

## COMMENT

### DISPERSED ALLIANCE, TERMINOLOGICAL CHANGE AND EVIDENCE

The general problem of Parkin's recent article, 'Dispersed Alliance and Terminological Change in South Asia' (*JASO*, Vol. XXIII, no. 3, pp. 253-62), has been raised by its author in an earlier and related publication (Parkin 1990). There he discredited others for their 'uniform reliance on slender and suspect evidence' (*ibid.*: 70). These attacks against 'such figures as Barnett, Carter and above all Dumont' (p. 253) are now continued, so the question of 'evidence' should be discussed. Coincidences between Parkin's article and my own comparative ethnographic work in middle India (1982, 1983) will not, however, be elaborated in the following examples of 'evidence'.

1. Anyone who actually takes the trouble to read Dumont's contribution of 1966 will have to notice how Parkin misrepresents essential statements. Dumont has never attempted 'to prove' that northern address terminologies 'were as classificatory as' the southern reference terminologies (p. 253). He has, moreover, repeatedly (1975: 198; 1983: 23, 160) renounced his publication of 1966. Parkin's article simply ignores this radical shift.

2. The most relevant ethnographic data in Parkin's article relate to 'the rule of delay of three generations' (p. 256) among the middle Indian Juang. Thus the 'Juang-type system' is born, representing the crucial 'third stage' (p. 258) of the overall scheme. In fact, these data are just a good example of invented ethnography. Neither McDougal (1963) nor any other ethnographer of the Juang has ever reported such a 'rule'.

3. Who are 'the Malto' (p. 258)? In almost all his previous publications Parkin reified categories designed by linguistic experts (e.g. 'the Munda' or 'the Koraput Munda') as if they were social groups whose social norms and categories differed from those of speakers of other languages within the village, tribe or region where these linguistic categories could be applied. In a kind of escalation, in this latest article (and its predecessor) Parkin has now reified a language—Malto—into a social group when, it seems, the tribe called Sauriya Pahariya by officials and Maler by its members is meant. By this technique it will be impossible to detect other 'evidence' on 'the Malto', since the fairly large number of ethnographers of the Sauriya Pahariya use the correct name. Thus the uninformed reader is likely to think that the data on the group rechristened 'the Malto'—including the Table, or caricature of a kinship terminology (p. 259)—were the result of the author's primary ethnographic research. No evidence of the real sources is supplied. Since (after Parkin 1990), this is the second attempt to bestow reality upon a hitherto unknown tribe called 'the Malto', we can presume that evidence is created by repetition.

I will not bother further about South Asian ethnography here but concentrate my other comments on 'typology and transformation', i.e. the theme of the Moscow conference at which the paper on which Parkin's essay is based was originally given. My own work on 'dispersal' or *Zerstreuung* (1985: 178) of invariant ties of intermarriage in South Asia will not be introduced, even though it deals with the by now familiar Juang and Jat, north, south and central Indian as well as Omaha marriage rules, and the equation, in contrast to the differentiation, of MB and WF. It was written in a foreign language, too difficult to be checked. Only those colleagues I had invited as guests to the Berlin Institut für Ethnologie had been forced to accept copies of the paper.

Carrying coals to Newcastle may be a better idea. More than two decades ago, a prominent representative of Oxford anthropology gave the following verdict:

What I wish to propose therefore is quite seriously that this kind of typology, i.e. one in which the types are defined by isolated features of named societies arbitrarily selected as paradigm cases, should be entirely abandoned. It is methodologically faulty, it misdirects research, and it has served no useful purpose. (Needham 1971: 16-17)

The substance of Parkin's article is an example of such a type of typology, so little else need be said. Furthermore, it advocates a particular path of evolution among the terminological types. The *lineal* will be ultimately transformed into the *cognatic* type that is identical to the pattern of our own vocabulary of kinship. The shift is supposed to take time. FB is said to 'switch...from the terminological companionship of father to that of MB', but not in one step (p. 254). The hop, skip and jump of his performance are described as intermediary evolutionary stages.

The nineteenth-century evolutionists had also seen our own pattern of kinship vocabulary in the ultimate stage of their scheme. In comparison with Parkin's article, I prefer their work, because they do not isolate kinship as the only domain. In contrast, the 'diachronic approach' of Parkin's article elaborates historical transformations of marriage rules and terminological patterns unrelated to either the technological and economic sector or the political and administrative systems, not to speak of such sociocultural values as status. Had the scheme referred to these features, the diachronic sequence attained in the Table opposite would have been the outcome. The rather unusual sequence of developments on the side of the political economy, ordered according to the historical transformations in the field of kinship, would require some further explanation of the 'diachronic approach' advocated by Parkin.

My own evolutionary speculations of about a decade ago (1982: 97) refer to the tribal pattern as initial. The structure of terminology and alliance may be described as the repetition of intermarriage in every second or fourth generation. Such a highly sophisticated scheme would go together with the simple political economy of swidden cultivators as witnessed today among the Juang and others. In the course of diachronic change, the growing sophistication of the political

TABLE 1: Kinship Terminology and Alliance versus Values and Political Economy<sup>1</sup>

	<i>Kinship Terminologies and Alliance</i>	<i>Values and Political Economy</i>
Stage 2	'the Dravidian system we are all familiar with, from Morgan onwards' (p. 258)	caste order plus sophisticated government and economy based on plough cultivation
Stage 3	'the Juang-type system, which retains some prescriptive features' (p. 258)	tribal order in acephalous society and simple economy based on swidden cultivation
Stage 4	'the Malto...poised between the last vestiges of prescription and individualizing north Indian' (p. 258)	same as Stage 3
Stage 5	'the Jat system, terminologically north Indian, with the four- <i>got</i> rule ensuring dispersal' (p. 258-9)	same as Stage 2
Stage 6	'standard north Indian terminologically with no particular tendency to renew alliances' (p. 259)	same as Stage 2

---

economy would coincide with alternative schemes of simplification of terminology and alliance: *either* affinal prescription would be simplified and then imply *immediate* repetition of intermarriage, as is well known in south India today; *or* the distance between alliances of different generations would lead to an individualizing scheme, as is well known in north India today, implying the end of affinal prescription and alliance by terminological and normative prohibitions of repetition and reversal of marriage ties. *Both* the simplifications could be traced back formally to the sophisticated tribal pattern.

1. I deliberately leave aside Stage 1 or 'tetradic society' (Allen 1986) since this scheme belongs to an analytical level altogether different from empirical cases. Like this *pure* case, the Juang and Njul-Njul schemes alternate kinship categories in adjacent generations. In Allen's parsimonious model, the two-line scheme is retained, while in the Juang and Njul-Njul case alternate generation equations imply four terminological lines.

What do we compare when we compare 'kinship terminologies'? Parkin's article offers a straightforward reflectionist account ignoring all differences among societal types. A number of well-known Oxford and Paris analysts have, however, convinced myself and others of the value of a sceptical approach towards any given domain of 'kinship'. 'Kinship' differs, as societies differ. Some of these differences include the following principles:

1. A term we call 'kinship term' may relate ego to another individual and denote alter's kin type. Designating classes of kin types, the vocabulary may reflect the manner of ordering such individual relationships, as is the case in most European languages. Parkin generalizes the reflectionist approach, but many other authors will differentiate. Although common sense might assume that all 'kinship terminologies' depend upon egocentrically articulated references or addresses, common sense may not be the best of guides.<sup>2</sup>

2. In other societies, such as the north Indian one, ego may apply a term to a class of alters that is not exclusively designated by genealogical space. My informants in coastal Orissa, for example, referred to a person as *phupha* (FZH) by the logic of oppositions and *not* because of (non-existent) genealogical links. His daughter would be ego's 'sister' and ego's daughter's *phuphi* (FZ) etc. The simple reflectionist dogma does not apply but, at the same time, no other *overall* principle of order can be detected.

3. Prescriptive and classificatory equations (see Allen 1989: 176) may result in a *single unified system* of terms that is *not only* ordered by the egocentric perspective (see Dumont 1983: 5). An example of such a holistic system is 'the Dravidian system we are all familiar with from Morgan onwards', i.e. a system of two affinally interrelated terminological lines. Parkin ignores the order of the whole and the particular implications of holistic orders. He is only concerned with partial relationships and their transformations.

4. A fourth concept of a 'vocabulary of kinship' may offer the same type of system as the third one, but—in addition—relate the order of terms to orders of ceremonies, settlements, dances, groups etc., i.e. a general pattern of classification beyond the domain of 'kinship'.<sup>3</sup> The two-line order of the Garo in north-east India (Burling 1963: 348f) may serve as an example,<sup>4</sup> but anthropologists unfamiliar with South Asia may be better acquainted with the four-line system of the Njul-Njul (see Elkin 1956 [1938]: 70).

2. An as yet unpublished talk by Serge Tcherkézoff on terminological 'type' in Samoa (given at the same conference as the paper on which Parkin's article is based) has cautioned me against such generalizations.

3. This is, of course, a reference to Dumont's 'global formula of exchange' (Dumont 1971).

4. Essential is the holistic—and not the local—point of view. The terminological system of the Garo does *not* completely fit the marriage rules. I doubt if *any* empirical system permits such a complete fit.

The Njul-Njul alternate generation equations (see Allen 1989: 177) differ from those of the Juang, but the latter's overall terminological order also implies two alternating terminological lines of affines for the elaborated line of kin. Another term gives a hint of a fourth line that is logically implied by the system. Quite different from the Njul-Njul order, the Juang system contains within each of the three central generational levels expressions of relative seniority for both male and female affines and consanguines, but these additions can be accommodated without contradictions within the overall four-line structure (Pfeffer 1982: 63).

Quite irrespective of these details, on the evidence of his *JASO* article Parkin would evaluate the Njul-Njul order—or any 'Aranda-type system'—in the manner applied to the Juang case. The Njul-Njul scheme would be 'obviously intermediate' and 'the WB of one generation becomes, of course, the MB of the next, but his son does not succeed to his position: MBS is never WB, and MB never WF, hence the regular terminological separation of direct affines and cross kin...the Juang [and Njul-Njul] system is clearly transitional, a symmetric prescriptive system in process of decay, preserving original features in the +1 and -1 cognatic terminology, but losing these in ego's level and with separate affinal terms in all three medial levels' (pp. 256-7).

I disagree with such an interpretation. Njul-Njul and Juang apply a holistic terminological scheme that is truly structural and does not separate a domain of 'kinship' from other domains of social organization. Parkin fails to take notice of the social whole. He equates systems of symmetric prescription with two-line systems. The sophistications of four-line systems are, therefore, viewed as 'decay', or at best as 'transition'.

GEORG PFEFFER

#### REFERENCES

- ALLEN, N. J. 1986. 'Tetradic Theory: An Approach to Kinship', *JASO*, Vol. XVII, no.2, pp. 87-109.
- ... 1989. 'The Evolution of Kinship Terminologies', *Lingua*, Vol. LXXVII, pp. 173-85.
- BURLING, ROBBINS 1963. *Rengsanggri: Family and Kinship in a Garo Village*, Philadelphia: University of Pennsylvania Press.
- DUMONT, LOUIS 1966. 'Marriage in India: The Present State of the Question, III. North India in Relation to South India', *Contributions to Indian Sociology*, Vol. IX, pp. 90-114.
- ... 1971. *Introduction à deux théories d'anthropologie sociale: Groupes de filiation et alliance de mariage*, Paris: Mouton.
- ... 1975. 'Terminology and Prestations Revisited', *Contributions to Indian Sociology*, n.s., Vol. IX, no. 2, pp. 197-215.
- ... 1983. *Affinity as a Value: Marriage Alliance in South India, with Comparative Essays on Australia*, Chicago: University of Chicago Press.

- ELKIN, A. P. 1956 [1938]. *The Australian Aborigines: How to Understand Them*, Sydney: Angus and Robertson.
- MCDUGAL, CHARLES W. 1963. *The Social Structure of the Hill Juang*, University of New Mexico, Albuquerque: Ph.D. thesis.
- NEEDHAM, RODNEY 1971. 'Remarks on the Analysis of Kinship and Marriage', in his (ed.), *Rethinking Kinship and Marriage* (ASA Monographs 11), London: Tavistock, pp. 1-34.
- PARKIN, R. J. 1990. 'Terminology and Alliance in India: Tribal Systems and the North-South Problem', *Contributions to Indian Sociology*, n.s., Vol. XXIV, no. 1, pp. 61-76.
- PFEFFER, GEORG 1982. *Status and Affinity in Middle India*, Wiesbaden: Franz Steiner.
- ... 1983. 'Generation and Marriage in Middle India: The Evolutionary Potential of "Restricted Exchange"', *Contributions to Indian Sociology*, n.s., Vol. XVII, no. 1, pp. 87-121.
- ... 1985. 'Verwandtschaftssysteme der südasiatischen Regionen im Vergleich', in Hermann Kulke and Dietmar Rothermund (eds.), *Regionale Tradition in Südasiens*, Wiesbaden: Franz Steiner, pp. 171-90.

#### REPLY TO PFEFFER

My gratitude to Professor Pfeffer for his comments is eclipsed only by my astonishment at the confusion of thought that underlies practically all of them. I will start with the first of his objections, namely my supposed misinterpretation of Dumont. There is no question that Dumont introduces data from address usage at several points in his treatment of North Indian kinship terminology (1966: 96-103), which he initially sees as descriptive in type. For instance, he concludes his discussion of the term *bhai*, 'brother', which has a wide meaning extending far beyond the close genealogical specification implied by the English translation, by saying: 'If I am right, this is equivalent to leaving the descriptive scheme and using its categories, or some of them, *as if they were classificatory*. This tendency might well be indicative of Dravidian "contagion"; *it is clearly more developed in address than in reference*' (1966: 101; my emphases). And when discussing the treatment of ZH, FZH and MB, which he regards unambiguously as affines in the southern terminology, Dumont says that in the north too, the fact that they are addressed as *pahuna*, 'guest' (ibid.), means that they 'are actually treated as affines, but the terminology [i.e. the reference terminology] does not register the fact' (ibid.: 102).

I had acknowledged Dumont's own misgivings about the feasibility of this comparison on two previous occasions (1990: 69-70; 1992: 12), but chose in the present case to focus, not on his later change of heart, but on the doubts he expressed right at the outset: 'the postulate of the unity of India is seriously

challenged in the field of kinship' (1957: 18). This did not prevent him from going ahead with the attempt none the less—as, of course, he was perfectly entitled to do. Indeed, my own position was and remains one of applauding the aim but of finding fault with the approach. Thus I do not think I was being unduly unfair to Dumont in choosing this as the best and most famous example of the sort of difficulty I was talking about. At all events, I have not ignored the matter in my work generally, but merely found a different, and earlier, statement of Dumont's to cite.

As regards his second point, Pfeffer is evidently not quite so familiar with McDougal's writings as he thinks. McDougal's major work (1963) has plenty of references to the fact that marriage ties between spouse-exchange groups are discontinuous down the generations, in the sense that there are norms against the immediate repetition of alliances between the same alliance groups (*ibid.*: 158ff., 168, 174-5, 428, 429). This means that one cannot repeat the marriage of one's father, as with cross-cousin marriage; and other, statistical data recorded by McDougal (*ibid.*: 160) suggest that the marriage of one's FF is also avoided. This actually entails a three-generation rule of delay, since the two-generation rule it logically implies would come up against the strict opposition of alternate generations that is so crucial a feature of Juang life and thought: FFF is linked with one's father and therefore belongs to a genealogical level defined as adjacent to ego's, into which marriage is formally prohibited and whose own marriages ego does not repeat.

Such rules are by no means uncommon in middle India. They may sometimes be negative rather than positive, i.e. they impose a delay before realliances become possible, rather than actually enjoining repetition after the stipulated number of generations has passed. This does not stop them from being considered, or described as, rules. The Munda may even represent an instance of a positive rule, if a fairly explicit statement by Yamada (1970: 385) is anything to go by. Elsewhere in the area, the possible existence of such rules is supported by evidence of spouse-selection being directed by kin term (Munda, Korwa: *goi*; Santal: *sangat*; Juang: *salirae*) and, more circumstantially, of a rule against marrying anyone with whom *no* previous relationship can be traced (the Hill Kharia). The data assembled in my book (Parkin 1992: especially ch. 8 and appendix II) substantiates these matters fully, giving all the necessary references.

In fact, it would seem that Pfeffer is not terribly familiar with his own writings either. Two passages of his (1982: 97, para. 3; 1983a: 101, para. 3) can certainly be construed as accepting the existence of a three-generation rule among the Juang and other groups.<sup>1</sup> Other passages (especially 1982: 56-60) are still more specific in mentioning delays of three generations before alliances can be renewed in a number of different groups. Many of his statements refer explicitly

1. Though in neither instance is the wording entirely clear, as is often the case in Pfeffer's work. It is this, not the language in which it is written, that is the real reason for it sometimes being 'too difficult to be checked' (see above, p. 50).

to this practice as a 'rule', and two (ibid.: 57, on the Dangria Kond; ibid.: 60, on the Bhuiya) are based on his own field enquiries.

Pfeffer's third comment, on the reification of linguistic names as ethnic groups, has no basis in my own writings. Indeed, in the preface to my major work (1992: ix), I expressly deny any intention to argue for any sort of linguistic determinism. I accept at many points (ibid.: ix, 4, 217, 225-6, 234-9) that Munda speakers, like Austroasiatic speakers generally, share much with immediate neighbours who speak different languages and that they cannot really be distinguished from them on other grounds. The chosen topic of that work was the comparative ethnography of a group of middle Indian peoples who all speak a Munda language, and as such the word 'Munda' was bound to appear frequently in it. This simply means that the term 'Koraput Munda', for instance, refers to 'the peoples who speak languages of the Koraput Munda branch' or (which comes to the same thing) 'the Munda-speaking groups who live in Koraput district': it certainly does not represent any sort of reification.

As for the term 'Malto', my employment of it in the article in question is admittedly looser than it might have been. None the less, as a general principle its choice can be readily justified in the present context, where it is mainly kin terms and terminologies that are being discussed. Given the embodiment of terminologies in language, 'Malto', being the term for the language of the Maler, seems more natural here than the ethnonym itself, just as one would normally speak of the Latin, not the Roman, kinship terminology (not to mention the Sanskrit, Hindi etc. terminologies). 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. It is hard to see why Pfeffer should decide that my table is no more than a 'caricature' of a kinship terminology: it shows just what it purports to show, no more and no less, namely the transitional nature of the pattern in the parental (i.e. +1) level of two terminologies, the part that is most significant to the argument currently being pursued, being in each case that part where the last traces of prescription are to be found.

It is when Pfeffer goes on to complain about my treatment of kinship terminology in general, and to offer his own disquisition on the topic, that we begin to meet with real caricature. At one point (p. 52), he calls my study a 'reflectionist account ignoring all differences among societal types', his train of thought apparently being that because, in his view, there is no separable domain of kinship, one has to anchor one's study of it firmly to other sociological factors. This is an opposite use of the word 'reflectionist' to the one with which I am familiar. In my experience, it is usually applied precisely to those explanations for kinship terminologies that see them as epiphenomena of other social facts, such as (depending on the writer) descent or marriage systems, behavioural stereotypes, or status. McKinley, for instance, argues:

[the reflectionist tendency] assumes that the *only* way to demonstrate a relation between kinship terminology and features of social organisation is by locating the



points at which the terminology appears to be a direct reflection of some aspect of the social or behavioural system. In other words, a certain amount of one-to-one mapping or congruence is expected between terminology and social structure. Usually the terminology is treated as the dependent side of this relationship. (1971: 239-30; original emphasis)<sup>2</sup>

This careful definition might be called the restricted view. Under certain circumstances, the term 'reflectionist' might equally be applied to correlations with societal type of the sort that Pfeffer appears to be advocating. The debate is an old one, going right back to Morgan and one of his earliest critics, Kroeber. The latter, in a famous paper of 1909, was the first to advocate the treatment of kinship vocabularies in their own right, as autonomous systems of classification. This, in general, has always been my own tendency too. Indeed, I cannot conceive how any sort of reflectionism can be attributed to myself, either in this article, or in anything else I have ever written. My own position is and always has been that it is perfectly legitimate to restrict one's investigations to one particular aspect of what we call kinship; that if that aspect happens to be terminology (as is largely the case in my article), then to begin with it can be treated simply as a matter of classification; and that it is no more than prudent to concentrate initially on establishing the nature of the system before looking to see what correlations of a sociological nature *might* emerge from the data. I am certainly not hostile to attempts to establish correlations as such: I simply believe that the onus of proof lies on the author of them, and that they are much less easy to bring to fruition than is often realized. The major obvious exception is the possibility of correlating prescriptive terminologies with those systems of affinal alliance loosely but not entirely satisfactorily referred to as 'cross-cousin marriage'. While I accept the strictures of those who claim that such coherence, though logical, is strictly ethnographically contingent, there are certainly cases where the fit does obtain—including, in my opinion, the Juang and allied groups (as I define their systems). Yet not even this degree of recognition entails giving an existential priority to either kinship terminology or affinal alliance system, as a reflectionist approach would require.

In fact, Pfeffer's own attempts to set up a range of associations between kinship terminology and societal type stops just short of being reflectionist, since he does not go so far as to accord existential priorities to either. None the less, they bring him much nearer to such a position than anything in my work brings me. The correlations he suggests are considerably more ambitious than my own much more modest attempts to match—and only where ethnographically appropriate—affinal alliance system to terminology. I infer from the last paragraph on p. 50 that the intention of all this is to challenge the validity of the diachronic stages I had advocated for types of terminology by showing that the order does not correspond with his own evolutionary scheme of societal types. Whatever the

2. See also Barnes 1976: 385. Pfeffer seems to have understood the standard definition better on an earlier occasion (see his 1985: 701).

validity of this scheme as such, what it shows is simply the difficulties involved in establishing regular correlations between the two, difficulties that are severe enough to make most authorities content to assume that there are none. My diachronic suggestions, on the other hand, are based on transformations internal to the terminological data themselves that are both logical and, I believe, ethnographically justified. This is yet another reason for wanting to concentrate on terminology as a separate domain, a decision supported by the circumstance that a terminology is embedded in linguistic much more than in societal factors. Indeed, there is no evidence that I know of showing that kinship terminology differs comprehensively according to societal type. A simple example will suffice here. Pfeffer associates the 'Dravidian', i.e. South Indian terminological system, with 'caste order plus sophisticated government and economy based on plough cultivation' (p. 51). The doctoral thesis of one of his own students (Werth 1992) has shown that the wandering Vagri of south India have a terminology that is basically standard South Indian in type (though lexically Indo-European, not Dravidian). Yet they live by hunting, scavenging, begging, minor trade and such ritual services as healing, not at all by plough cultivation, and they can hardly be said to have 'sophisticated government'—whatever that is supposed to mean.

Pfeffer also appears to want to say (p. 52) that some terminologies are not only, or even not at all, egocentric, a view apparently based on the mistake of thinking that terminologies are egocentric because, or when, they chart genealogies (assuming this is what is meant by 'individual relationships'; cf. also the words 'genealogical space' in the following paragraph). Here, Pfeffer has got himself well and truly tangled up in an elementary confusion that has bedevilled anthropology for far too long. All terminologies are egocentric, simply because they are systems of classification centring around, i.e. seen from the perspective of, an imaginary person called ego. This has nothing to do with genealogy as such. Kin terms may delineate actual genealogical positions, and even particular analytical kin types, but they need not and frequently do not. What they always do, however, is to map out the indigenous categories ego uses for the people he recognizes as relatives.

These principles apply just as much to the English term *uncle* as to the Tamil term *maman*: an English ego may have several uncles, each representing a separate genealogical position and a separate kin type. It is only when this English ego wants to define the relationship of one particular uncle more exactly for some purpose that he or she resorts to genealogy. In my view, those alliance theorists who risk the wrath of the extensionist school in distinguishing non-Western terminologies from Western ones because the former use category rather than genealogy actually understate their case. The distinction is a false one, though the real reason for this is the opposite to the one usually given. All kinship terminologies deal in categories, and all peoples have ways of describing relationships more exactly than reference to category is always able to do. One example of the latter, though it is not the only one, is genealogy. The distinction

is thus not one between types of society but between sorts of knowledge, i.e. between the contexts in and for which relationships are identified.

Pfeffer none the less appears to think (p. 52), on the authority of Dumont, that a separate class of terminology is defined by the possession of a holistic perspective. Again, I would say that *all* terminologies are holistic, in the sense that they form a discrete semantic domain. What Dumont is actually doing in the place cited (1983: 5) is warning us against the mistake of taking the genealogically minimal definition of a term as its chief meaning and the classificatory referents of the term as mere extensions. The example he gives concerns the relationship between two classes of +1 males who are minimally defined as father and mother's brother in the typical Dravidian terminology. Its purpose is certainly to show that this is a relationship between two classes of affines (i.e. affines to one another) that in itself is structured not by their own individual relations to ego, but by their involvement in repeated affinal alliances with each other. However, none of this affects the circumstance that a kinship terminology is egocentric. Ego is also implicated in this arrangement, it clearly being Dumont's intention to argue that ego's relationship to the mother's brother is just as affinal as that between the father and mother's brother. Indeed, the entire analysis would lose much of its force were this not the case, given that ego's father and mother's brother will be affines to each other in any society, ours as much as among Tamils (this is true by definition, i.e. mother's brother will always be ego's father's wife's brother). Thus, with regard to the question of whether or not terminologies are egocentric, it matters not at all whether ego's relationship with the mother's brother is traced indigenously through the mother, exploiting links that are entirely consanguineal, or through the father, as mother's brother's affine. In either case, father and mother's brother are defined in themselves—like their classificatory equivalents, where appropriate, and even though all are subsumed under a common term—precisely by their relation to ego. They are respectively *ego's* father and mother's brother, or, in Tamil, *ego's appa* and *maman*, *not* each other's. Dumont himself says, in deliberate and direct contradiction of his own question as to whether his account entitles one 'to speak of a structure *sensu stricto*': 'but here lies the characteristic of a kinship terminology as compared with other kinship groupings, *that it is a constellation revolving around the Ego*' (1983: 12; my emphasis).<sup>3</sup>

This in no way contradicts the idea of a kinship terminology as a whole, i.e. a discrete semantic domain. But the notion should not be confused with other classifications that are sometimes found alongside the kinship terminology. Although there are frequently terms for whole classes of relations, such as affines, cognates, and patri- or matrikin, these are not part of the kinship terminology as such, which, in Dumont's famous phrase, 'has not as its function to register *groups*; it is on the contrary the basic fact of its nature that it universally ignores them' (1962: 92; original emphasis). Rather, they form a parallel classification

3. Here too, Pfeffer had more success earlier in interpreting Dumont correctly (see his 1985: 702).

supplementing, but not normally contradicting, the terminology. Labels for phratries, moieties and sections are an obvious example. However, their presence is strictly contingent: it was the respective presence and absence of sections that led Dumont to distinguish the Karia and Dravidian systems from each other as global and local forms of exchange (1983: ch. 5). In practice, it is mostly these, rather than kin terms, that one works with when attempting to establish correlations with 'orders of ceremonies, settlements, dances, groups etc.' (p. 52).

The Garo example given by Pfeffer proves, when examined, to have precious little to say about such things, Burling being mainly concerned on the pages cited with kinship terms and how they are used. Perhaps, as Pfeffer suggests, data on the Njul-Njul of Australia would prove more fruitful, though as I have never even mentioned them in print he is hardly entitled to say (p. 53) how I would treat their system. What is more remarkable about this part of Pfeffer's critique is his attribution of a four-line 'Aranda-type system' (p. 53)<sup>4</sup> to the Juang of central India, the main example of my article. I have already challenged this claim briefly in print (1992: 187). The point deserves developing at length, but suffice it to say here that the Juang terminology as recorded by McDougal (1964: 329) bears no relation to the hypothesis. One can find four terms in each of the three medial levels readily enough, but the specifications given for them do not correspond to a true four-line scheme: in particular, they are not joined by lineal descent to corresponding positions in the other levels. A number of terms are for affines only, another deleterious circumstance. Nor is any category of second cross cousin stipulated as alliance partner, as in most such systems. As already mentioned in my article, marriage partners among the Juang and similar groups are rather to be seen as GEG categories to each other. The vacuousness of Pfeffer's hypothesis is shown by the passage in which he says that the Juang's 'overall terminological order also *implies* two alternating terminological lines of affines for the elaborated line of kin. Another term gives a *hint* of a fourth line that is *logically implied* by the system' (p. 53; my emphases). Such language does not improve one's confidence in the sort of reasoning being employed, since all it demonstrates is a fondness for deep but hypothetical mental structures as a way of compensating for what is really a complete lack of evidence.

Here, then, is a fundamental disagreement over how an important and (thanks to McDougal's ethnography) well-described affinal alliance system is to be interpreted. Because he sees it as four-line prescriptive, and is therefore able to link it with a textbook scheme of kin classification, Pfeffer treats the Juang system as static. I, conversely, see it as closely related but by no means identical to the two-line symmetric prescriptive scheme of south India, and therefore as transitional between this and non-prescriptive North Indian. Pfeffer accepts the possibility of change between terminological types, but as regards India, he places the supposedly four-line system of the Juang not in any transitional position, but at the

4. I draw attention to this phrase because it offends against Needham's strictures as quoted so approvingly by Pfeffer three pages earlier.

evolutionary starting-point. Quite apart from the spuriousness of the four-line hypothesis, this would reverse the order conventionally assumed to be the most plausible, namely that it is two-line symmetric prescriptive systems, especially if they have extensive alternate generation equations, that have evolutionary priority over other forms (see Allen 1986 for the most developed account). Given all this, it is Pfeffer's notion of the South Indian pattern as a 'simplification' of the supposedly four-line systems of middle India (p. 51) that needs justifying.

Such disagreements, and the confusion and lack of clarity I frequently detect in Pfeffer's writings, account for my previous reluctance to put them to use in my own work, save to some extent ethnographically. This is important, because it provides an answer to Pfeffer's remaining point, to the effect that I have disregarded some prior claim he has on this material (pp. 49, 50).<sup>5</sup> I am happy to concede that Pfeffer has been actively studying middle Indian kinship for almost as long as I have. My own involvement dates back to the autumn of 1977, when I started intensive library research on the Munda. Pfeffer's earliest writings on tribal kinship would seem to derive mostly from two brief field trips to Orissa in 1980-81 and 1982, heavily supplemented by literature research that presumably preceded them in part.<sup>6</sup> It is also true that Pfeffer beat me into print on this topic by approximately a year: as far as I have been able to determine, his own first relevant publications are dated 1982, while mine are dated 1983.

There is, of course, a degree of convergence between our writings, but this is limited to generalities, including the existence of dispersed alliance in India. As regards the occurrence of this phenomenon among Munda speakers, my own views on the matter have certainly developed over the years, but I can point to some brief remarks in an article published in 1985 which, though they have come to seem unsatisfactory from many points of view, do show clear parallels with Pfeffer's paper (Pfeffer 1985) published the same year.<sup>7</sup> In my paper (1985: 716-17), I suggest that very few Munda-speaking groups have affinal alliance systems based on 'the direct exchange of women between alliance groups, repeated generation after generation', and that 'the north Indian preference for avoiding close kin in marriage seems to prevail'. And my very first published paper, on

5. One would not normally bicker over such matters. When this complaint originally arose, however, it was made so seriously that it led to my being expelled from a wing of an institute whose hospitality I had been enjoying and in which, indeed, I had even been allowed to teach on occasion. I hope the reader will therefore forgive me for taking this opportunity to clear my name of the charges of ethical misconduct then laid against me.

6. See Pfeffer 1983b: 649n. An earlier field trip to Orissa in 1971-2, undertaken as part of the Orissa Research Project into the Jagannath cult, led to Pfeffer's habilitation thesis (1976) and two other publications (see his 1978: 422 n.5 and *passim*; and 1982: 1 and Part I) on the Brahmins of Puri. It would appear that other members of the project had responsibility for the tribal dimension on that occasion, especially Hermann Kulke and Anncharlott Eschmann.

7. A revised account of my own paper can be found in my book (1992: ch. 7).

Lévi-Strauss's treatment in *Elementary Structures* of Austroasiatic-speaking groups, shows me grappling with the intricacies of these systems, not as yet entirely clear as to their nature, but at least recognizing the absence of cross-cousin marriage in many of them, with in its place rules of delay before alliances may be repeated (1983: 80-83). Both circumstances automatically entail the dispersal of alliances for any one affinal alliance group. It does begin to look as if Pfeffer and I have been working in parallel to some extent, at least as regards such general points.

Yet neither of us, I think, can really claim priority even here, nor as regards fleeting comparisons with so-called 'Crow-Omaha' systems. It was McDougal who originally worked out the Juang system, in his excellent thesis, thirty years old this year (1963). Parry introduced Crow-Omaha kinship into the question of affinal alliance in India back in 1979, clearly alluding to the system in Kangra as one with 'Crow-Omaha-type rules of exogamy' (1979: 312; the local rules are described at 221ff.) and linking such systems with 'the development of a pattern of repeated alliances between descent groups' (*ibid.*: 313); in other words, the people of Kangra can be said to have dispersed alliance mitigated by the clear possibility of renewal in the long term. And it is Tiemann, I would say, who deserves most credit for having worked out, now well over twenty years ago (1970), not only the nature of the four-*got* rule, but its implications for the dispersal of alliances in north India generally (i.e. not just among the Jat). Nor should Trautmann's extensive and able treatment of Indian affinal alliance and terminology be forgotten (1981).

I wonder if Professor Pfeffer has received as few complaints from these authorities as I have. If, by whatever chance, he has received any at all, it is certainly not my intention to add to them here. But at least these few remarks may be enough to indicate that, in putting forward a claim to precedence concerning these matters, he is simply making a mountain out of a molehill that was never entirely his to begin with.

ROBERT PARKIN

#### REFERENCES

- ALLEN, N. J. 1986. 'Tetradic Theory: An Approach to Kinship', *JASO*, Vol. XVII, no. 2, pp. 87-109.
- BARNES, R. H. 1976. 'Dispersed Alliance and the Prohibition of Marriage: Reconsideration of McKinley's Explanation of Crow-Omaha Terminologies', *Man*, n.s., Vol. XI, no. 3, pp. 384-99.
- DUMONT, LOUIS 1957. 'For a Sociology of India', *Contributions to Indian Sociology*, Vol. I, pp. 7-22.
- ... 1962. 'Marriage in India: The Present State of the Question, I. Marriage Alliance in South-East India and Ceylon', *Contributions to Indian Sociology*, Vol. V, pp. 75-95.

- DUMONT, LOUIS 1966. 'Marriage in India: The Present State of the Question, III. North India in Relation to South India', *Contributions to Indian Sociology*, Vol. IX, pp. 90-114.
- ... 1983. *Affinity as a Value: Marriage Alliance in South India, with Comparative Essays on Australia*, Chicago: University of Chicago Press.
- KROEBER, A. L. 1909. 'Classificatory Systems of Relationship', *Journal of the Royal Anthropological Institute*, Vol. XXXIX, pp. 77-84.
- MCDUGAL, CHARLES 1963. *The Social Structure of the Hill Juang*, Ann Arbor: University Microfilms.
- ... 1964. 'Juang Categories and Joking Relations', *Southwestern Journal of Anthropology*, Vol. XX, no. 4, pp. 319-45.
- MCKINLEY, ROBERT 1971. 'A Critique of the Reflectionist Theory of Kinship Terminology: The Crow/Omaha Case', *Man*, n.s., Vol. VI, no. 2, pp. 228-47.
- PARKIN, R. J. 1983. 'Lévi-Strauss and the Austro-Asiatics: Elementary Structures under the Microscope', *JASO*, Vol. XIV, no. 1, pp. 79-86.
- ... 1985. 'Munda Kinship Terminologies', *Man*, n.s., Vol. XX, no. 4, pp. 705-21.
- ... 1990. 'Terminology and Alliance in India: Tribal Systems and the North-South Problem', *Contributions to Indian Sociology*, n.s., Vol. XXIV, no. 1, pp. 61-76.
- ... 1992. *The Munda of Central India: An Account of their Social Organization*, New Delhi: Oxford University Press.
- PARRY, JONATHAN P. 1979. *Caste and Kinship in Kangra*, London: Routledge & Kegan Paul (International Library of Anthropology).
- PFEFFER, GEORG 1976. Puris Sasandörfer, Basis einer regionalen Elite, University of Heidelberg (South Asian Institute): Habilitation thesis.
- ... 1978. 'Puri's Vedic Brahmins: Continuity and Change in their Traditional Institutions', in Anncharlott Eschmann, Hermann Kulke and Gaya Charan Tripathi (eds.), *The Cult of Jagannath and the Regional Tradition of Orissa* (South Asia Institute, New Delhi Branch, Heidelberg University, South Asian Studies No. VIII), Delhi: Manohar, pp. 421-37.
- ... 1982. *Status and Affinity in Middle India*, Wiesbaden: Franz Steiner.
- ... 1983a. 'Generation and Marriage in Middle India: The Evolutionary Potential of "Restricted Exchange"', *Contributions to Indian Sociology*, n.s., Vol. XVII, no. 1, pp. 87-121.
- ... 1983b. 'Fehlinvestitionen der soziobiologischen Ethnographie und Mädchen-tötung in Indien', *Anthropos*, Vol. LXXVIII, nos. 5/6, pp. 649-60.
- ... 1985a. Review of Dumont 1983, *Anthropos*, Vol. LXXX, nos. 4-6, pp. 701-3.
- ... 1985b. 'Verwandtschaftssysteme der südasiatischen Regionem im Vergleich', in Hermann Kulke and Dietmar Rothermund (eds.), *Regionale Tradition in Südasiens*, Wiesbaden: Franz Steiner, pp. 171-90.
- TIEMANN, GÜNTER 1970. 'The Four-got-Rule among the Jat of Haryana in Northern India', *Anthropos*, Vol. LXV, nos. 1/2, pp. 166-77.
- TRAUTMANN, THOMAS R. 1981. *Dravidian Kinship* (Cambridge Studies in Social Anthropology 36), Cambridge: Cambridge University Press.
- WERTH, LUKAS 1992. Die Vagri: eine wandernde Gesellschaft Südindiens, Freie Universität, Berlin: PhD thesis.
- YAMADA, RYUJI 1970. *Cultural Formation of the Mundas*, Tokyo: Tokai University Press.