REPLY

German V. Dziebel
Ph.D., Independent Scholar
216 Good Hill Rd.
Weston, CT 06883
USA
Email: dziebelg@gmail.com

Abstract: *My reply continues the discussion of Crow-Omaha skewing, Alternate-Generation equations, Bifurcate-Collateral and Bifurcate Merging kinship terminological types in the contexts of the contributions by Trautmann & Whiteley, Read, Parkin, Lea and Ensor. Special attention is given to the logical pitfalls in the definition and usage of the notion of "crossness" and to the need to re-focus on a more accurate notion of "merging." Empirical evidence for the transition from Alternate Generation equivalences to Crow-Omaha and from Bifurcate Collateral to Bifurcate Merging is revisited. Further information is provided regarding correlations between Alternate Generation equivalences and Crow-Omaha skewing, on the one hand, and patterns of sibling and cousin terminologies, on the other hand. Among the topics of general methodological and theoretical interest, my reply specifically addresses the scope of kinship studies and the methodology of integrating anthropology and linguistics in the study of kinship terminologies. Finally, the author presents an update on the "Out-of-America" theory of human kinship evolution in the light of recent advances in population genetics and ancient DNA analysis.*

I am honored to have received several thoughtful and substantive comments on my critical article addressing the treatment of the Crow-Omaha problem in Trautmann & Whiteley's volume *Crow-Omaha: New Light on a Classic Problem of Kinship Analysis* (Trautmann & Whiteley 2012). They are symptomatic of our shared passion for advancing solutions to the problem. Disagreements on what those solutions are continue to exist, and one just wants to hope that they will be productive enough to generate better ideas in the future. In their response Trautmann & Whiteley cross-review aspects of my book *The Genius of Kinship: The Phenomenon of Human Kinship and the Global Diversity of Kinship Terminologies* (Dziebel 2007) and discuss such broader methodological and theoretical issues as the use of linguistic evidence in the analysis of kinship terminologies, the world-historical aspects of kin terminological evolution and the status of our understanding of human kinship evolution in the context of human origins and dispersals. In the

following I will take Trautmann & Whiteley's reply as a backbone for my rejoinder adding reactions to Read's, Parkin's, Ensor's and Lea's comments along the way.

Ensor takes a full step back and looks at the whole debate from a metacritical standpoint. His piece is thorough, honest, rich in perspectives and most welcomed. It invites an extensive response of its own which unfortunately falls outside of the purview of my article and the responses by the other commenters. Just one counterpoint suffices here. I do not consider my work "neoevolutionist" in any way. Evolution and history are part of the "matter" anthropologists have to deal with regardless of their epistemological beliefs. Kinship terminologies objectively fall into types and those types, no matter how numerous and diverse, change according to a certain logic. This is what social science does – it studies the phenomena at hand using a synchronic or a diachronic lens. Post-modernist anthropology (and Ensor's commentary has a ring of it, which is likely unintentional and not representative of his work) has engaged in excessive typecasting of scholars and arguments as "structuralist," "neoevolutionist," "evolutionist," "functionalist"—all with a negative sign. It has politicized kinship studies to a ridiculous point when kinship became a non-subject unless it was described as an expression of political economy, ideology or innate human manipulativeness. The analytical study of kinship terminological systems suddenly became a roadblock to achieving racial and gender equality, and the scholars engaged in it became the de facto enemies of the "natives," the "poor" and the "oppressed." The damage done to kinship studies by post-modernist anthropology is comparable (in function, if not in scope) to the damage done by the Soviet regime to genetics and computer science in the 1950-1960s. They were declared "bourgeois sciences" and dismissed only to provide the West with a huge competitive advantage. Similarly, once a pioneer in building global databases of human variation and modeling the deep history of human culture and society, kinship studies now lag behind typological linguistics and, especially, population genetics. The singular kin-terminological focus of my analysis of Trautmann & Whiteley (2012) does not mean that I reduce kinship studies to one nerdy problem of transition from self-reciprocity to Crow-Omaha. I share Ensor's broad view of the scope of kinship studies. And there is room, in my opinion, for "political economic structure, practice, agency, negotiation, and identities" in kinship studies. I do object to the marginalization of kin-terminological research in contemporary anthropological scholarship. Kin-terminological research is inherently linked to other high-level topics such as lexical semantics, linguistic typology, evolution of language and cognition, the algebra of relations, human origins and dispersals, systems theory. The world of human kinship is much bigger than what is painted in post-modernist scholarship and the centrality of kinship studies to anthropology as a multi-field discipline must be (re-)affirmed.

This is why I praised the Trautmann & Whiteley's volume on Crow-Omaha—it restored the study of a particular kind of kinship terminologies to its rightful place in anthropology. My critique of this book concerned the technical intrinsics discussed in it and its neglect of my work which I believe brought new (or, rather, overlooked) empirical evidence to bear on this old problem.

Horizontal and Vertical Groupings

The thrust of my critique of Trautmann & Whiteley (2012) was two-fold. First, one should not speak of Crow-Omaha skewing as a variety or an evolution of Bifurcate Merging systems. Crow-Omaha systems do not invariably contain crossness. This critique stemmed from an observation made on the basis of cross-cultural databases that Crow-Omaha skewing is found in association

with Bifurcate Merging, Generational or Bifurcate Collateral systems. In their response Trautmann & Whiteley seem to agree with this and they cite Lounsbury's (1964) seminal article that references "nonbifurcate" Crow-Omaha systems (e.g., Trukese). Now, we also have Popov (1977) available in English, and this article provided a cross-cultural perspective on Lounsbury's "nonbifurcate" grouping of Crow-Omaha systems. This tells me that we are now all in agreement on the first point.

As a side observation that emerged upon my reading of Trautmann & Whiteley's response, the term "crossness" refers to the bifurcation of kin categories into "parallel" and "cross" but it does not capture the nuance of whether parallel categories are merged with lineal categories (FB with F, MZ with M, etc.) or are they kept apart. The former situation is called Bifurcate Merging and the latter Bifurcate Collateral. I believe we are not justified in using the term crossness without specifying whether merging occurs or not. If we do so, we reify a considerable diversity of empirically attested kin terminological types under an artificial moniker of "systems with crossness." Such "systems with crossness" are not uniform and occur in either the Bifurcate Merging or the Bifurcate Collateral variety.

Second, Crow-Omaha is a different breed of terminological equations from the ones involved in the definition of Bifurcate Merging, Bifurcate Collateral, Generational or Lineal. I refer to the distinction as "vertical" vs. "horizontal" equations. One is not reducible to the other as they manifest different dimensions of a kin terminological system. As a "vertical" kind of equation, Crow-Omaha skewing belongs to a different family of equations than Bifurcate Merging, Bifurcate Collateral, Generational and Lineal. It belongs with Alternate-Generation equations, Sliding Generation equations, or the "Siberian Generational" type, Stitching Generation equations (contra Trautmann & Whiteley, I don't invent new types but simply give tentative names to the ones that are empirically attested but poorly known), etc. (see Dziebel 2007). "Crow-Omaha" skewing has enjoyed a spotlight in kinship studies but cross-culturally there are a lot more ways in which categories in the generations of grandparents and parents are linked with some Gen 0 categories, and correspondingly categories in the generations of grandchildren and children are linked with other Gen 0 categories. For example, in the kin terminology of the Crow Indians MB = oB (in a Bifurcate Merging context), while in the kin terminology of the Siberian Nenets FyB = oB, while MB = MBS (in a Bifurcate Collateral context).

While the label "Crow-Omaha" selects for analysis a rich sample of kin terminologies, it's important to understand the whole spectrum of diversity of cross-generational equations that exist globally. They are not easily reducible to Lounsbury's 8 subvarieties of Crow-Omaha skewing and the "horizontal" contexts in which they occur are much broader than the Bifurcate Merging type. Lounsbury (1964) who is extensively discussed and praised by Trautmann & Whiteley divided Crow-Omaha cases into "fully bifurcate" (Bifurcate Merging throughout), "semi-bifurcate" (Bifurcate Merging for Gen +1 male categories but Generational for Gen +1 female categories) and "non-bifurcate" (Generational throughout). But even using such a focused lens, Lounsbury missed cases in which Crow-Omaha is found with a Bifurcate Collateral arrangement. This gap was filled in by Popov (1977). Trautmann & Whiteley (2021:4) make an important observation:

> 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).

It is indeed an important evolutionary progression. But the extremes of distribution are formed not by Bifurcate Merging and Generational but by Bifurcate Collateral and Generational. This is where Robert Parkin's puzzlement over my focus on Bifurcate Collateral may get answered. Fully Bifurcate Collateral on mother's and father's sides (with Superreciprocity) switched to Bifurcate Merging on one side (with either Crow or Omaha) followed by Fully Bifurcate Merging and then the sequences of changes would follow the one outlined by Lounsbury. In agreement with Parkin, Bifurcate Collateral, Bifurcate Merging, Generational, Lineal, etc. do not need to apply to the whole kinship terminological system. They can occur on mother's side but not on father's side, in Gen +1 but not in Gen 0, etc.

If we look at skewed systems with Bifurcate Collateral groupings in Gen +1 we will find remnants of Alternate-Generation equations. For example, in Mapuche Omaha-type skewing associated with Bifurcate Merging pattern for matrilates (*wéko* MB, MBS, *ñüke* M, MZ, MBD, *tcákam* mZC, mFZC, *koíñi* C, wZC, wFZC) co-exists with amito- and patruus-reciprocity and the Bifurcate Collateral grouping of patrilates (*pälu* FZ, wBC, *malle* FB, mBC) (Faron 1956). At an earlier stage of the evolution of the Mapuche kinship terminological system, matrilates must have been organized in a Bifurcate Collateral fashion with avunculo- and matertero-reciprocal terms. The whole system was Superreciprocal and Bifurcate Collateral on both sides. Then Gen +1 matrilates underwent a shift to Bifurcate Merging and Omaha skewing. Patrilates retained an original condition.

In the absence of such a fine-grained view of the empirical evidence, confusion, disagreements, self-deception and misunderstandings still reign. The very term "crossness" is not well-defined by Trautmann & Whiteley. It confuses Parkin (2021:3), too ("can one have crossness without the parallelness that it is opposed to?"). Trautmann & Whiteley (2021:16) write, for instance, that "Rotinese kinship system has…crossness (FB = F; MZ = M)" but they define crossness through merging. They forget that cross-relatives are singled out in Bifurcate Collateral systems as well and if we were to use the term "crossness" at all we should apply it to any system that segregates cross-kin (whether it merges parallel and lineal kin is secondary).

Ironically, Trautmann & Whiteley praise the old practice (of which Lounsbury was a representative) of looking at isolated case studies (Fox, Pawnee, Iroquois, etc.) and reifying them into "types" bearing the same name over large-scale databases of actual and complete kinship terminologies:

> [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 (Trautmann & Whiteley 2021:4)

In reality it's precisely a global database approach that allowed me to detect that skewing originates at a pre-Merging, Bifurcate Collateral stage, when kin terminological systems were still dominated by a more ancient way of collapsing generations, namely by cross-generational self-reciprocity. Narrowing it down to a particular exemplary system is totally acceptable but only once the "big picture" has been mapped out.

Trautmann & Whiteley (2021:14) sound triumphant because they found in Read (2018) a confirmation of their belief (as expressed in the book I reacted to) that

> skewing is an overlay upon a system with crossness.

They do not realize that the very notion of an "overlay" fits more with my interpretation of Crow-Omaha than with theirs: "overlay" means it's not of the same kind or order as Bifurcate

Merging and other "horizontal" patterns. It is precisely the origin of this "overlay" in an earlier Alternate-Generation equations that I have sought to prove logically and empirically.

Trautmann & Whiteley (2021:5) counter my critique of crossness with the following statement:

> Crossness is a property which crosses generations, especially the three central (parents' to childrens') and in some cases five generations (grandparents' to grandchildrens'), so to say it is horizontal is to be misled by the (horizontal) pattern of kin type equations within generations.

Crossness undoubtedly can bifurcate categories within any generation. However, my use of the term "vertical" refers, without any ambiguity, to the equations that exist between generations, not within them. The key Omaha equation MB = MBS is an equation of a Gen +1 category with a Gen 0 category. So is an Alternate Generation equation PP = CS (ms) whereby a Gen +2 category is subsumed together with a Gen -2 category under one kinship term.

While my argument that Crow-Omaha is consistent with any "horizontal" grouping (save for Lineal) was accepted, the argument that Crow-Omaha belongs in a completely different family of equations continues to face resistance. This is understandable because the implications of this postulate have far-reaching implications for the origin of Crow-Omaha equations. Needless to say, if Crow-Omaha is not part of a "horizontal" family of groupings, its origins must be sought in its proper family of equations, i.e., in the "vertical" one.

When faced with an argument, which, if proven true, revolutionizes our shared field of study, Trautmann & Whiteley (2012) instinctively reach out to other authors in search for earlier iterations of the same idea and in the hopes that they won't find them. Or they attempt to dismiss my use of prior authors (such as Alf Hornborg's) as misreading. They checked Popov (1977) and did not find the idea that skewing comes from intergenerational self-reciprocity there. This gave them hope that I must be wrong. But Popov's study did not deal with self-reciprocity at all. Vladimir Popov was my scientific advisor at the St. Petersburg State University in the early 1990s. In 1992, he was impressed with my honors thesis conclusion (Dziebel 1992) that skewing originates in self-reciprocity and this has propelled us into years of fruitful scientific partnership and personal friendship. But in 1977 his goals were completely different. As for Hornborg, "Kariera" for him is a special case (or derivation) of "Dravidian", hence the diagram that he drew and that I reproduced illustrates the same insight regarding the self-reciprocal backdrop for Crow-Omaha skewing. It does not mean that Hornborg and I made exactly the same claim but we both seem to view Crow and Omaha skewing as representing two halves of the same alternate-generation "apple" (whether the reciprocal sets were coded in a given terminology as self-reciprocal, as in Kariera/Numic, or not, as in Dravidian/Amazonian, is a secondary matter).

Regrettably in their response Trautmann & Whiteley left the bulk of my empirical examples drawn from multiple language families represented in the Trautmann & Whiteley (2012) collection largely unaddressed. They prefer to talk about crossness and Crow-Omaha in the abstract assuming that those types are well-grounded empirically. They did not fully realize that in Dziebel (2007) I reviewed the totality of available global evidence and found that the actual ethnolinguistic reality behind the labels "crossness," "Crow-Omaha" and others is actually richer than has been assumed (in agreement with Lea's comment) and yields itself to better interpretations than those that are currently available. In my review article as well as more broadly in Dziebel (2007) and earlier in Dziebel (1992;1997), I advanced an empirically-grounded and logically coherent hypothesis whereby Crow-Omaha skewing originated (and were predated within the chronological span of language family diversification) in systems with Alternate-Generation

equations. In other words, Alternate-Generation equations are "older" and represent an earlier, symmetrical version of cross-generational equations. They evolved into Crow-Omaha equations with changing social conditions (such as the emergence of unililneal kin groups). While Trautmann & Whiteley (2021: 13) see "the close juxtaposition of systems with crossness of Dravidian and Iroquois kinds, and skewing" (oftentimes among societies speaking related languages) and they interpret this as evidence for a close causal association between crossness and skewing ("explanations should attend to crossness as the underlying condition of skewing"), I see this too but in addition I take a full view of kinship terminological variation in a language family and observe that Alternate-Generation equations lurk behind Crow-Omaha equations and Bifurcate Collateral grouping lurks behind Bifurcate Merging.

If we look regionally we see that Crow-Omaha systems are (relatively) rare in the areas where Alternate-Generation equations are (relatively) rare. In Australia, there are no Crow systems, very few Omaha systems, and there are very few cases of avunculo- and amito-reciprocity. Papua New Guinea furnishes the opposite picture. South America has some cases of Alternate-Generation merging and Crow-Omaha systems (see Vanessa Lea's comment) but overall both kinds of cross-generational equations are rare. This contrasts with North America where both Alternate-Generation equations and Crow-Omaha are common. If we zoom out to adopt a global view, the cultural practices of personal name and substance inheritance can generate Alternate-Generation equivalences (among Khoisans or Inuits), Stitching Generation terminologies (Munda studied by Parkin) or Crow-Omaha (Ge Indians in South America, see Lea's comment). Whichever way we look at the global evidence, Alternate-Generation equations and generational skewing are intertwined.

I take instances of Alternate-Generation equations as archaisms that sometimes survive in linguistically most divergent branches of a language phylum (e.g., the Ritwan, or Yurok and Wiyot, languages of California in the Algic family) or in areas from where a language spread started (e.g., Cape York for Pama-Nyungan in Australia). In a reversal of this pattern of retention of Alternate-Generation equivalences in the basal branch of a language family, in some case, Alternate-Generation equations are widely found across branches providing a model for the emergence of generational skewing in an outlier branch. This is the case of Crow-type skewing among the Hopi in North America (discussed at length by Trautmann & Whiteley). Whether an endogenous development within Uto-Aztecan (with subsequent outward influence from Hopi to Tewa) or, contrariwise, a product of the "Puebloanization" of the Hopi, Alternate-Generation equations (with elements of Superreciprocity) are found on both the Uto-Aztecan and the Puebloan sides (see Hill 2018). A recent paper by Jane Hill in a collection of essays edited by Peter Whiteley deals specifically with generational skewing among the Pueblo Indians. Unsurprisingly to me (or have I misread Hill?) she (Hill 2018:135) notes,

> The literature on Puebloan terminologies has neglected a second type of equation found in almost Puebloan kin-terminologies between generations G^{+2} and G^{-2} and, less commonly between G^{+1} and G^{-1}.

In fact, Towa kinship terminology described as having "odd, partly skewing equations" (Hill 2018:143) provides a glance into the mechanics of a shift from Alternate-Generation equations to generational skewing. The term *tomu* covers kintypes MB, ZC, MBS, FZS, so it has both old self-reciprocal (MB = ZC, MBS = FZS) and new, skewed (MB = MBS) kintypes. In more "normal" or advanced skewed terminologies, cross-cousins will be segregated and self-reciprocity purged from the system and an ethnographer will record a classic "Omaha" system.

This process is fully consistent with the Hornborg diagram (adopted in Dziebel 2007: 243) illustrating reciprocal sets truncated on the maternal or paternal sides to generate Crow or Omaha skewing.

An empirically grounded approach to the origins of Crow-Omaha skewing is essential for the goal of differentiating between abstract analytical models and historical reality. Both Parkin and Read advance fine logical arguments for how a non-prescriptive, Iroquois-like (or a symmetric-prescriptive, Byansi-like) system could transform into an Omaha system (e.g., patrilineal descent lineages in the sociocentric domain introduced the rule 'son' of 'brother of mother' = 'brother of mother' in the egocentric, kinterm domain, in Read's interpretation). One might see a connection between Read's approach and John Moore's (cited by Ensor) who believed that generational skewing represents a non-genealogical, "filiocentric", group-based logic derived from the presence of unilineal groupings in society. One might also detect the same insight in a comment made by Terence Turner who I took classes from in Kayapó ethnology and kinship studies at the University of Chicago in 1998-1999 (this may explain my partiality to him as noticed by Vanessa Lea). Turner (2012: 238-239) observed that "societies with Omaha or Crow terminologies will generally be found to constitute hierarchical systems, with a lower level of segmentary units of identical structure, and an upper level comprising a communal framework of collective groups and ritual activities." What is missing from Parkin's and Read's approach is evidence that the addition of a new skewing rule to a unskewed terminology is indeed what happened in the prehistory of Sino-Tibetan, Algonquin or other language families. One might also argue that Alternate-Generation equivalences are a filiocetric, sociocentric, group-based, communal arrangement in and of themselves. They are different from Crow-Omaha in form but identical in function and precede it in history.

Omaha skewing is widely attested across Sino-Tibetan branches (Kuki-Chin, Konyak, Jingpho, Sherpa, Khumbu, Karen, Mishmi, Dafla, Rgyalrong, Sangtam Naga, to name a few), which indicates considerable antiquity. The same can be said of Bifurcate Collateral (without self-reciprocal terms). It's pervasive in Sino-Tibetan. Some of the Omaha systems, e.g., Khumbu, are Bifurcate Collateral (Allen 1976: 571). Symmetric prescriptive systems are rare (Byansi and ancient Chinese are two examples). Superreciprocal systems—unknown at the time of Needham's and Allen's work on the transition from Symmetric Prescriptive to Asymmetric Prescriptive systems—are found as a small cluster in the Bodo-Garo branch (Dimasa, Bodo, Rabha). They split parallel and cross categories in Gen +1 by age, while maintaining self-reciprocity. They are all of necessity Bifurcate Collateral. Comparative linguistic and etymological work needs to happen in order to test the hypothesis that Bodo-Garo systems provide a realistic antecedent to the Sino-Tibetan systems with Omaha skewing (based on more well-studied Amerindian parallels) and, potentially, to the Byansi (also Bifurcate Collateral with age distinctions in the MZ and FB kin classes) and ancient Chinese systems.

Once there is empirical evidence for a transformational path, the analytics of how this happened cognitively can be worked out depending on the formalism used (componential, equivalence-rules, algebraic or else). There is no disagreement that patriclans and patrilineages are correlated with Omaha skewing and matriclans and matrilineages with Crow skewing. The debate is about whether generational skewing was introduced *ex nihilo* in what used to be a system without any cross-generational equations or the presence Crow-Omaha skewing (and other forms of cross-generational equations such as Siberian Generational type or Stitching Generation) implies that at a prior stage in the evolution of this language family, population or society (prior to the

introduction of a lineage system or another external, sociocentric, factor such as a naming system or a marital rule) it used to have Alternate-Generation equivalences.

Of foremost importance are instances in which the kinship terms defining skewed equations in some branches are cognate with kinship terms defining self-reciprocal equations in other branches. Kinship terms involved in Hopi skewing have Uto-Aztecan antecedents (with some phonetic irregularities reflecting ritual usage) and those antecedents are reconstructible with self-reciprocal semantics and are nested among self-reciprocal terms for similar kin categories (Hill 2018). In the Central Algonquian languages Omaha-type skewing is widely attested (Fox kinship terminology described by Tax [1937], used as a model of skewing type 1 in Lounsbury [1964] and recently revisited by Dwight Read [2018] in a Crow-Omaha paper). But as it was discerned by a foremost authority on Amerindian languages, Edward Sapir, back in 1923 (Sapir 1923), the Algonquian terms for MB and MZ, which are "skewed" in Fox, Kickapoo, Menominee, Shawnee, Miami-Illinois and Potawatomi (see Popov 1977:50-51) must be reconstructed as self-reciprocal at a deeper, Proto-Algonquian level. E.g., Sapir (1923: 2) writes:

> Undoubtedly derived from Algonkin **ne-lo-ss* 'my maternal aunt' is its reciprocal **ne-lo-kwa*, **ne-le-kwa* 'my nephew (niece)'….

At an even deeper, Algic protolanguage, i.e., when Yurok and Wiyot kin terminologies are included in the analysis, Alternate-Generation equations appear on the surface (they require less of expert knowledge of the historical phonetics and were likely obvious to speakers as well) and become more abundant (comp. Sapir 1923:51).

Mapuche, Towa and Algic examples illustrate the transition from Alternate-Generation equations to Crow-Omaha skewing quite directly. This should assuage Parkin's disbelief that such a dramatic transformation can happen and hopefully make him re-think his hasty verdict that Crow-Omaha cannot evolve directly from a system with Alternate-Generation equations. As it turns out, there is no need, as Parkin envisages, for Alternate-Generation equations to first disintegrate into a Lineal terminology with subsequent shift of Lineal equations to Crow-Omaha. Amerindian systems show that a system with Alternate-Generation equations morphs into Crow-Omaha seamlessly creating "odd, partly skewed" polysemies and then old terminological equations peter away and new lexical items sometimes emerge.

While indeed Crow-Omaha and Bifurcate Merging may co-vary (and Trautmann & Whiteley [2021:2] are comfortable with this formulation because they write: "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") the reason they co-vary is because they both represent asymmetric tendencies in the kin terminological system that originate in older symmetric structures.

In systems with multiple Alternate Generation equations (I call them "Superreciprocal" in Dziebel 2007) spanning both Gen +/-1 and Gen +/-2, horizontal grouping in Gen +/-1 of necessity becomes Bifurcate Collateral. So, bifurcation/crossness is already there. Importantly, the evolutionary novelty of Bifurcate Merging systems stems not from their bifurcation (of cross vs. parallel kin) but from their merging (of lineal and parallel kin). This is what Lounsbury (1964) defines as the "merging rule." In Bifurcate Collateral systems lineal and parallel kin are left unmerged, hence symmetrical. What has been overlooked by generations of kinship typologists is that in North America (and sporadically elsewhere) Bifurcate Merging systems may co-exist with Lineal Type A (Dziebel 2007:204, Table 1) in which MB equates not with FB (as in the well-known Lineal Type B) but with MZ! In Lineal Type A asymmetricity arises due to the hori-

zontal merging of kin categories on the basis of their laterality (maternal vs. paternal sides), not on the basis of their gender (as in Lineal Type B). Parallel and lineal kin remain unmerged. Lineal Type B is attested among Yurok and Wiyot in the Algic language family in association with self-reciprocity (Sapir 1923:51). This means that, while Algonquian kinship terminologies went the Bifurcate Merging route in the horizontal dimension of grouping, Yurok and Wiyot terminologies opted for a rarer (but structurally coherent) Lineal Type A solution.

Bifurcate Merging and Crow-Omaha co-vary in some pragmatic registers of a language (David Kronenfeld's Fanti example or Schneider & Roberts's Zuni example), in some language families or some geographic areas not because crossness drives skewing but because of a general tendency, which we may consider a diachronic universal, whereby symmetric structures transform into asymmetric ones across both horizontal and vertical dimensions of a kin terminological system. Sometimes this asymmetricity expresses itself as a Bifurcate Merging configuration, sometimes as Lineal Type A, sometimes as Crow-Omaha generational skewing. Crossness/merging and skewing stand in a relationship of correlation and not causation to each other.

In a more radical reversal of Trautmann & Whiteley's beliefs about crossness, one might consider the possibility that it is skewing that in fact drives crossness (or, using a more accurate language, skewing drives merging). Murdock (1970) called out the universality of naming FB as "little father" and MZ as "little mother". He classified this pattern as Simple Bifurcate Merging but, as correctly pointed out by Pans (1989:344) it is in fact "partly bifurcate collateral, because in this case the partial difference between the terms for lineal and collateral relatives cannot be ascribed to any cause other than the influence of the criterion of collaterality." This observation dovetails well with my theory that Bifurcate Merging originated from an ancient Bifurcate Collateral. The worldwide choice of the description "little father" for FB and "little mother" for MZ can hardly be coincidental and must be interpreted literally as a primitive downward categorical skewing whereby an intergenerational hierarchy is introduced into a same-generation relationship (comp. Lat *avunculus*, lit. "little grandfather", as an example of the use of a diminutive to introduce an Omaha skewing rule into the system). At a prior stage, FB and MZ were independent variables in the system naturally linked to their reciprocates (mBC and wZC, respectively) but now they got subsumed under a lineal category (F and M, respectively).

Crow-Omaha Skewing, Sibling Classification and Cousin Terminology

In the previous section I re-visited the problem of relationship between "horizontal" and "vertical" patterns of grouping in kinship terminological systems. Another important dimension of kinship terminologies is the internal differentiation in sibling and cousin categories. Here I must concede—following Parkin's critical remark—that in my treatment of the evolutionary sequence from Alternate Generational equations to Generational Skewing I somewhat neglected the split of cross-cousins into matrilateral and patrilateral subcategories. This is a prominent feature of Crow-Omaha terminologies because, in the vast majority of cases, due to generational skewing either avuncular or amital categories get lowered (and their reciprocals are correspondingly raised) to equate with matrilateral or patrilateral cross-cousins. The reason for my oversight is a belief that the internal subdivision of cross-cousins into matrilateral and patrilateral is not "diagnostic" (Parkin's term) of Crow-Omaha but a mere by-product of the skewing rule. Skewing breaks up earlier self-reciprocity between MBS, MBD, FZS, FZD terms (just like it ruptures Alternate-Generation equations).

The split of cousins into subcategories based on descent lines is even more pronounced in Descriptive kinship terminologies because cousins (cross or parallel) are named using complex descriptive syntagms, so that MBS is literally called mother's brother's son (with further morphological contractions), whereas FZS as father's sister's son. Same for parallel cousins. They are so differentiated because of a particular morphosyntactic principle applied to the whole kinship terminological system. Interestingly, in Sub-Saharan Africa (and to a certain degree elsewhere) siblings are regularly subdivided according to laterality. Among the Bantu, siblings are classified into 'mother's son', 'father's son', 'mother's daughter', 'father's daughter'. Trautmann & Whiteley (2021:3) unduly ignore this empirical fact when they write about Lounsbury's half-sibling rule that it is "universal, so that we may leave it aside." It is not universal and must be put under an analytical microscope.

In Dziebel (2007) I reported on a strong cross-cultural association between Crow skewing and the differentiation of siblings by Relative Sex (RS), on the one hand, and between Omaha skewing and the differentiation of siblings by Relative Age (RA), on the other. Trautmann & Whiteley (2021:12) object that the distinction is not always clear-cut as there are cases (e.g., Hopi) in which siblings may be differentiated by both RS and RA, while skewing is either Crow or Omaha. This is indeed sometimes the case. But even the classic case of Crow Indian kinship terminology illustrates the association I found globally very well. Its sibling terminology has both RA and RS categories (*biˑikˑa* moB, *basaˑare* woB, *matsuˑka* yB, *basakaˑata* oZ, *makukaˑta* oZ, *basoˑoka* wyZ, *basatsiˑita* myZ). It obviously has "Crow" equations (e.g., basbaˑxi FZ, FZD) but then it has an "Omaha" equation, too, as moB = mo//cousin = MB = MMB and yB = y//cousin = mZS (Lowie 1935:20; Lesser 1958:133). Crossness is present as Gen 0 terminology is Bifurcate Merging but, while the "Crow" skew connects FZ with a cross-cousin, the "Omaha" skew connects MB not with a cross-cousin but with an (older) sibling/parallel cousin. This again illustrates that generational skewing operates on its own logic not derived from crossness.

I think (pending further examination) that kinship terminological systems that have both RS and RA in their sibling nomenclatures, while showing only Crow or only Omaha skewing are transitional systems: as matrilateral skewing persists, RA distinctions peter away; as patrilateral skewing persists, RS distinctions progressively disappear. Overall, however, it is highly unlikely that any global sample will contain a Crow system with an RA-only sibling nomenclature or an Omaha system with an RS-only sibling classification.

The reason for the association between RS, Crow and matrilinearity and between RA, Omaha and patrilinearity is not self-evident and requires future research. But it does appear likely that Lounsbury's "half-sibling rule" is not only not universal when it comes to siblings' Laterality but also limiting when it comes to the role siblings' Relative Age and Relative Sex play in the transformation of Alternate Generation equations into Crow-Omaha generational skewing.

Linguistics and Anthropology in the Historical Reconstruction of Kinship Evolution

One of the defining aspects of my work is that I am always looking at problems from a multidisciplinary perspective. Kin terminological systems as a scientific area exist at the intersection of anthropology and linguistics. Modern human origins and dispersals are informed by archaeology, paleobiology, population genetics, linguistics and anthropology. In reference to Dziebel (2007) Trautmann & Whiteley (2021:10) raise a broad methodological issue:

> 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 in-

> form 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.

It is not clear what "selective dependence" or "mid-range" vs. "long-term reconstructions" mean. Trautmann & Whiteley invoke "countless studies" without referencing a single one.

When it comes to kin term etyma, I typically rely on standard, mainstream protolinguistic reconstructions at the level of first-order language families (Indo-European, Na-Dene, Algic, Niger-Congo, Austronesian, etc.) There is a degree of uncertainty and debatability to any protolinguistic reconstruction and that is the natural "cost of doing business" in historical linguistics once scholars have to go deeper in time than what is attested in ancient written sources and in contemporary usage.

When it comes to assessing global linguistic variation I again follow mainstream continental language classifications and typological trends. This means relying significantly more on Lyle Campbell and much less on Joseph Greenberg, on Johanna Nichols and not as much on Merritt Ruhlen, etc. Trautmann & Whiteley cite Whiteley et al. 2019, a fine piece of work offering a different approach to the internal Bantu phylogenetic tree. However, it also shows that the authors' belief in the "pre-scientific methodology" and the presumed subjectivity of historical linguistic methods comes from reading Joseph Greenberg whose unorthodox approach to language classification generated much controversy in the 1990s only to be dismissed by nearly all Amerindianists and to be heavily redacted by Africanists. I had a pleasure of knowing the late Joseph Greenberg and the late Merritt Ruhlen personally. They were scholars of immense intelligence and integrity that left an indelible mark on multiple areas of inquiry. If properly married with traditional comparativist methodology, their approach to linguistic taxonomy yields progress in long-range linguistic reconstructions and classifications (see Edward Vajda's work on Dene-Yeniseian). However, if taken in an uncritical isolation from the mainstream, Greenberg's and Ruhlen's approach leads to massive false positives in different parts of the world and has been heavily and extensively criticized. Having been relatively close to some of those debates in the 1990s I examined the issue from different perspectives and have aligned with mainstream linguists.

There is indeed a body of literature, which Trautmann & Whiteley may have in mind when they speak of "countless studies," that applies data analysis and computer simulations drawn from biology to the modeling of language diversification within first-order language families. One of the metascientific thrusts of these studies is that they bring advanced tools to an area of inquiry long dominated by "primitive" word comparisons. An example of such an approach in the area of Indo-European linguistics is Bouckaert et al. (2012). But they, too, were heavily and justly criticized by mainstream linguists (see Pereltsvaig & Lewis 2015). The main reason for their failure is that primary linguistic material drawn from human speech and writing (unlike, say, the molecular material drawn that population geneticists work with) is highly intricate and cannot be easily understood through visual inspection or computer automation. It does require years of training, craftmanship and analysis. In this sense a linguistic reconstruction is akin to an archaeological dig. The most sophisticated software will do more damage to a linguistic reconstruction than an excavator would do to an archaeological site.

This does not mean that traditional comparativist methodology is flawless. It is not. In parallel and in conjunction with my global research in the patterns of evolution of kinship terminological systems I have been working on critiquing Indo-European and Nostratic reconstructions

(Dziebel 2000a, 2006, 2009b), sharpening comparativist methodology and experimenting with new etymologies (Dziebel 2000b, 2009a), re-comparing the Indo-European lexical corpus (Dziebel 2016, 2021) and, finally, advancing new rules of phonetic change, new cognate sets and new etymologies (Dziebel 2022) for the basic Indo-European vocabulary. Traditional comparativist methodology is indeed caught in a vicious circle whereby "objective" protolinguistic reconstructions are based on subjective and naïve practices of grouping words into cognate sets. The moment different semantic or visual-similarity criteria are utilized, the composition of a cognate set changes and this leads to a different etymon for the set. Where software could indeed help is in simulating multiple possible "worlds" of cognate sets in a language family, with different semantic and formal properties, and assessing the likelihood of each one of them to be a necessary and sufficient cognate set. This is the nexus of subjective judgment powered by tradition and inertia in historical linguistics but that is not where computer methods have so far been applied. Whiteley et al. (2019:5) write:

> For basic vocabulary, expert judgment is indispensable for identifying similarity for both morphemes and meanings (the source data used herein are equally expert products at this level). At least potentially, such similarity judgments are independently verifiable, as representing speech recorded in the field. However, the next common step—establishing underlying cognacy, and reconstructing ancestral proto-forms—is typically more opaque.

In my opinion, reality is the opposite from the picture portrayed by Whiteley et al. Expert judgment in the area of identifying similarities for "morphemes and meanings" has never been established in historical linguistics. Similarity judgments are not independently verifiable because objective formal and semantic variation (a meaning change here, a consonant deletion there) may obscure cognacy calls for many (if not all) observers. This area is still steeped in the naivete of old Roman grammarians. The practices of reconstructing ancestral protoforms, on the other hand, are perfectly rational and have been perfected by generations of scholars.

An Out-of-America Scenario of Modern Human Dispersals

In their response Trautmann & Whiteley touch upon the out-of-America hypothesis for modern human origins and dispersals that I introduced in Dziebel (2007). Their description of it as "idiosyncratic" (Trautmann & Whiteley 2021: 10) is fair considering that it has largely been advocated for by a single scholar (although there are some remote antecedents) and radically contradicts the mainstream theory of the origin of modern humans in Africa (some 200,000 years ago) and the recent peopling of the Americas (between 15,000 and 20,000 years ago). I also have fond memories of my correspondence with Tom in the late 1990s after his talk at Stanford University when he called the out-of-America idea "preposterous."

The out-of-America hypothesis is the youngest among models of modern human origins and dispersals and it is hard to expect a young theory to have the same degree of elaboration as theories that have been around for decades. Hence, some of the critical remarks by Trautmann & Whiteley (e.g., regarding the far-fetchedness of correlations between sibling terminologies, linguistic diversities and maps of genetic variation), although perfectly legitimate, mistake raw and directional observations for final and robust conclusions. One specific comment by Trautmann & Whiteley (2021:11) deserves a in-depth response. They write:

> 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.

In support of their thesis, they reference most recent syntheses of new evidence for modern human dispersals, namely Grouwcutt et al. 2015 and O'Connell et al. 2018. However most interesting developments in the field are often found not in general overviews of the state of the art but in specific research papers. Trautmann & Whiteley have neglected to mention that since the publication of *The Genius of Kinship* in 2007 the field of modern human dispersals has been revolutionized by paleogenetic (ancient DNA, or aDNA) research. For students of human kinship, this is a significant milestone because paleogenetics is paleobiology equipped with a kinship "code." Without aDNA information (and this comes in a growing number of genetic subsystems such as mtDNA, Y-DNA, autosomes, etc.), the phenotypic resolution of fossil evidence can no more be considered sufficient or reliable in determining our understanding of the ancient human and hominid past. While the Neanderthal species was identified back in the 19th century using only phenotypic characteristics, the Denisovan species has so far left behind meager fossil evidence (a finger and two teeth) and only aDNA extracted from those samples allowed for the identification of this new hominid species. The historical reality of the Neanderthal species is confirmed by 300+ fossils steadily accumulated over nearly 200 years, while Denisovans have been invisible during all this time and would have remained unknown if not for the new aDNA extraction technology.

Prior to the paleogenetic revolution, one of the major human origins debates in the 1980s-1990s centered around the notions of "anatomically modern humans" (AMH) and "behavioral modernity." According to one of the best-argued theories of that time (Klein 1995), humans became anatomically modern by 200,000 YBP (as evidenced by a slew of African fossils). This is what paleobiology told us. But they became behaviorally modern only after 50,000 YBP and the transition from anatomical to behavioral modernity was rapid in pace and resulted in an expansion of a new, encultured species from its cradle in Africa to the rest of the world. This rapid enculturation of the AMH and the colonization of the Old World by behaviorally modern humans can be observed archaeologically because lithic toolkits and resource management strategies changed dramatically in several parts of the world between 50,000 and 40,000 YBP.

While out-of-Africa indeed remains the mainstream theory of modern humans, the paleogenetic revolution introduced a subtle but dramatic change into this scenario. We have firm aDNA evidence showing that different continental groups of modern humans, while being *genetically modern* (i.e., falling into the same haplogroups as identified in modern populations), carry derived alleles shared with Eurasian hominins (Neanderthals and Denisovans). The Neanderthal signal has been detected in modern African populations as well (Chen et al. 2020). Another surprising but consistent aDNA result is that Neanderthal Y-DNA and mtDNA lineages from samples of 100,000 YBP and younger are closer to those of modern humans than to those of Denisovans (Petr et al. 2020). This may mean that not only do Neanderthals and modern humans share parts of their autosomal DNA, but they descend from a single genetic ancestor on paternal and maternal sides. Anatomical evidence can be phylogenetically misleading: modern humans are anatomically closer to orangutans (Grehan & Schwartz 2009) but are genetically closer to chimpanzees. In the absence of aDNA from African AMH fossils (and we may not be able to ever obtain any due to DNA preservation challenges), we do not know if African AMHs were also genetically modern, if they were admixed with African hominids or belong to an entirely different genetic lineage from modern humans. We assume they are genetically modern based on their anatomical traits but we do not really know it. In the absence of direct evidence favoring the genetic modernity of African AMHs, their anatomical modernity may be a product of evolutionary convergence. So, while outside of Africa we have a tight marriage of the new, paleogenet-

ically defined genetic modernity, with hominid continuity, and the old archaeologically defined behavioral modernity, the "anatomical modernity" of African fossils is currently a purely phenetic entity empirically detached from both behavioral modernity and genetic modernity.

Paleogenomics falsified yet another key pillar of the out-of-Africa theory—the serial bottleneck idea. This is because it was shown that in Europe ancient populations (e.g., Mesolithic Loschbour) were less genetically diverse than modern populations and that modern European genetic landscape was formed through admixture between several ancient populations (Lazaridis et al. 2014). This means that higher levels of diversity found in modern populations (and it is most extreme in modern Africans) is due not to their age but to other factors such as admixture between once-isolated populations and a larger effective population size.

Low levels of genetic diversity are especially pronounced in Neanderthal samples. Among modern human populations, Amerindians yield the lowest diversity values (they are superior only to Neanderthals), which is consistent with Amerindians, rather than Africans or Europeans, having an archaic population structure. This fact is tempting to explain as a return to an archaic state by a small population of Beringian hunters who branched off from a larger Siberian population and experienced a dramatic bottleneck on their way to Tierra del Fuego. This interpretation, however, runs into a number of hard paleogenomically validated facts. Amerindian variation is not just an offshoot of Asian variation. The Amerindian genome is peculiar in its relatively equidistant relationship to Asians and Western Eurasians. Modern Asians are clearly different from Western Eurasians, while Amerindians are between the two. In addition, aDNA and whole-genome studies of modern Amerindian populations of South America (Amazonia and the Pacific Coast) detected a consistent signal of shared ancestry with Australasian populations (Raghavan et al. 2014; Skoglund et al. 2015; Castro e Silva et al. 2021).

The pattern of a dual (and even triple) link between Amerindians and Old World populations begs for an explanation. The dominant explanation for an intermediary position of Amerindians between Asians and Western Eurasians invokes admixture between Asians and Western Eurasians somewhere in Siberia prior to the migration to the Americas. This, however, is hardly possible because of the other peculiarity of Amerindian populations that I mentioned above—they are genetically very homogeneous. Admixture between two heterogeneous populations would not yield a homogeneous outcome. An answer to an intermediary position of Amerindians between Asians and Western Eurasians must be sought elsewhere. An ancient population with an Amerindian genetic profile was identified in the Mal'ta boy sample in Siberia dated at 24,000 YBP (Raghavan et al. 2014). While it is possible that there once was an ancient population in Siberia that survived in the New World but in Eurasia it got replaced by later populations movements, one cannot exclude a more parsimonious scenario, namely an out-of-America migration. And this is exactly what Raghavan et al. (2014:89) acknowledge because they write:

> Thus, if the gene flow direction was from Native Americans into western Eurasians it would have had to spread subsequently to European, Middle Eastern, south Asian and central Asian populations, including MA-1 before 24,000 years ago.

Mal'ta boy DNA challenged mainstream ideas about the peopling of the Americas and the origin of Eurasian populations. Importantly, the earliest modern human sample in Europe, namely Bacho Kiro (Bulgaria), shows affinities with Asians and Amerindians but not with modern Europeans (Hajdinjak et al. 2021). While, as Trautmann & Whiteley (2021:11) emphasize, uncontroversial archaeological evidence of human presence in the Americas remains of relatively recent origin, indirect evidence from aDNA in Eurasia strongly suggests that the Amerindian

population are much older than presently assumed. And another "elephant in the room" that has not made it into the general overviews such as Grouwcutt et al. (2015) and O'Connell et al. (2018) is the fact is none of the earliest Eurasian aDNA samples sequenced to date (Tianyuan at 40,000 YBP, Ust'-Ishim at 45,000 YBP, Peştera cu Oase at 37,000-42,000 YBP, Kostenki at 36,000 YBP, Mal'ta at 24,000 YBP, Bacho Kiro at 42,000-46,000 YBP, etc.) contain any evidence of African ancestry.

African ancestry, as defined by highly divergent, African-specific uniparental lineages, can easily be observed in the descendants of African slaves brought to the New World after 1492. This is a model out-of-Africa migration because it happened during historical times. Specifically, one of the deepest—vaccording to the present phylogenetic trees—Y-DNA lineages with a coalescent time of 338,000 YBP, namely A00, was immediately detected among the Mbo of Cameroon as well as among African Americans (Mendez et al. 2013). aDNA samples from the first Eurasians show none of those uniparental lineages known from modern African populations. Y-DNA and mtDNA haplogroups detected in Tianyuan, Ust'-Ishim, Peştera cu Oase, Kostenki, Mal'ta, Bacho Kiro, etc. all stem from a shared, pan-human set of mutations with some basal markers characteristic of the major non-African macrohaplogroups such as mtDNA R, N and Y-DNA P and C. This is not good news for the out-of-Africa theory. One possible explanation for this pattern is that the conventional trees of divergence of uniparental lineages—the bulwark of out-of-Africa thinking—must have got it backwards, that the "molecular clock" is wrong because different lineages have different mutation rates and that it is the most frequent alleles (and not the most divergent ones) that must be deemed basal in a phylogeny. This new set of assumptions has become the cornerstone of an "out-of-Asia" hypothesis (Zhang & Huang 2019; Chen & Huang 2020), which can be considered a next-of-kin to the "out-of-America" model.

Any theory must be judged by its ability to predict future findings. While out-of-Africa continues to enjoy the status of a go-to theory of modern human origins in Western academia, it has so far not passed a critical test of aDNA evidence. The Clovis-I model of the peopling of the Americas has been proven wrong on the archaeological grounds. To date, no Siberian/East Asian technological complex has been identified as a likely precursor for the earliest New World sites. Out-of-America remains an idiosyncratic alternative but ancient DNA results collected to date seem to give it more credibility. In combination with the phenomenal degree of linguistic diversity found in the Americas and Papua New Guinea (with language being a hallmark of behavioral modernity), its isolated geographic location (an ideal speciation environment) and slow but steady increase in pre-Clovis archaeological sites in the New World, out-of-America must be taken seriously as a radical but thoughtful alternative to the out-of-Africa model of modern human origins and dispersals. Considering how keen Trautmann & Whiteley are on critiquing historical linguistics for some deeply embedded "pre-scientific" beliefs, they may find it worthwhile to apply the same lens to the dominant perspectives on the peopling of the Americas. We need to bear in mind that the idea of an Asian origin of American Indians originated in the writings of a Jesuit missionary, José de Acosta, and had a strong political and religious backdrop to it (Gemegah 2005). Based on my reading of the totality of interdisciplinary evidence (including evidence from kinship terminological systems), I prefer to leave open the question of whether subsequent scientific research authentically verified de Acosta's insight or was retrofitted into one naïve and pre-scientific idea.

The term "Out-of-America" has yet another, more narrow meaning in my work. It encapsulates the need for kinship theorists to switch from Old World-centrism to New World-centrism in their interpretations of kinship terminological evolutions. One of the reasons Parkin cannot see

how a system with Alternate-Generation equations could shift to Crow-Omaha is because his (and his teacher, the late Nick Allen's) areas of expertise are/were South, Southeast Asia and Europe. Amerindian kinship terminologies are more conservative and diverse than Old World terminologies. They lay bare the generative role of such variables as Relative Age and Relative Sex, Speaker Sex, Self-Reciprocity and Bifurcate Collaterality, which are regressive in the Old World, while providing a myriad of iterations and attestations of terminological change.

Reconstructing a Proto-kinship Terminological System of Homo sapiens sapiens: Dravidian or Kariera?

Over the past 30 years, population genetics has provided a new foundation for modeling the deep prehistory of our species. Having made a foray into the new developments in the modern human origins and dispersals, it is now time to come back to kinship studies and revisit some of the issues of intrinsic concern for the students of human kinship in the light of broader discoveries in the field of human origins.

We now have empirically grounded reasons to believe that Mid-to-Late Pleistocene hominid and human populations lived in small (30-150 breeding adults), isolated demes. One deme was genetically vastly different from another deme but the internal genetic variation within a deme was small. Neanderthals and Denisovans are an extreme example of such a population structure. Some Neanderthal demes (e.g., Altai [Prüfer et al. 2014], El Sidrón, Spain [Ríos et al. 2019]) were prone to inbreeding, while others (e.g., Vindija in Europe [Prüfer et al. 2017]) had more open mating practices. Several Neanderthal samples in Europe and Asia from 60,000-50,000 YBP contain molecular evidence—however inconclusive (Vigilant & Langergraber 2011)—of patrilocal residence (Lalueza-Fox et al. 2011; Gibbons 2021). Ancient modern human samples show that Homo sapiens sapiens were more outbred than any Neanderthals. Among modern humans, Amerindians (as exemplified by Karitiana) show levels of heterozygosity that is the closest to the Mid-to-Late Pleistocene model represented by Neanderthals and Denisovans (Prüfer et al. 2014:SI, Fig. S9.1.), although even they show a dramatic increase in population size and interpopulational gene flow compared to Neanderthals and Denisovans. This population increase continues as one moves further away from the New World. Africans are located on the very opposite pole of the heterozygosity distribution. (The distribution of sibling terminologies, as shown in Dziebel [2007] shows exactly the same geographic trait distribution cline, with more complexly differentiated sets concentrated in the New World and Papua New Guinea and with a single term for sibling most frequently found in Africa.)

Among social anthropologists and kinship theorists there has always been an assumption that early *Homo sapiens sapiens* had a simple social structure. This assumption has culminated in Nick Allen's tetradic model that Trautmann & Whiteley revisit in their response. They summarize my divergence from the consensus:

> 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, Dziebel sees these as derived (Trautmann & Whiteley 2021:7).

They find it surprising that I abandoned it in favor of more complex models of ancestral kin terminological systems. *A priori* I do not see why primordial small demes could not have had a rather complex system of kinship classification. In the absence of advanced social and political institutions early kinship systems performed a comprehensive and totalizing role in society and hence may have been quite intricate. Cross-linguistically morphological complexity is inversely

correlated with the size of a language community (Lupyan & Dale 2010; Nettle 2012). It comes therefore as no surprise that a small Amazonian isolate uses a Complexly Differentiated sibling terminology, while a large, agricultural population in West Africa has only a single term for 'sibling' in their language.

Ultimately it is not the complexity vs. simplicity that is at stake in our reconstructions of ancestral sapient kinship terminological systems but rather what kind of categorical distinctions did such systems contain. The advantage of sorting through some 2500 kinship terminologies was a realization that Superreciprocity, Relative Age, Relative Sex and Speaker-Sex must be integrated into the "Kariera" and "Dravidian" terminological models, and Amerindian kinship terminological systems provided rich material for such an integration.

In *The Genius of Kinship,* I actually left the reconstruction of a single ancestral kinship terminological system for the future. I could not fully resolve the structural contrast between "Dravidian" and "Kariera" (in Old World terms) or "Amazonian" and "Numic" (in New World terms) poles of kinship classification evolutionarily. Some empirical evidence favors the loss of self-reciprocity on the way to Dravidian/Amazonian. However, sibling classification associated with Kariera/Numic terminologies tend to be reduced to RA, whereas Amazonian systems show more complete sibling sets (all the way to Murdockian "Complexly Differentiated" type, or G-8). Some scholars (e.g., Alf Hornborg cited by Trautmann & Whiteley [2021:9]) derive "Kariera" (or "Panoan" in the South American context) from "Dravidian." The emphasis on adoptive kinship in North America (in North American Bifurcate Collateral systems FB = StF, MZ = StM, with or without Superreciprocity) vs. the emphasis on affinal kinship in South America (MB = SpF, FZ = SpM) add further complexity to the problem of terminological origins because it appears that the evolution of human kinship terminological systems is tied to the evolution of the cultural notions of what kinship is all about.

Since the publication of *The Genius of Kinship,* I became aware of the Chiquitano kinship system in Lowland South America (Adam & Henry 1880). It was brought to my attention by Luke Fleming, now Associate Professor of Anthropology at the University of Montreal. Traditionally set aside as an isolate, the Chiquitano language is increasingly being classified with the Macro-Gê family (Santana 2012, with prior literature). The specificity of the Chiquitano language is that Speaker-Sex and Referent-Sex are encoded on nouns. In Chiquitano kinship terminological system, which is of Dravidian/Amazonian type, grandparent terms are differentiated according to Speaker-Sex (*iñuma* mPF, *napae* wPF, *kiasi* mPM, *nakarïma* wPM). Two of these terms are self-reciprocal: *iñuma* mPF = mCC, *nakarïma* wPM, wCC, which may provide an antecedent for Crow-Omaha skewing widely found among the Gê Indians.

The Chiquitano grandparental set can be contrasted with a self-reciprocal set frequently found in systems of the Numic/Kariera type, e.g., Shoshone *guno* FF, mSC, *hutsi* FM, wDC, *dogo* MF, wCS, *gago* MM, wDC. In both sets Referent Sex is marked for the older relative but not for his or her reciprocal. In the Chiquitano set Speaker Sex plays the same role as Connector Sex plays in the Shoshone set. In a parallel fashion, Chiquitano sibling terminology recognizes Relative Sex (*[ti]bauxi* wB, *kiasi* mZ, *saruki* //Sib), while Shoshone sibling terminology recognizes Relative Age (*bavi* oB, *dami* yB, *badzi* oZ, *nami* yZ) (Steward 1938; Dziebel 2005). One of the Chiquitano terms, namely *kiasi* links mZ and mPM. I coined the term "Stitching Generation" to capture this kind of generational skewing and prior to my discovery of the Chiquitano system I had only global examples involving Relative Age and some of them were dealt with by Parkin (see Parkin 1988) (e.g., Kond [Dravidian] tāda FF, oB, nāna MM, oZ). Clearly, RS-based skewing mirrors RA-based skewing.

The Chiquitano example shows that the classification by RS and RA found in sibling sets c2021onstitutes a systemic structural principle not restricted to siblings alone. One can envision a Complexly Differentiated grandparental type composed of 8 terms recognizing Alternate-Generation equations, Relative Sex and Speaker Sex to mirror the Complexly Differentiated sibling type recognizing Relative Age, Relative Sex and Speaker Sex. A split in these sets, with the neutralization of some distinctions, would yield the Shoshone system and the Chiquitano system. This in turn gives confidence that the clear global cline between the New World and Africa observed in sibling terminologies does provide a microscopic view of the world-historical aspects of kinship evolution that can be mapped onto the population-genetic maps of human dispersals.

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