

COMMENT ON DZIEBEL

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Abstract: *German Dziebel's critique of our Crow-Omaha volume of nine years ago rests on his book of fourteen years ago. He acknowledges that crossness and skewing may in some instances covary but denies the covariance has any causal significance. Instead, he argues, Crow-Omaha systems derive from kin-terminologies marked by intergenerational self-reciprocals, which are purely linguistic in nature and uninfluenced by social organization; that sibling terminologies emphasizing relative age evolve into Omaha systems, and those emphasizing relative sex into Crow systems; and that in kinship-system evolution it is sibling terminologies—rather than crossness that predicates marriage alliances—which are the driving force.*

We show in reply that systems with skewing are intimately and dynamically associated with crossness, even more robustly than previously thought, both empirically and, through re-interpretation of Lounsbury's work, analytically. The interaction of crossness and skewing through linguistic or geographic contiguity is the best and most promising way forward in the study of Crow-Omaha, and work since the appearance of our book bears this out. We show too that works of Popov, Hornborg and Barnard, that our critic cites in his favor, support our position and not his. And we suggest that the argument of his 2007 book, for all its strengths, hitches his evolutionary model to a belief that Homo sapiens arose and spread "out of America" rather than "out of Africa", an entailment of his kinship analysis that readers will likely find off-putting. We affirm the deep embedding of skewed systems within systems having crossness, controvert his (Kroeber-like) insistence that kinship is purely linguistic and not social-organizational, and dispute that the many who find the "out of Africa" thesis well-grounded are all wet.

0. Introduction

German Dziebel, author of several works on kinship notable for their boldness, no sooner congratulates us on our Crow-Omaha book than he condemns it. Of what great crime are we guilty?—Of being too modest! Of siding with tradition! It is not a crime for which he will be convicted. He does not blush to name the qualities of his own work as “constructive and adventurist pragmatism.”

Dziebel’s review of our book dismisses its central idea, about the relation of crossness and Crow-Omaha skewing, and promotes another, having to do with self-reciprocals across the generations. We will direct our comment to this central matter.

1. Crossness and Crow-Omaha

Readers trying to weigh these alternatives, or even trying to decide whether the alternatives are really opposed to one another, need to know that we and some of our authors have participated before in an exercise such as this one. It concerned a reanalysis of kinship data from the ethnography of Junod upon the Thonga, by Dwight Read (Read 2018; Trautmann and Whiteley 2018). In our comment we laid out the evidence for crossness in the two classic articles by Lounsbury, on the formal analysis of Seneca and on Crow-Omaha (1964a, b). We quote the gist of our comment in the appendix to this comment. We will come back to it. We will approach it by steps, beginning with the conference at the Maison Suger (1993) organized by Maurice Godelier and its conference volume (Godelier, Trautmann and Tjon Sie Fat, eds. 1998).

In that volume a chapter by Trautmann and Barnes (1998) on the details of the Morgan tables of kinship had been provoked by Kronenfeld’s study of crossness and skewing in Fanti, in the course of which he discovered discrepancies in Morgan’s tables (Kronenfeld 1989). Concern over the need to be sure of the integrity of the data caused Trautmann and Barnes to study the original manuscripts of Morgan’s tables, which confirmed Kronenfeld’s discovery of errors; but examination showed that the subsequent scholarly literature had not relied upon the small handful of erroneous data-points in Morgan’s tables. Additionally, Trautmann and Barnes reanalyzed the kinship data in the Great Lakes region of the United States and Canada, showing there was a north-south gradient associating crossness of Dravidian and Iroquois type (called Type A and Type B) with skewing of Omaha type.

Also in the volume from the Suger Conference is a superb article by Viveiros de Castro, showing eight forms of ethnographically attested crossness, and another by Tjon Sie Fat showing 16 theoretical forms of crossness, whose interrelation can be shown with a four-dimensional hypercube in which all 16 types are connected with one another at a distance of one, two, three or (at most) four simple transformations. (Viveiros de Castro 1998; Tjon Sie Fat 1998; Godelier, Trautmann and Tjon Sie Fat 1998:10-12). Several other of the papers expanded upon Dravidian or other forms of crossness. As a result of the Suger Conference crossness had become much more complex and interesting, and the bearing upon kinship of new ethnography and new interpretations of crossness and affinity, from South America, represented in the volume by Hornborg and Viveiros de Castro, became even more evident. Crow-Omaha was addressed by a few papers, notably those of Trautmann and Barnes, and Kryukov.

Some time after, considering that the Suger Conference volume had been largely devoted to crossness, Whiteley was moved to organize a follow-up conference devoted to Crow-Omaha, with support of the Amerind Foundation (2010). Our Crow-Omaha book published papers of the

Amerind Conference (2012), which offers abundant evidence of the association of crossness with Crow-Omaha skewing.

Dziebel's critique is as follows:

The Trautmann & Whiteley volume approaches Crow-Omaha from an inherently flawed angle inherited from the earlier kinship typologies such as George P. Murdock's. In this tradition Crow-Omaha is considered to be a version or a transformation of Iroquois or Bifurcate Merging systems.

One quote from Trautmann ("Crossness and Crow-Omaha," p. 31) that "Crow-Omaha kinship—by which I mean kinship terminologies containing skewing—invariably also contains crossness" may be invoked as a justification for the overall Bifurcate Merging-centric stance, but Trautmann's claim is simply not true cross-culturally. Crow Omaha equations are readily found in terminologies without crossness (See Popov 1977)

Popov's (1977) worldwide survey of Crow-Omaha patterns discovered that only Lineal terminologies are known not to be compatible with Crow-Omaha equations.

The disproof consists of showing empirical evidence of Crow-Omaha skewing with Bifurcate Merging (here used as a stand-in for crossness), but also with Generational and with Bifurcate Collateral, though not with Lineal. But this is not the same as saying that Crow-Omaha skewing invariably contains crossness, as we may see by examining Lounsbury's formal analysis.

Dziebel, however, does *not* say they (Bifurcate Merging, or, crossing, and Crow-Omaha skewing) "cannot be related or cannot co-vary." Here at least is something of a common ground. He does not deny, we take it, scenarios in which communities which are contiguous to one another in some way, linguistically or geographically let us say, have some of them crossness, and some of them skewing, as in the Great Lakes region of North America, and his article discusses such scenarios in chapters of our book, such as those of McConvell, Ehret and Whiteley. In so doing, Dziebel tacitly concedes our view that crossness is associated with Crow-Omaha skewing and that regional studies are a good way to advance understanding of Crow-Omaha. A forthcoming paper by Whiteley and McConvell surveys North America and Australia, showing "How Crow-Omaha skewing spreads" in the two continents, building upon the finding that "Skewing occurs as an addition, or new type of equation, upon systems with crossness". It demonstrates how very productive this idea is, in leading us to new ideas, in this case quite different, mirror-image patterns of spread as between Crow and Omaha:

As Omaha skewing, with its patrilineal and patrilocal tendencies, correlates with downstream expansion and encompassing of demes from neighboring groups, Crow, its mirror opposite, with matrilineal and matrilocal tendencies, absorbs immigrant demes in a pattern of in situ consolidation and growth.

It also demonstrates anew the continuing worth of close-grained comparison of systems within regions.

Now, suppose a terminology containing crossness and another containing skewing is found, not in a neighboring community but within one and the same community, speaking one and the same language, each used in different contexts. This is what Kronenfeld finds for Fanti (Kronenfeld 2012 and prior texts cited therein). It points to an association that is very much stronger than those shown by contiguity of language or geography. Kronenfeld's work had a good response among conference participants that appeared in discussion and in the chapters of the book.

As a further increment of strength for the association demonstrated by linguistic and/or geographic contiguity of crossness and Crow-Omaha skewing, as among communities of the Great Lakes region, or simultaneous co-existence of them in one and the same community such as the Fanti of Ghana, we need to examine Lounsbury's formalization of Crow-Omaha. It is symptomatic of Dziebel's misunderstanding of our interpretation that he does not mention Lounsbury in his critique of us, whose classic paper is crucial, and is cited by Popov. Discussion of Lounsbury in our book, admittedly, is concise, because of the length-constraint imposed by the publisher (explained in the preface), and it does not do justice to the place his work serves in the making of our book; to this extent Dziebel may be excused. We repaired this deficit in the comment on Read mentioned above and refer readers to an extract of it in the appendix to this comment.

Lounsbury's formal analysis of Crow and Omaha contains three rules: the half-sibling rule, the merging rule, and the skewing rule. The first is universal, so that we may leave it aside. The other two go to the heart of the question, as merging has to do with crossness, and skewing has to do with skewing of Crow-Omaha type, the two terms of our interpretation.

The merging rule is, put more fully, the same-sex sibling merging rule, which says that (in specified contexts) the same-sex sibling is merged with ego, i.e., a brother with ego male, a sister with ego female. This serves to merge the parallel kin, and the unmerged residue constitutes the cross kin. It accounts for the merger of FB with F and MZ with M, as is diagnostic for terminologies of Bifurcate Merging type.

The skewing rule comes in four subvarieties which, in concert with the former rules, generate four types each for Crow (matrilineal) and Omaha (patrilineal), or eight in all, of which the four Omaha types are mirror images of the four Crow types. The subvarieties of the skewing rule vary in strength, and Lounsbury gives them starting from the weakest and ending with the strongest form.

What is the relation of crossness to skewing among these eight subvarieties? Lounsbury is explicit about this. The best locus is his note 3. in which he compares the varying states of "bifurcation" (crossness) for Crow, and for nonskewed systems:

A fully bifurcate system is one that distinguishes between "cross" and "parallel" kin types for both males and females in the first ascending generation, and in relation to both a male ego and a female ego for kin types of the first descending generation. An example of a fully bifurcate Crow-type system is Cherokee (cf. Morgan, 1871; Gilbert, 1937, 1943).

A semibifurcate system is one that distinguishes between "cross" and "parallel" kin type[s] only among males in the first ascending generation, and only in relation to a male ego for kin types of the first descending generation. An example of a semibifurcate Crow-type system is Pawnee (cf. Lounsbury, 1958).

A nonbifurcate system is one that makes no distinctions between "cross" and "parallel" kin types. An example of a nonbifurcate Crow-type system is Trukese (cf. Goodenough, 1951, 1956).

These characterizations are applicable also to nonskewed systems. E.g., among the Iroquois tribes, the Seneca, Wyandot, and Tuscarora systems are fully bifurcate, while the Mohawk, Oneida, Onondaga, and Cayuga are semibifurcate (cf. Morgan 1871; Lounsbury, 1962). The so-called Hawaiian-type systems are the nonbifurcate analog to these.

Our description of Lounsbury's formal account of the eight subvarieties of Crow and Omaha is that the skewing rule, in its four degrees of strength, *acts upon cross-kin only*, such that with

each increment of strength of the skewing rule there is an increase in the number of cross-kin who get reclassified as parallel, till, under the strongest version of the skewing rule, *no* cross-kin remain (Type 4). So, for example, the categories of the parents' generation would be, from weakest to strongest version of the skewing rule, F, M, FZ, MB (bifurcate); F, M, FZ or F, M, MB (semibifurcate); or F, M (nonbifurcate), showing, respectively, two parallel and two cross kin; two parallel and one cross kin; and two parallel and no cross kin.

Our reading of Lounsbury is, of a skewed system with no cross kin in evidence, that this system *has* crossness, but the cross kin category is *empty*, at the surface level, because the application of a strong skewing rule empties it of content, but that crossness remains in the deep structure. The semibifurcate systems are midway between the extremes. Both these will not appear to be Bifurcate Merging.

This being so, a method relying upon a large database and equations of kintypes is unable to find the true relation between crossness and skewing because crossness diminishes at the surface level—to zero!—with the increasing strength of the skewing rule. Taking skewing to be simple, without discriminating increments of strength as Lounsbury shows us to do, and crossness likewise, is an error that vitiates the analysis, and renders the method unable to correct itself. For the configuration of crossness at the surface level will lose the features of Bifurcate Merging. Assignment of systems to the fourfold classification will reflect the gradient of crossness at the surface level which Lounsbury identified, and which change kin type equations, and not the strong crossness of the deep structure. A database of Murdockian structure will obscure the effect Lounsbury describes.

A virtue of Dziebel's remarks, however, is that it brings to our attention Popov 1977, a work deserving to be better known outside of Russia. (We are grateful to Anastasia Kalyuta, who was a student of Popov, for making a translation.) It is a study of the place of Crow-Omaha within a typology of kinship terminological systems world-wide, interpreted as a series of evolutionary stages connected with socio-economic changes from primitive to complex. It engages most closely with the work of Kryukov which is better-known in the West through the Suger Conference volume in which it appears. (It also is unique, perhaps, in the literature on Crow-Omaha in citing a head of state, namely V.I. Lenin, for his social-science knowledge.)

Popov's typology adds six variants to the Crow and Omaha type (Bifurcate; Bifurcate-Lineal; Generational; Bifurcate — Bifurcate-Lineal; Bifurcate — Generational; Bifurcate-Lineal — Generational) with ethnographic instances. So far from contradicting the association of crossness and Crow-Omaha, Popov takes it as the starting-point of analysis, and nothing he says in this piece contradicts the connection of skewing with crossness. Associations of Crow-Omaha skewing with other organizing principles are consistent with our reading of Lounsbury. Popov does not undermine our book in the least, nor does his argument give aid and comfort to the Dziebel hypothesis concerning a connection of Crow-Omaha with self-reciprocals.

Dziebel further says,

Crow-Omaha cannot be described as a version or an evolution of Bifurcate Merging because Bifurcate Merging is a principle of *horizontal* (same-generation) grouping of kin categories, while Crow-Omaha is a principle of *vertical* (cross-generation) grouping. They are different in principle and one doesn't evolve from the other and can't be a subset of the other.

It is a doubtful proposition, which needs more than mere assertion to make it persuasive. Crossness is a property which crosses generations, especially the three central (parents' to children's) and in some cases five generations (grandparents' to grandchildren's), so to say it is horizontal is to be misled by the (horizontal) pattern of kin type equations within generations.

2. Crow-Omaha, Self-Reciprocals, and Tetradic Theory

“Crow Omaha arises with the dissolution of alternate-generation equations”
(Dziebel above)

Dziebel's review is a densely argued version of a very large book and its lead-up publications, to which it often refers. The book has both strengths and weaknesses in our opinion. Its strengths include: a lengthy review of nineteenth century studies of relationship, starting with Morgan; a survey of 20th century anthropological study of kinship terminology; re-presentation of N.J. Allen's tetradic theory and resituating it to bring forward self-reciprocal alternate generation kin terms; a reanalysis and extension upon Nerlove and Romney's analysis of sibling terms; and the putting into play of an argument for convergence among kinship, linguistics and genetics on issues of human population and migration around the world in the last 40,000 years, i.e., since the putative beginning of language and kinship terminologies. Its primary weakness lies in the approach to human sociocultural evolution, which includes a repudiation of the global consensus from archaeology and biological anthropology for an origin of *Homo sapiens* in Africa, replacing this with a remarkable “Out of America” scenario. Accepting Dziebel's argument for kinship system evolution thus requires rejecting two scientific consensuses: a) that Early Modern Humans originated in Africa; b) that the peopling of the Americas occurred much later than most areas of the Old World. Scientific consensuses persist until they are overturned by better interpretations. Dziebel offers no rereading of the existing archaeological and biological evidence nor any scientifically sound basis for doubting them, just the conviction that they must be wrong since they do not support his thesis. He wants a paradigm shift without a demonstrated rationale: a Copernican Revolution without Copernicus. As this outcome results from his reconstruction of the evolution of kin terminologies, it casts an enormous shadow onto his entire analytical apparatus. This may be why his book has not generated the interest its strengths deserve, including that of our book on Crow-Omaha.

Dziebel's argument for Crow-Omaha systems boils down to this: instead of the by-now standard view that such systems emerged from (globally widespread) systems with crossness, Crow-Omaha skewing derives evolutionarily rather from kin terminologies marked by intergenerational self-reciprocals. Such terminologies unite PP and CC with the same term (e.g., FF=SS; MM=DD), and variably four other, adjacent-generation collateral relationships, e.g., MB=ZC, FZ=BC, FB=BC, and MZ=ZC (2007:206). If a system has all five self-reciprocals, Dziebel dubs it “super-reciprocal.” He maintains this represents the earliest human kin terminology, from which all others have evolved. As self-reciprocal equations begin to break down, sibling terminologies arranged by relative age and sex become the driving force of kinship system change, some transforming in turn into Crow-Omaha systems. In time Crow-Omaha equations and distinctions are superseded by “descriptive” simplifications of kin terms. In effect, Dziebel argues here for an evolutionary trajectory from many terms to few. We return to this below.

Except for his novel inclusion of adjacent-generation self-reciprocals, Dziebel borrows from Allen's tetradic theory (e.g., Allen 1986), which situates alternate-generation equations (“AGE” hereafter) in a suite of three “primordial equations” proposed as constituting the earliest human kinship systems. The other two equations are “classificatory” (e.g., F = FB; S = BS; W = WZ) and

“prescriptive” (e.g., $W = MBD = FZD$; $MB = FZH = WF$), i.e., those classically associated with crossness. “Tetradic” refers to the set of four terms/symbols/units Allen proposes as comprising the earliest formal social relationships. Allen’s theory is multi-dimensional, but in one key aspect the four terms represent B-Z pairs in two generations. With numbers (0-3) symbolizing the terms, core genealogical meanings are: 0 = mEgo+Z; 1 = F+FZ; 2 = cross-cousins; 3 = M+MB (Allen 1982:140). Allen explicitly treats the terms as “classificatory” (i.e., encompassing more than primary biological kin) and “prescriptive” (i.e., predicating marriage partners). From a sociocentric (rather than genealogical) perspective, the four components may manifest as marriage sections, similar to some empirical Australian cases (e.g., Allen 1982:142, 1998:321). Perpetuation of this system down the generations—Allen prefers “recruitment” to “descent”—occurs via repeating cycles of bilateral cross-cousin marriage and, the main point here, via re-application of kin-terms to successive alternate generations. In effect, the AGE terms in G^{+2} are “inherited,” marking the same set of categorical distinctions and associations in G^0 and, as children are born and replace the prior generation, in G^{-2} ($G^{+2} = G^0 = G^{-2}$). Likewise, the terms in G^{+1} are inherited by G^{-1} etc. ($G^{+1} = G^{-1} = G^{-3}$). AGE thus perpetuate the system diachronically. But what it is exactly that they perpetuate are the other two sides of Allen’s primordial triangle, i.e., the classificatory and prescriptive structure. In Allen’s model AGE are indissolubly associated with terms that are classificatory in grouping people into isomorphous categories of relatives, and prescriptive in specifying the mode of social reproduction from one generation to the next. Tetradic theory is explicitly about “tetradic society,” i.e., a hypothetical original condition in which kin-terms specify social relations (e.g., Allen 2012). The closest empirical realizations of the theory are Kariera and Dravidian systems (e.g., Allen 1998, 2012; Hage 2001:489).

In contrast, Dzielbel’s thesis attaching adjacent-generation equations to AGE rests upon his rejection of tetradic theory as a whole. He removes AGE from their intrinsic interdependence with crossness in Allen’s model. Dismissing crossness and correlative affinity, Dzielbel argues that free-floating superreciprocal terminology is purely a linguistic system with “no social correlates” (2007:156). Crow-Omaha skewing emerges, he claims, with the collapse of some self-reciprocal sets and fades out when kin-terms shift from “classificatory” (including Crow-Omaha) to “descriptive” type. Descriptive terms represent an evolutionary simplification in which primary terms (“*father*,” “*sister*” etc.) require combining into a noun phrase to name non-primary relatives (e.g., “*father’s sister*,” or a lexical abbreviation like “*faster*” [FZ] in Swedish). Thus Crow-Omaha systems occupy an intermediate evolutionary phase: Superreciprocal → Crow-Omaha → Descriptive (2007:249).

While Dzielbel proclaims a belief that “terminologies are dialectically related to the systems of affinal alliance” (2007:125)—though what he means analytically by “dialectically related” is unclear—that assertion often falls by the wayside in his actual analysis of the evolution of terminologies. Indeed, he maintains that the superreciprocal form only incidentally coincides with marriage sections in Australia but is wholly separate from affinal arrangements in Native North America. He claims North American systems are characterized by “superreciprocity with no moieties and no bilateral cross-cousin marriage” (2007:300). No serious Americanist could assent to this. The majority of ethnographically described Native North American terminologies lack superreciprocity. There are multiple instances of exogamous moieties (including northern Northwest Coast, California, Plains, and Southeast Woodlands—some co-occurring with Crow-Omaha terminologies) as well as agamous moieties (e.g., Driver and Massey 1957:409-412, figure 158). Bilateral cross-cousin marriage is well-established on the northern Northwest Coast and for a large swath

of the Sub-Arctic (ibid.:398, map 152); it seems very likely to have been present among the Pueblos also (e.g., Fox 1967; Whiteley 2018).

While many theorists have concluded (whether or not they accept tetradic theory) that a system with crossness of Dravidian or Kariera type likely represents the oldest arrangement of social relations via kinterms, Dzielbel sees these as derived. Instead, he claims that the more causally salient array of kinterms is not that which predicates marriage alliance, but rather the arrangement of sibling terms, especially those marked by relative age [“RA”] and relative sex [“RS”] of speaker and/or referent: “sibling terminology provides the most coherent picture of human kin terminological variation” (2007:287). This is a wholly novel concept: we are not aware of any prior argument that identifies sibling terminology as definitive for social evolution. Sibling terminologies that place greater emphasis on RA may evolve (with the erosion of prior intergenerational equations), Dzielbel argues, into systems with Omaha skewing; those emphasizing RS may evolve into systems with Crow skewing (2007:292-293). Inverse transformations, RS → Omaha, RA → Crow, he claims, are rare to non-existent.

This is part of Dzielbel’s larger argument for the emergence and evolution of kin terminologies across the globe and since the beginning of the species. He depicts his aim in the spirit of L.H. Morgan in its comprehensiveness and sweep. Indeed, much more than Morgan, Dzielbel engages the epistemological assistance of multiple forms of argument: from cognitive psychology, the mind-brain identity theory, marking theory in linguistics, Schopenhauer on genius and madness, historical linguistics, human genetics, archaeology and many more besides. Dzielbel assembled data for some 2,500 kin terminologies, reducing them to a series of types, partly according to conventional groupings and partly at his own invention. He cites a prolific array of ethnographic sources in seeking to support his overall argument.

From his general evolutionary trajectories Dzielbel postulates concurrences among kin terminologies, language histories, and genes worldwide: “there are strong indications that languages, kinship structures, and genes concord in their basic global configurations” (2007:374). If the reader finds this assertion surprising *prima facie*, it leads to an even more radical conclusion: that the evolution of kin-terminologies shows modern humans did not originate in Africa, but rather in the Americas, from where they outspread, with their genes, tools, and progressively simplifying kin-terminologies throughout the (misnamed) Old World. In holding onto the most diverse patterns of kin terminologies (notably of putatively widespread superreciprocals and RA and RS sibling types) as well as language families—together, he argues, with commensurate patterns of genetic variation—the Americas are the cradle of humankind, a *Homo sapiens* sociocultural and demographic substrate: the archaic source of all that has followed. Positioning his argument against the “out-of-Africa” model of human global population and diversification, Dzielbel thus advances a highly novel perspective. Even at first blush the notion that the most internally variegated kin terminologies—including the most complex RA and RS sibling variations—are the oldest forms is puzzling, particularly in contrast to Allen’s trajectory from few terms to many. Dzielbel’s evolutionary trajectory is from many kinterms to few.

As regards Crow-Omaha skewing, a key problem is how this emerges from Dzielbel’s superreciprocals. None of his adjacent-generation self-reciprocals aligns with Crow-Omaha equations. Instead Dzielbel derives the transition via evolutionary change of a preexisting marriage rule (how and where it preexists, given his exclusion of social correlates from his primordial terminology, is a mystery) with the simultaneous emergence of unilineal descent. The postulated preexisting rule identifies prototypical marriage partners in three medial generations. With the arrival of a rule of unilineal descent, one of those generations drops out, leaving two that are equivalent to each other

as marriage partners for ego (Dziebel 2007:243-244). So, in a prior condition of FZ, FZD and ZD as marriage partners for ego male, with the addition of unilineal descent, ZD drops out in a matrilineal (Crow) system, leaving FZ and FZD; in a patrilineal (Omaha) system, FZ drops out, leaving FZD and ZD as prototypical partners. Similarly, for ego female, from a prior condition in which prototypical partners are MB, MBS and BS, once patrilineal descent is added BS is deleted in an Omaha system, leaving MB and MBS as equivalent prototypical partners; if the added descent system is matrilineal, MB is deleted, leaving only MBS and BS as equivalent marriage partners in a Crow system. From two-generation equivalence as marriage partners emerge terminological equations of oblique lineal type, e.g., FZ = FZD for Crow-matrilineal, and MB = MBS for Omaha-patrilineal (but only for those two generations, it seems). (We note in passing that there are many patrilineal and matrilineal systems without Crow-Omaha skewing—which Dziebel [2007:104] glancingly acknowledges but does not engage in his deployment of unilineal descent to explain Crow-Omaha. As Leslie White [1939] trenchantly argued—largely for North American cases—Crow-Omaha systems coincide with a strengthening of social institutions, like clans, associated with unilineal descent in a terminological system of Dakota-Iroquois a.k.a. bifurcate-merging type—i.e., with crossness. Independently, McConvell [2012] has concluded the same for Australia [cf. Whiteley and McConvell forthcoming].)

For his superreciprocal \rightarrow Crow-Omaha transition, Dziebel (2007:243-244) borrows heavily from an argument by Hornborg (1998). In his review of our book (above) Dziebel notes:

In (Dziebel 2007) I fleshed out a hypothesis (first put forth in Dziebel 1992 and later independently alluded to by Alf Hornborg, see Dziebel 2007:243) that Crow-Omaha skewing originates from Alternate Generation equations.

In his book, Dziebel (2007:243-244) centers his enlistment of Hornborg on one of the latter's diagrams, quoting part of its accompanying argument. Hornborg (1998:177) captioned that diagram "From Dravidian to Crow-Omaha: "Core" Relationships from Male and Female Perspectives and the Logic of Skewing as Geared to Reifications of Gendered Perspectives (Unilineality)." In other words, the problem Hornborg sought to explain was how a system with Dravidian crossness might transform into Crow-Omaha. However, neglecting to inform the reader about the switch, Dziebel (2007:243) retitled Hornborg's diagram, "The structural transformation of self-reciprocal terminology into Crow-Omaha." Moreover, while limning some of Hornborg's argument for Amazonian Crow-Omaha, Dziebel (2007:243) leaves out its principal framing, notably:

. . . by now one can see the many alternative series of congruities that lead from Dravidian to Crow-Omaha classification. . . .

The best way to understand the structural principles at work in generating Crow-Omaha equations is to see their "skewing" as the result of a *reification* or *hegemonization*, of either the male or female perspective in a Dravidian context of parallel transmission. . . . (Hornborg 1998:176, emphases in original).

A few pages earlier, discussing intergenerational self-reciprocals, Hornborg notes:

Alternating Generations. Pano-speaking groups . . . suggest a specific development of the Dravidian model. . . . The Kariera system, which is clearly a variant of the Dravidian pattern . . . seems to be a cognitive adaptation to the emergence of socio-centered kin-affine dichotomies in societies practicing genealogically close (e.g., bilateral cross-cousin) marriage. As such, it could be seen as an alternative solution to the Iroquois one of introducing separate affinal terms (Hornborg 1998:172).

Elsewhere in his argument (2007:299), Dziebel recognizes Hornborg’s treatment of Panoan alternate-generation terminology as intermediate between Dravidian classification and unilineal descent but fails to connect this with Hornborg’s Dravidian → Crow-Omaha problematic. Hornborg’s several suggested (*reversible*) routes from Dravidian into Crow-Omaha include AGE as one of “the many alternative series of congruities” that go with skewing. But his whole conceptualization of the problem is precisely the one that Dziebel disputes, i.e., explaining how Crow-Omaha systems transform from Dravidian systems:

Crow-Omaha are defined by the effects of unilineal models on this basic [Dravidian] scheme, generating oblique equations of categories that in the Dravidian system are affinal (Hornborg 1998:182).

We dwell on this because it seems to represent a more pervasive tendency in Dziebel’s procedure: selecting aspects of an analysis or ethnographic record that are congenial to his argument and excluding or obviating others that are not. Hornborg sees both AGE and Crow-Omaha skewing as deriving from underlying Dravidian crossness. Whether or not we agree with this, Hornborg does not support Dziebel’s detachment of Crow-Omaha from crossness—quite the opposite.

3. Historical Linguistics, Terminological Innovations, and “Out of America”

Another key problem in Dziebel’s argument is a selective dependence on historical linguistics, both in general and in specific cases. More broadly, while historical linguistics may usefully inform a middle range of historical processes, uncritical reliance on putative longer-term reconstructions is scientifically unsupportable. As countless studies have shown, opposite conclusions in historical linguistics have frequently been generated from similar data under different premises. Largely driven by authority statements rather than falsifiable hypotheses, much historical linguistics remains stuck in a pre-scientific paradigm (Whiteley et al. 2019). How an account drawn from purportedly identifiable patterns of language change over, say, a 10,000-year period can be aligned with identified genetic processes among ethnolinguistic demes, and sibling-terminology histories retrojected as coordinate within language families, seems very far-fetched. Yet this is Dziebel’s proposal. So-called kinship-system histories and their historical-linguistic ambient, Dziebel maintains, are more reliable indicators of historical and demographic processes than existing archaeological or genetic reconstructions:

Since kinship systems supply evidence so much in discrepancy with the current models of the origins of humans out of Africa and the peopling of the Americas, it is advisable to suspend established judgements until the whole human genome is sequenced, all proto-languages and their kin vocabularies are reconstructed, and several long-term, wide-range, and open-minded archaeological investigations are performed in all three parts of the Americas (Dziebel 2007:374).

This is the ultimate escape clause for his argument. Reconstructing *one* proto-language is hardly uncontroversial (for recent Proto-Indo-European variations, see Bomhard 2019 and accompanying responses). Reliance on hypothetical proto-languages as historically real ancestors reflects the same “hapless appeals to plesiomorphy” (Rosen et al. 1981:264) that dogged evolutionary biology prior to the cladistic revolution: explanation is inevitably teleological. Completing disinterested, falsifiable reconstructions of *all* proto-languages, together with their kin-terminologies, is about as likely as discovering that the moon really is made of green cheese. And if we must defer all extant archaeological and genomic results in order to accept Dziebel’s “out-of-America” thesis,

his entire analytical edifice is revealed for what it is: a house of cards. That is a pity, as Dziebel has many kinship insights to offer, not least about the variations in sibling terminologies.

The “out-of-America” conclusion is, to say the least, idiosyncratic. In the review of our book, he seeks to borrow an (apparently unwilling) voice of kinship authority: “Alan Barnard (2012) who is exploring the links between social anthropology and modern human origins recently compared my interpretation of kinship and linguistic evidence to the traditional out-of-Africa view” (above). Yet Barnard (2012:116-117) rejects Dziebel on kinship-system evolution and the out-of-America conclusion out of hand, as Dziebel has acknowledged elsewhere (<http://anthropogenesis.kinshipstudies.org/blog/2012/07/21/barnard-on-dziebel-social-anthropology-meets-human-origins/>). So, his invocation of Barnard in the review above is both irrelevant and misleading. As with Hornborg on Crow-Omaha, the reader would be well-advised to consult the original sources.

Since Dziebel’s book was published, there has been no change in the consensus for a dispersal of Early Modern Humans (EMH) out of Africa. While migration chronologies and routes (e.g., into South Asia, Southeast Asia, Australia, and Siberia) remain debated, the basic fact of African origin and much earlier dispersal than in the Americas is not contested (e.g., Grouwcutt et al. 2015; O’Connell et al. 2018). While some archaeological sites (e.g., at Monte Verde in Chile) and genetic evidence push back arrival in the Americas somewhat earlier than the Clovis horizon, a singular claim that one archaeological site in southern California dates to ~130 kya is, to say the least, highly controversial (Holen et al. 2017; Ferraro et al. 2018). The earliest consensus dates from genomics for the populating of the Americas all lie within the ~23-15 kya range (e.g., Reich et al. 2012; Reich and Skoglund 2016; Moreno-Mayar et al. 2018; Posth et al. 2018; Flegontov et al. 2019; Montaigne 2020). This is far later than attested EMH remains-based dates in Africa (~200-150 kya) and Southwest Asia (~120-90 kya).

Dziebel’s book makes some valuable contributions, as we have noted. But the arrangement of his premises into such a drastically unsustainable conclusion (“out of America”) vitiates his argument’s logic. To promote that conclusion, Dziebel must depend on the wholesale rejection of more than a century of systematic archaeological research in the Americas, founded on eminently falsifiable methodologies, including several absolute-dating techniques. Absence of evidence, Dziebel avers, in the Americas is not evidence of human absence much, much earlier. Instead, archaeologists have deluded themselves into rejecting any finds that do not conform to some quasi-mystical belief about the relatively late populating of the New World. In short, the claims of scientific archaeology are bunk, mere magical thinking.

In Native North America (as well as in Australia, Amazonia, East Africa, and southeast Africa, at least), the ineluctable fact that many Crow-Omaha cases exist nearby systems with crossness in the same respective language-families cries out for an explanation (see, e.g., Schlee 1994, 2017; Hornborg 1998; Viveiros de Castro 1998; Coelho de Souza 2012; McConvell 2012; Trautmann 2012; Whiteley 2012, 2018; Trautmann and Whiteley 2018; Whiteley and McConvell forthcoming). All Dziebel is prepared to concede is that crossness occurs as a “counterpart” to Crow and Omaha skewing, never in a transformational relationship with it. We contend that relationships among geographically and linguistically proximate systems with crossness but without skewing are the obvious places to look for an explanation of the conditions under which skewing appears. Regionally focused comparisons, as our book sought to demonstrate, are the most robust route to an explanation for empirical Crow-Omaha. And they frequently disclose evolutionary patterns showing the addition of skewing to crossness via kin-term borrowing (semantic and sometimes morphological) across ethnolinguistic boundaries (e.g., McConvell 2018; Whiteley and McConvell forthcoming).

Dziebel’s argument for congruity among kin-terminologies, languages, and genes, rests on his preferred tree model of language change, to the exclusion of horizontal network influences, i.e., a wave model. For example, he presents Hopi Crow skewing as an autonomous innovation on other, older Uto-Aztecan terminologies (Hopi is the only one with skewing). The much greater likelihood, however, is that skewing diffused via alliances with their Pueblo neighbors. Dziebel derives Hopi skewing from RS sibling terminologies (2007:291) as for other Crow cases, asserting (2007:292) there are few to no exceptions in his RS → Crow/RA → Omaha axiom. Yet enigmatically he (2007:290-291, 321) also acknowledges (without explanation) that Hopi has both RS and RA terms as well as Crow skewing. In the review (above), he cites Hopi *kya* (“FZ = FZD” etc.) as a reflex of Uto-Aztecan **ka* (PM): “But from the general Uto-Aztecan perspective one can observe that Hopi *kya* is a reflex of Proto-Uto Aztecan (PUA) **ka* which has self-reciprocal meanings ‘grandmother; granddaughter’ in a number of daughter languages.” Hill (2018:139), however, has recently emphasized that Hopi *kya* is an *irregular* reflex that shows clear signs of “Puebloanization” from Zuni, Keresan, and Tanoan (Pueblo) languages, contributing to the emerging sense of a Pueblo *Sprachbund* (see also Whiteley and Snow 2015). It seems likely Crow skewing spread along the same historical pathways as other borrowings, including naming conventions and specific ritual practices, especially from Keresan influence (Whiteley and McConvell forthcoming). Long-term intermarriage among these Pueblo groups underwrites patterns of sharing. Again, the importance of affinity as intrinsic to kin-terminology seems undeniable in empirical cases. In sum, “That a Crow cluster of proximate Pueblos representing four linguistic groups, whose other shared cultural features derive from long-term systematic exchange, strongly argues for diffusion” (Whiteley and McConvell forthcoming). The same is true throughout North American Crow and Omaha distributions elsewhere.

4. Conclusions

In sum, while parts of Dziebel’s argument may have cognitive appeal, more practical solutions to the “Crow-Omaha problem” must focus on: a) actual patterns of kin-term usages, including sharing among neighbors, particularly those of different language groups in the same regions; and b) attendance to known ethnohistorical patterns, including adaptation, demographic change, and identified alliance patterns, as well as described changes in kin-terminologies. This is what our book set out to demonstrate. Empirically, the close proximities and relationships between Crow-Omaha cases and systems with crossness but without skewing, worldwide, together with the clear persistence of crossness in other parts of Crow-Omaha terminologies, strongly suggest that explanations should attend to crossness as the underlying condition of skewing. If Ockham’s Razor is our guide, as an explanation for the independent origins of multi-generation skewing in all parts of the world, derivation from a prior condition of cross-cousin marriage and Dravidian or Iroquois terminologies—which are globally contiguous in many instances to Crow and Omaha systems—is a much more parsimonious route than self-reciprocal terms conceived as detached from social relations. And an evolutionary trajectory from few terms to many is both logically and practically more likely than the converse.

Appendix: Lounsbury on Crow-Omaha and Crossness

Extract from Trautmann and Whiteley 2018:2-6.

Lounsbury's analysis was inaugurated in two classic papers published virtually simultaneously in 1964, one of them devoted to Seneca Iroquois and crossness, the other to Crow-Omaha skewing (Lounsbury 1964a, b). They address the very things that Morgan learned to his great surprise, that served to launch kinship analysis as a field of study and anthropology as a discipline. Morgan learned that for the Iroquois the father's brother "was equally a father" and the mother's sister a mother (crossness), and that for certain other Native American groups the son of an uncle was an uncle (MBS = MB) (skewing). Lounsbury's rules of "same-sex sibling merging" formalizes the nature of crossness, and the "skewing rule" that of skewing in its several forms. One of the gains of his Crow-Omaha paper was that his analysis enabled him to distinguish with great clarity four distinct kinds of Crow skewing, and four of Omaha.

Another outstanding example of the fruitfulness of this approach, from the first of these papers, delivers one result of the careful comparison that the formalization made possible, from the moment of its appearance: the hitherto unattended difference between Iroquois and Dravidian crossness. Lounsbury states that while he became acquainted with the fact that Iroquois might have "fathers" and "maternal uncles" in any clan, so that predictions of relations through the matrilineal clans were often false, "my colleague Leopold Pospisil was finding out the same thing for the Kapauku Papuans" (of West Irian in New Guinea). "My astonishment at discovering the real principle operative in the reckoning of bifurcation in an Iroquois-type kinship system was matched by his. It was contrary to all of the expectations to which we had been led by the anthropological theoretical writings on the subject." (1964a: 1079, fn. 4)

The consequences of this important finding were many, starting with the revision of theoretical understanding on this point, from which we may estimate the value of Lounsbury's intervention. New forms crossness were found, beginning with Reay, in whose earlier work on the Kuma it was now seen that the type of crossness was different from Dravidian and Iroquois (Reay 1959). Lounsburian analysis was extended to resolve some of the questions about actual Dravidian kinship of South India and Sri Lanka, and to trace the deeper time-depth of Dravidian there (Trautmann 1981). The Maison Suger conference took up analysis of crossness at great depth. In the conference volume Eduardo Viveiros de Castro (1998) identified eight actual types of crossness. Franklin Tjon Sie Fat undertook a mathematical assessment of Trautmann's Lounsbury-style rules for Dravidian, and devised a hypercube by which formal relations among 16 types of crossness could be visualized in the form of a small number (maximum four) transformational "steps" separating any two types from one another (Tjon Sie Fat 1998; Godelier et al. 1998:11-12, 18-25). Through analysis of neighboring societies in a single contiguous region of North America, that of the Great Lakes, Trautmann and Robert Barnes (1998) were able to show the close juxtaposition of systems with crossness of Dravidian and Iroquois kinds, and skewing. These and indeed all the contributions to the conference may be said to have been stimulated by the Lounsburian analysis and the better understanding of Iroquois and Dravidian that he supplied.

One must add the work of Harold Scheffler, his article surveying kinship systems of Melanesia (1971), his valuable structural typology of kinship systems (1972), his book surveying kinship systems of Australia giving Lounsburian rules for all the kinship systems examined (1978), and his collaboration with Lounsbury, on Sirionó (1971). The formalization of Lounsbury's rules by Sydney Gould (Gould 2000) should also be mentioned (regarding its specific Crow-Omaha explanatory value, see, e.g., Whiteley 2012). Scheffler, in David Kronenfeld's (2009:129) terms,

became a “spokesman for a kind of Lounsburian ‘school.’” However loose-limbed and elective, that “school” included some rigorous analysts of kinship systems, including A. Kimball Romney, Eugene Hammel, Charles Frake, Paul Friedrich, Harold Conklin, and Kronenfeld himself. Their “Lounsburian” contributions, uniting kinship formalism with empirical social and cultural contexts, can hardly be considered only “descriptive.” For example, speaking of Lounsbury's treatment of conjunctive definitions vis-à-vis kin-terms, Kronenfeld (2009:121) emphasized “. . . the fact that Lounsbury could find one and only one conjunctively defined definition of cross-parallel in Seneca is nontrivial; and the complexity of that definition argues strongly for the social and cultural importance of whatever it represents.” In other words, Lounsbury's formal analysis of Seneca-Iroquois crossness disclosed an otherwise obscured but robust structural underpinning of Seneca social relations: surely, this is precisely what constitutes genuine anthropological explanation. Or, in countering Needham's critique of Lounsbury's genealogical-extensionist convictions, “Lounsbury's assumptions are reasonable not because of his metaphysical or sociological beliefs but because empirically they work where nothing else does” (Kronenfeld 2009:125). They work exactly because of their heuristic, i.e., *explanatory*, capacity in application to actual ethnological phenomena, not because they entail mere description.

Dwight Read himself should be added to the list, and this very paper of his. He says (p. 43):

The Fox example is of particular interest since their terminology has a qualitatively different generative logic than does the Thonga terminology, even though both are considered to be Omaha terminologies. The difference can be seen in the fact that the equivalence rules used by Lounsbury (1964) to generate the genealogical categories corresponding to the Fox kin terms do not generate the genealogical categories for the Thonga-Ronga kin terms. This qualitative difference traces back to the fact that, as will be shown next, the Fox terminology, with its Omaha skewing, can be derived from an Iroquois terminology.

Remove skewing from Fox, and a fully-formed terminology of Iroquois type (i.e., with crossness) remains. This confirms what we have said in our book, and it is pleasing to see Read show that Lounsbury leads us thither; we agree. Thus skewing is an overlay upon a system with crossness. By implication, it confirms the value of regional comparison, in the degree to which crossness and skewing may be found in the kinship terminologies of adjacent peoples, forming among them a field of variation. We are happy to think we have found common ground here.

None of our discussion of Lounsbury should be construed to imply that we think his two 1964 articles on equivalence rules are beyond criticism. Our point is that their contribution, as measured by the subsequent work that they have stimulated, is of the first importance--as we may see even in Read's article, in which reference to Lounsbury recurs throughout.

Criticisms that have been made of Lounsbury's equivalence rules involve such matters as genealogism or biologism, extensionism and the commitment to polysemy as its theoretical anchor, and metaphor. It is our view that these criticisms are misplaced. Lounsbury embraced extensionism as a theoretical underpinning for his sets of extension rules for kinship terminologies. There is nothing outlandish about the idea that some meanings are extensions of other, prior ones, an idea we encounter often, whenever we open a dictionary, for example. But in this context it has proven controversial, and Lounsbury seems to have recognized at the outset that it would. Our view is that there are no stakes here, and that one may be perfectly agnostic on the question and yet find the extension rules useful, even indispensable, as tools extending knowledge of kinship terminologies as ordered sets. Our view is that the extension rules are neither biological nor genealogical, but essentially pedagogical in nature, serving as a means to render unfamiliar terms

into familiar ones, and in so doing to make the structure of the different logics apparent.¹ We take them to be *translations*. In doing so we refuse to be scared off by the analysis of David Schneider (1984), which we consider to be extreme and, in the end, self-defeating, while acknowledging the impact his intervention has had in the promotion of a cultural approach at the expense of a mathematical one. If extension rules of the kind Lounsbury invented serve the purpose of shedding light upon the problem Morgan posed, by creating translation rules that properly predict kinship terms in systems with crossness and skewing that culture-bearers would affirm, they can hardly lack cultural appropriateness in any meaningful sense. Just as we would presume equivalence of some kind among different analyses that lead to correct results, we think the same would hold in respect to correctness of results as between a kind of formal analysis and the users of a given kinship terminology.

Let us close this consideration of Lounsbury with a dissent over the matter of metaphor. Lounsbury had included only human kin in his analysis of Seneca, not the personal beings of mythology, among other things. He is scrupulous in being transparent upon this point:

I should confess at once that I have not included all of the meanings of the Iroquois kinship terms in the tabulation of data given in the paper. Not included, for example, are the moon in the list of denotata of the “grandmother” term, or the thunderers amongst the “grandfathers”, or the earth as our “mother”, or the sun as our “elder brother”. Nor have I included the metaphoric uses of the “brother” and “cousin”, “father” and “son”, “elder brother” and “younger brother” terms, in ceremonial discourse, for divisions of the Longhouse and of the political confederacy of the Six Nations; or that of the “uncle” term for the Bigheads (certain masked dancers at the Midwinter ceremonies) or, formerly, for prisoners at the stake. There is no difficulty here in identifying these as “marginal” or “transferred” meanings, to use Bloomfield’s terms.

Hallowell notably (though not in reference to Lounsbury) took the other tack, choosing to regard “other-than-human persons,” such as thunderers and others whom one may encounter only in dreams, as on the same ontological plane as human persons for the Ojibwa (1960). McKinley, in a classic paper, agrees with this, and argues against Schneider that social kinship uses biological kinship as a metaphor; kinship is metaphor all the way up, one may say of his view (2001).

Writing more directly in criticism of Lounsbury, James J. Fox published an analysis of the kinship system of the Rotinese of eastern Indonesia and Timor couched as an alternative to Lounsbury’s approach, though a respectful one.² It is called “Sister’s child as plant” (Fox 1971), and it thickly interprets the role of mother’s brother toward the sister’s child who is conceptualized as a kind of plant that has to be nurtured by the mother’s brother. Instead of limiting the object of analysis to a bare vocabulary of kinship terms, relating only to human beings, Fox explains the social-structural, ritual and poetical aspects of the relation in great and loving detail. Briefly, a person must have a singular mother’s brother, called the “stem mother’s brother” or “mother’s brother of origin” (the plant metaphor of a tree trunk) to serve five ritual performances from marriage to death. The mother’s brother will in the normal case be older than the individual in question,

¹ The University of Arizona Press impressed on us the importance of keeping the conference book accessible for students, which led us to shape the text with pedagogy in mind; hence the English-language renderings in the book of Dravidian, Iroquois, Crow and Omaha terminologies. This work has convinced us that pedagogy and translation for purposes of understanding across cultural difference is what is in play here, not biologism.

² Fox (1971: 219) calls Lounsbury’s Seneca Iroquois paper (1964a) “a clearly important and deservedly reprinted paper.”

so that should the mother's brother die before the sister's child, as will often occur, it is necessary to have a succession from among a large category of mother's brothers. In the general case the determination of the "mother's brother of origin" will be the mother's eldest brother, and his successor will be his eldest son; but the large class of mother's brothers from which this singular individual is drawn will be the whole patrilineage of the mother. From his analysis we may say, as Fox does not but as will be evident to someone who has read the classic articles of Lounsbury, that the Rotinese kinship system has both crossness ($FB = F$; $MZ = M$) and Omaha skewing ($MBS = MB$). The abstraction of these from the thick description of the relation and Fox's rendering of the liturgy of the ritual performances in which the mother's brother of origin acts, with its abundant imagery encompassing many species of plants, is immensely rich. Fox's expressions of respect for the Lounsbury articles in question seems to imply that he took his approach and that of Lounsbury to be, not as right to wrong, but equivalent in some sense; more exactly, as thick to thin. The same may be said, we think, of the approach of Hallowell and McKinley.

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