelated to Dravidian or Indo-European' (Parkin 1985: 705). The accompanying figure (ibid.: 706) introduces 'the Munda language family (after Zide 1969: 412)'. Parkin also complains that 'virtually no comparative work has been done on them as a group outside the realm of linguistics' (ibid.: 705).

The latter statement is correct. Apart from Parkin *no* anthropologist—not to speak of the tribals themselves—has ever been aware of Parkin's 'group'. Since the pioneering work of Sarat Chandra Roy on *The Mundas and their Country* (1912), anthropological usage has confined the sociological term 'Munda' to a group of this name, nowadays registered as a 'scheduled tribe' on the lists of several Indian states. Historians and administrators have discussed the well-known actions of this tribe in the nineteenth century, just as innumerable Indian anthropological reports have described the group itself. But middle Indian ethnography is poorly represented in Europe and the USA, and 'Parkin's group' has thus been able to survive in the most respectable journals.

Similar phantom groups have attracted previous scholars. In South Asia 'the Aryans' are the most popular such group. For decades Leach (1990) could not demolish the concept of 'Aryan invasions' some millenniums ago. But the people

Parkin classifies as 'the Munda' can be personally consulted. The Gadaba of Koraput (who speak a Munda language) would not normally know of the existence of such tribes as the Munda, Santal, Kharia or Juang (who also speak Munda languages) as no form of communication is maintained. But most Gadaba identify themselves as part of the Desya or indigenous people of Koraput, who share totemic and status categories across the tribal boundaries between Jodia, Rona, Kondh and Ollari people, i.e. speakers of Indo-European or Dravidian languages. In the north, the Juang (with a Munda language) are seen as the 'junior siblings' of the Bhuiya, who speak an Indo-European language. Similar dual classification across the boundaries of the language family is found elsewhere.

The issue may be illustrated by an analogy. Anyone could define all the speakers of Germanic languages (including Swedish, Flemish and English) as 'the Germans' and write books and articles on their 'social organization' that would deal with such similar 'subgroups' as 'the Scandinavian Germans' and 'the West Germans' in Belgium, the Netherlands and Britain. Describing such a social universe would imply that, apart from the linguistic commonalities, these 'Germans' shared a unique social criterion when compared to, for example, the Finns, Walloons or the Welsh. But I am not so sure that the anthropological public or the people themselves would readily accept such a classification.

4. In both his original article in *JASO* (p. 259) and in another article in another journal (1990: 73) Parkin has introduced primary ethnographic data without any reference to the ethnographers themselves. In the course of a general, evolutionist argument he has assigned these data to a tribe he called 'the Malto' (and nothing else) without further explanations. No other author of any type of literature has ever called this tribe 'the Malto'.

In his reply Parkin justifies these omissions as if his articles were not meant for a general readership: 'as for the complaint concerning absence of sources, these are given in full in Appendix II of my book (1992: 234-6; and in the notes, *ibid.*: 276), a manuscript copy of which Pfeffer has long possessed' (p. 56). Following this hint, I purchased a copy of Parkin's book and found there general references to the ethnographies of Sarkar (1933-4), Vidyarthi (1963), Bainbridge (1907-10) and Verma (1959) (Parkin 1992: 276), but *no* detailed indication of who was responsible for the specific data.² In particular, nobody was identified as the author of the 'terminology poised between the last stages of prescription and individualizing north Indian' that 'the Malto' were supposed to frequent as representatives of 'the fourth stage' in the evolutionary scheme for South Asia

2. In 1987 Parkin kindly gave me a copy of a manuscript entitled 'The Sons of Man: An Account of the Munda Tribes of Central India'. He seems to refer to this work now, but misses my point. I had always known that 'the Malto' did not exist, whereas other readers of his articles could hardly evaluate his evidence without being provided with a reference to the primary research and the correct name (Maler or Sauria Pahariya) of the tribe that was supposed to represent Parkin's 'fourth stage'. A lack of differentiation between primary, secondary and tertiary research is Parkin's general problem.

presented in Parkin's *JASO* article (p. 258). Having thus been forced to examine these sources in detail, the only account I have found that appears to fit is Sarkar's (1933-4: 257-9), though when compared to Parkin's table (p. 259), there are minor differences of spelling. The other sources differ or are irrelevant.

Sarkar was a meticulous reporter. His account introduced six different terminologies collected in six different villages among tribals he correctly introduced as 'the Malers'. The six terminological schemes offered the vocabulary for 54 kin types within each of them. Sarkar also wrote on language—'the Malers speak Malto' (1933-4: 151)—but Parkin omits this reference. Parkin also refers to important variations in meaning among five out of the eight Malto terms supplied, and attributes them to 'some dialects' (p. 259). But neither Sarkar nor any of the other sources inform us about 'some dialects'. Thus, instead of a reference to Sarkar's elaborate ethnographic work, Parkin represents the 'fourth stage' in the evolution of South Asian kinship by eight terms, of which five have a different meaning in 'some dialects' unknown to any of the ethnographers.

It would be helpful if Parkin could correct or confirm my researches on his ethnographic sources. His ambitious hypothesis requires careful study of the data base. Having accused several renowned authors of a 'uniform reliance on slender and suspect evidence' (Parkin 1990: 70), he could explain why it was so difficult to detect the sources of his data on 'the Malto', and why his selection of eight terms out of Sarkar's rich and informative ethnographic account should be the only basis of the hypothesized 'fourth stage' of the 'evolutionary paradigm...for south Asia'. I raise these questions because the 'system', i.e. the eight terms published by Parkin, is supposed to be 'especially significant for the overall hypothesis' (p. 258).

5. A positive aspect of Parkin's reply is the reunification of his 'fifth stage', i.e. 'the Jat system' (1992: 258) with his 'sixth and final stage', i.e. 'standard north Indian' (p. 259). He recognizes now that the 'four-got rule', as described by Tiemann (1970), has 'implications for the dispersal of alliances in north India generally (i.e. not just among the Jat)' (p. 62).

When I introduced Tiemann's ethnography in the course of my evolutionary speculations on the dispersal of alliances (Pfeffer 1983: 115; 1985: 179), I could not have anticipated Parkin's subsequent elaboration of a separate evolutionary stage for the north Indian subcaste named Jat, in contrast to a stage for the other people of north India. I selected Tiemann's account of the Jat (1970) as a primary source for the 'four-got rule', because his work was easily available to German (and other) readers. Had I known the consequences, I would have chosen Blunt (1931: 60-62) or any of the many older, ethnographically more general sources, as I had done before (Pfeffer 1970: 83).

6. Parkin's hypothesis of terminological evolution in South Asia proposes that prescriptive terminologies give way to cognatic ones. In several steps FB is supposed to 'switch...from the terminological companionship of father to that of MB' (p. 254). It would be helpful if Parkin could provide a single South Asian kinship vocabulary containing a 'terminological companionship' of FB and MB.

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