COMMENT ON GERMAN DZIEBEL

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Abstract*: This comment is directed critically at certain arguments made by German Dziebel concerning the derivation of Crow-Omaha terminologies. Dziebel asks why such features cannot be derived from alternate generation equations. It is shown that this would have to happen indirectly, if at all. Dziebel's difficulties with the mixing of cross and parallel and the place of bifurcate merging and collateral terminological features in this context are also commented on, it being argued that they are all perfectly compatible with Crow-Omaha and indeed regularly found with such terminologies.*

I am pleased to have this opportunity to write this response to German Dziebel's commentary, not least because I myself have been looking at the Crow-Omaha (C-O) question recently (in Part 2 of a collection of some of my past papers on kinship; see Parkin 2021). It is in relation to C-O in general that I have issues with some of Dziebel's interpretation; I do not feel the need to comment on his discussion of Indo-European kin terms, on which I also write in Ch. 6 of the above volume.

Dziebel's commentary is directed at Thomas Trautmann and Peter Whiteley's (2012) fairly recent edited volume, *Crow-Omaha: New light on a classic problem of kinship analysis* (henceforth T&W). Dziebel appears to have two main goals in his rather critical discussion of it. The first can be dealt with quite swiftly, as it basically consists of his complaint of T&W's neglect of his own previous and extensive work on kinship analysis generally, including in relation to C-O: although I have not checked the whole of T&W's volume, the index indicates that his work is indeed only cited once, by Nick Allen. This is despite the fact that, although originally in Russian, Dziebel's work had already been available in English for five years before T&W came out and three years before the advanced seminar on which it is based (cf. T&W's preface). One can sympathize, as I'm sure we have all suffered similar disappointments. Clearly the translation was necessitated by the fact that most western scholars of kinship

do not read Russian: ironically one who did was Nick Allen, though he originally learned it for recreational rather than scholarly reasons, and although he engaged with Michał Kryukov's work, that too was in English; I'm not aware that Allen ever used his Russian in his work on kinship.

Dziebel's second aim can be summed up as answering the basic question, why can't C-O terminologies be derived (Dziebel appears to mean directly, though this could be made clearer) from alternate generation equations (AGEs) of a sort postulated in Allen's tetradic model and its immediate evolutionary successors, and as empirically attested in a variety of terminologies across the world. The short answer is that they cannot; that is to say, while Dziebel may be able to find AGEs alongside C-O skewing in particular terminologies, this could have a number of reasons – for instance, to move a post-tetradic terminology away from prescription (see below on Sherpa) – and is very different from a hypothesis that C-O evolves directly from any terminology with thorough-going AGE patterning like the tetradic form itself. AGEs in tetradic theory and sometimes in reality link +2 and -1 with ego's generations as a block opposed to the set formed by the intervening generations, +1 and -1. To move in any direction so as to arrive at C-O, one would first have to unfold these blocks into a linear series by breaking up the AGEs. That on its own will produce a terminology that, like the tetradic model but unlike C-O, retains prescription (i.e., is linked to cross-cousin marriage), is symmetric (positing direct exchange of spouses between groups) and is in two descent lines. Such an evolution appears to have happened in the Dravidian language family, which at the present day combines terminologies with at least +2/-2 equations in the north of the Dravidian-speaking area (mostly among tribes in central India like the Oraon, Kui, Khond and Gond) with terminologies in the urban, more developed south that lack them, like Tamil and Telegu. Ironically, this is all set out in a paper of mine (1988) that Dziebel cites for other reasons, and it is also mentioned in Trautmann's earlier work, *Dravidian Kinship* (1981), so it is hardly a new discovery. Incidentally, I would not characterize either variant of terminology in the Dravidian language family as skewed, *pace* Dziebel: many of them are straightforwardly symmetric prescriptive, and skewing does not feature; if it were to be introduced, they would be very different terminologies.

Having arrived at a two-line symmetric prescriptive terminology of linear type (i.e., without AGEs), then what? One possible trajectory is change to a three-line asymmetric prescriptive terminology of the sort that is quite common in parts of Southeast Asia, possibly extending even to Native Siberia, but that only occurs sporadically elsewhere in the world, mainly the New World. That would involve breaking up the terminological equations uniting wife-takers with wife-givers, and also splitting the cross-cousins into two categories, matrilateral and patrilateral, which most commentators on C-O but not Dziebel see as essential, almost diagnostic, of such terminologies. For some, including Trautmann in his own chapter in T&W, the next step would be to introduce some vertical equations that resemble the well-known C-O ones into what is still an asymmetric prescriptive terminology, at which point one is on the verge of saying goodbye to prescription by banning the immediate repetition of alliances between the same two groups. I have queried this theory in Ch. 7 of my forthcoming book (Parkin 2021) by comparing the Kachin terminology, the textbook example of asymmetric prescription, with the actual Omaha terminology and finding that, although one would expect a degree of correlation between their vertical equations in accordance with Trautmann's hypothesis, the fit is actually not that close. There is in addition another problem. Omaha-type equations express patrilineal descent, Crow matrilineal. Unfortunately, there are very few examples of asymmetric prescriptive alliance with matrilineal descent, so although the theory of a shift to Omaha-type terminologies may look promising, similar evidence of a shift to Crow-type ones is much less apparent.

As both Dziebel and I realize, on the basis of Allen's early fieldwork in the Himalayas (1975, 1976) and in my case Dyen and Aberle's work on Athapaskan as well (1974), C-O terminologies might equally be derived direct from two-line prescriptive, which is related to tetradic society in Allen's hypothesis, as a later variant, or a development of it; as we have seen, such terminologies may or may not have AGEs, usually rather residual and vestigial. This is therefore still an indirect trajectory from tetradic to C-O, but a shorter one. Lastly here, Allen (in T&W) posits that vertical C-O type equations might develop with regular ZD marriage, which has skewing of a sort. As is well known, ZD marriage is not an independent form of affinal alliance but is found, though not consistently, with symmetric prescriptive alliance apparently in just two of the world's regions, south India and Native South America. Again, this is an indirect trajectory. And again, there objections to it in terms of descent in that the hypothesis has a patrilineal bias towards Omaha-type and against Crow-type examples. Returning to the initial question of whether C-O equations can evolve direct from AGEs, one example Dziebel might find interesting are the Iatmul in Francis Korn's rather idiosyncratic interpretation of it (Korn 1973), where there are a number of Omaha equations, some being just that, while others are discontinuous (i.e., alternating without being thoroughgoing AGEs). This might help sort out Dziebel's issue with Burling's Bodo-Garo languages, though for my part I cannot detect skewing in the series of equations with which this passage in Dziebel's commentary ends, *pace* the author.

Dziebel also makes much of the distinction between bifurcate merging and bifurcate collateral in accounting for his claimed trajectory from AGEs to Omaha and Crow respectively, for reasons I cannot admit to having understood. In any case, there is no reason why you can't have bifurcate merging with Crow, *viz*. F, FB, FZS, FZDS, nor bifurcate collateral with Omaha: for instance, the +1 level in the Khumbu Sherpa terminology is bifurcate collateral in so far as the terms for mother and father are separate from those for FB and MZ (Allen 1976: 571). Similarly, both bifurcate merging and bifurcate collateral can appear in prescriptive terminologies, though only bifurcate merging fits tetradic logic, with its emphasis on simplicity and small terminologies of just four terms (ignoring gender). For example, the Byansi terminology, also studied by Allen (1975: 82), combines bifurcate collateral in +1 terms for parallel kin (one term for each kin type, as in North Indian) with fully symmetric prescriptive equations for +1 cross kin. One could say that the Byansi terminology is bifurcate collateral in +1 because of the fact that F, FB and MB have separate terms, but that would miss the fact that it is also prescriptive by virtue of the term for MB having the additional specifications FZH and EF: in other words, the term for MB is not an isolating term like those for F and FB (ignoring relative age in the latter). This also means that Byansi has cross-parallel distinctions, assuming that this is what it meant be crossness (though I'm not entirely sure about that: can one have crossness without the parallelness that it is opposed to?). The Byansi examples shows that although the labels 'bifurcate merging' and 'bifurcate collateral' have the weight of tradition behind them, their usefulness is limited: I can think of no circumstances in which they can firmly be treated as diagnostic of any terminological type on their own, and attempting to do so can be misleading.

A related argument of Dziebel's against T&W concerns the mixing of cross and parallel in C-O. Although the equivalence between M, MZ and MBD, for example, certainly misses cross and parallel, it is perfectly logical of an Omaha system. It is obvious that a man's successor in a patrilineal system is his son, hence 'Omaha' MB = MBS (both cross kin, NB); perhaps less obvious, but just as logical, is M (parallel) = MZ on the principle of same-sex sibling equivalence; also parallel) = MBD (cross): all these women are born into the same patriline and represent female succession within that line (cf. straightforward matrilineal descent between a mother and her own daughter, who, given exogamy, are be born into different

patrilines). Where C-O clearly separates cross and parallel (and I'm sure this was what Trautmann had in mind) is between cross and parallel cousins and their respective descendants, and not just +1 and -1 collaterals (whether bifurcate merging or bifurcate collateral). Without these distinctions, C-O terminologies would not exist. Also, while the logic of the system dictates that patrilateral and matrilateral cross cousins should be distinguished as to side because of the gender bias, there is no such necessity in the case of the respective parallel cousins, and indeed the actual Omaha terminology unites them by side (i.e., patrilateral and matrilateral, distinguishing only gender; Barnes 1984: 132 ff., ms terms 7-9). It is true that even the key terms for vertical equations in C-O terminologies may attract additional specifications that don't seem to belong there: thus actual Omaha attaches MZH to the bifurcate merging term for F, FB etc. (ms term 3), FBW to the term for M, MZ, MBD etc. (ms term 4), and BW to a term (ms 16) otherwise reserved to wife's relatives (at least term 4 unites cross and parallel). However, none of these three examples is random, as they actually indicate an Iroquois (or, back an evolutionary step, Dravidian) pattern and, ipso facto, indicate symmetry, which one does not associate normally with C-O. This in itself may indicate C-O origins in Iroquois and/or Dravidian, despite both being symmetric systems and not asymmetric ones like C-O (see further below). This is further suggested by the fact that the +1 equation FZ = MBW (both cross kin) one gets with symmetric prescriptive terminologies also occurs in actual Omaha (ms term 5), though the other two +1 cross-relatives involved in an inter-cognate equation with prescription, MB and FZH, are, of course, separated here because MB has to do duty in the classic diagnostic Omaha equation MB = MBS (ms terms 6 and 15 respectively). The actual Omaha case is pretty consistent here, but there are clearly other examples worldwide that are not so consistent, as some of the data presented by Dziebel seem to show, and like the examples I have just uncovered. An obvious step forward would be to examine C-O terminologies in detail in this regard, such as the actual Omaha terms in Barnes's rich data (1984), though deciding whether remoter kin types were cross or parallel would be difficult.

On Allen's Byansi and Sherpa examples (1975, 1976), it is not true that the former is the only Tibeto-Burman example of prescription, though admittedly many other instances in the Himalayas are asymmetric (MBD/FZS marriage): Needham studied the latter extensively among Bodo speakers in the Indo-Burmese borderlands, and another well-known example for those who read German, further west, are the Kham Magar (Oppitz 1988). However, in his account of Byansi, Allen in effect used it as a prescriptive foil to explain what he felt had happened to the obviously post-prescriptive Sherpa terminology—and that had nothing to do with AGEs. In becoming non-prescriptive, the Sherpa terminology had to break the diagnostically crucial cognate-affine equations, thereby creating new categories looking for terms. There are a number of possibilities in this situation, some of them mentioned by Kriukov, whom Dziebel usefully mentions in another context: borrowing from another language, coining new lexis, etc. What Sherpa did, however, was to add the now free-floating specifications to terms in the adjacent generations, up and down depending on Omaha logic.

Thus, the transition from a symmetric prescriptive system like Sherpa to an 'Omaha' system is actually pretty straightforward: all that needs to happen is that some of the new free-floating post-prescriptive categories are 'seniorized', or raised a level, others 'juniorized' or dropped a level: despite Dziebel's puzzlement, it is this that produces the famous skewing affect in Sherpa. One can find similar changes in Athapaskan languages, studied intensively by Dyen and Aberle (1974), which I revisit in Ch. 7 of my forthcoming collection, and which, like Sherpa and Byansi, are basically symmetric (originally Iroquois in the opinion of the authors). Nonetheless, ultimately C-O terminologies are themselves fundamentally asymmetric for their distinction of cross-cousins by side, and of +1 cross kin in the line of diagnosis (MB etc. in

Omaha, FZ etc. in Crow) from the line of the other cross-relative (respectively FZ and MB; in both cases also, the lines of + 1 parallel kin, FB and MZ, are distinct as well).

Dziebel also briefly discusses the ancient Chinese text the *Erh Ya*, dating back to at least 200 BC, in relation to Kryukov's study of it. In fact, Kryukov was anticipated in this in a long article by Fêng Han-yi (1937; later published separately). Fêng concluded from a study of the history of Chinese kin terms that Chinese kinship was originally symmetric prescriptive, with cross-cousin marriage being allowed, but then banned and permitted alternately throughout Chinese history. For the second millennium BC there is an indication of semi-complex-like marriage restrictions in Fêng's finding that one could repeat marriage into another patriclan after five generations, but not before. As in actual Omaha (Barnes 1984), WBD marriage was allowed in certain periods as an occasional practice of elite groups. Terminologically there is evidence of a change from MB = EF to MB = WB, the latter a possible proxy for MB = MBS (cf. MBS = WB with asymmetric prescriptive alliance)?

A general point: apart from cross-cousin marriage, what is there to link terminological types to social morphology and practice apart from the coincidences of parallel historical developments that Dziebel mentions, partly with reference to Christopher Ehret's work in T&W? Statistical correlation does not equal cause and effect, and such parallels might well be adventitious. This applies inter alia to Ehret's speculations regarding African history (also 2008), well-informed though they might be in themselves. Even prescriptive terminologies are a feature of a wide range of societies, from mobile hunter-gatherers in Native Australian and Amazonian populations to the long-established literate and settled urban societies of south India, with their very different 'population and demographic processes', in Dziebel words. I am generally skeptical of such correlations for that reason. In truth, there may be different origins for C-O, and we should also recognize that the sociological features that have been associated with them by such authorities as Lévi-Strauss do not always occur and do not need C-O terminologies (a point already made for North India, including caste-influenced tribes like many Munda and Dravidians).

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