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COMMENT ON GERMAN DZIEBEL: CROW-OMAHA AND THE FUTURE OF KIN TERM RESEARCH

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Abstract: *Kin terminology research—as reflected in Crow-Omaha and Dziebel (2021)—has long been interested in “deep time” evolution. In this commentary, I point out serious issues in neoevolutionist models and phylogenetic models assumed in Crow-Omaha and Dziebel’s arguments. I summarize the widely-shared objections (in case kin term scholars have not previously paid attention) and how those apply to kin terminology. Trautmann (2012:48) expresses a hope that kinship analysis will join with archaeology (and primatology). Dziebel misinterprets archaeology as linguistics and population genetics. Although neither Crow-Omaha nor Dziebel (2021) make use of archaeology, biological anthropology, or paleogenetics, I include a brief overview of recent approaches to prehistoric kinship in those fields—some of which consider Crow-Omaha—to point out how these fields’ interpretations are independent of ethnological evolutionary models, how their data should not be used, and what those areas do need from experts on kinship.*

Introduction

I was delighted by the invitation to contribute to the debate initiated by Dziebel (2021) on *Crow-Omaha: New Light on a Classic Problem of Kinship Analysis* (Trautmann and Whiteley 2012a). From the moment I first reviewed *Crow-Omaha* I was struck by the mosaic of theoretical perspectives and it lit a spark of hope for a long-overdue transition in kin term research toward contemporary theory. I believe my perspective on kinship—a product of a somewhat unique context—differs greatly from most contributors in *Crow-Omaha*, Dziebel (2021), and most kin term researchers in general. If I were to classify myself I would be 50 percent archaeologist, 25 percent social anthropologist (cross-cultural and ethnohistoric research), and 25 percent biological anthropologist—with the intersection of political economy, kinship, and gender as my center. I have strived to emphasize the importance

of kinship—often Crow-Omaha specifically—from contemporary social anthropological groundings through methods in archaeology (Ensor 2002, 2011, 2013a, 2013b, 2013c, 2016, 2017a, 2017b, 2018, 2020:191-198, 2021a), biological anthropology (Ensor 2021a; Ensor et al. 2017), and more recently critiques of paleogenetics (2021a, 2021b). In this journey I have engaged with audiences having different understandings of what “kinship” means, different degrees of understanding kinship, different materials and methods, different regional perspectives, and above all different theoretical perspectives. Fortuitously, I find myself outside looking in on the different audiences, attempting to clarify basic understandings of kinship in each, which sometimes means calling for redirection in methods of interpretation appropriate to their materials, with the elusive goal of pointing the different approaches toward a common center. This current response—ironically to an audience of experts on kin terminology—is no different. This position has advantages. It broadens one’s perspectives on kinship and also to its treatment. I am attuned the complaints from social anthropologists about archaeological and biological anthropological treatments of kinship. To the present audience’s concern, I am also very familiar with the criticism of kin terminology research by social anthropologists, archaeologists, and biological anthropologists. For this reason, my comments on Dziebel’s article (2021) will probably not be what is anticipated by either Dziebel or the editors and most contributors to *Crow-Omaha*. As I see it from my context, the “Crow-Omaha Problem” is primarily the theoretical lens through which Crow-Omaha terminology is generally viewed, which also biases representations of Crow-Omaha terminology and marital alliances in both *Crow-Omaha* and Dziebel’s (2021) article.

Kin terminology research—as reflected in *Crow-Omaha* and Dziebel (2021)—has long been interested in “deep time” evolution. In this commentary, I point out serious issues in neoevolutionist models and phylogenetic models assumed in *Crow-Omaha* and Dziebel’s arguments. I summarize the widely-shared objections (in case kin term scholars have not previously paid attention) and how those apply to kin terminology. Trautmann (2012:48) expresses a hope that kinship analysis will join with archaeology (and primatology). Dziebel misinterprets archaeology as linguistics and population genetics. Although neither *Crow-Omaha* nor Dziebel (2021) make use of archaeology, biological anthropology, or paleogenetics, I include a brief overview of recent approaches to prehistoric kinship in those fields—some of which consider Crow-Omaha—to point out how these fields’ interpretations are independent of ethnological evolutionary models, how their data should not be used, and what those areas do need from experts on kinship.

Above all, this comment on *Crow-Omaha* and Dziebel (2021) is about the theoretical diversity in kin term scholarship and the disconnections of the more common perspectives with the past few decades of social anthropology, archaeology, and biological anthropology. My intention is to try to nudge kin terminology research—in general, using Crow-Omaha as an example—toward what I think is a more sustainable path that *Crow-Omaha* actually concludes with (Trautmann and Whiteley 2012b:296-297) yet is misconstrued and set aside by Dziebel (2021). As for “deep history” the tools are increasingly shifting toward archaeology, biological anthropology, and paleogenetics, which could benefit from engagement with experts on kinship but not necessarily on the latter’s terms. By covering the wider world of kinship research, I try to identify where more possibilities for kin nomenclature scholarship can be found in the coming decades, requiring a shift in theoretical perspectives that has only just begun.

From my context looking in, here is what I see...

Why is Crow-Omaha a Problem?

The “Classic Problem” of Crow-Omaha kinship is a consequence of the neoevolutionist theoretical perspective. The specific neoevolutionary framework—unlike neoevolution outside kin term research that focused on ecological factors—is a continuation of 19th century unilinear evolutionism. It differs by not invoking the 19th century explanation for transformation (advancement in “morality”). Instead, formalist analysis seeks an underlining principle that predicts all (or at least most) of a system with as few assumptions as possible that leads to a functionalist explanation or generalization—the broader significance (e.g., Lounsbury 1964:351-352). The principle for explanation is the precedent to, if not the cause for, a predictable succeeding evolutionary outcome. Dziebel (2021) critiques the ideas put forth in *Crow-Omaha* that did not adopt his underlying principle—self reciprocal—which he argues is the evolutionary basis for the development of Crow-Omaha terminology.

Neoevolutionism—popular in 1960s social anthropology but not since (e.g., Peregrine 1996:320-321, 2001a)—for whatever reason anachronistically continues as a major guiding theoretical framework for questions and analyses on kin terminology (e.g., Godelier, et al. 1998; Trautmann and Whiteley 2012a). Abstract types or models of ego-centric genealogical classifications are envisioned and debated (e.g., Kronenfeld 2004). The abstract types are not ethnographic cases. Due to cultural idiosyncrasies, few actual communities’ term usage perfectly match a given abstract model and there is cultural variation in which of the abstract type’s elements, and how many of those, are present. Despite their poor representation of real communities—the cause for the rejection of such typologies elsewhere in social anthropology since the 1980s—neoevolutionism requires abstract models. To link the terminology with social relatedness and marriage practices, the abstract types must also rely on structural functionalist associations, which some view as an added layer of speculation (e.g., Ensor 2013b:59-61; McKnight 2004:103-108). The abstract types are placed in an evolutionary sequence: simple to complex (from a Western cultural perspective). Any proposed sequence is logic-based speculation. There exists no deep historical *empirical evidence* for the imagined transformations from one abstract type to another. The proposed sequences are unidirectional but scholars vary in their sequences and entertain reversibility (e.g., Ives 1998:134-136; Trautmann 2012; Trautmann and Whiteley 2012b:293-294). Because neoevolution was also concerned with logic-based speculation on early human societies, the sequences allow an opportunity to speculate on the earliest, which is the simplest abstract type (from a Western cultural perspective). Only by assuming a neoevolutionist framework would comparisons of human hunter-gatherers (unrealistically depicted as simplistic) with primates be of interest and only with this theoretical perspective would Allen’s (2008, 2012) fictitious “Tetradic” model have a purpose—to speculate on the origins of human kinship. Evolutionism also assumes all early human populations had the same kinship practices. However preconceived, once a sequence is adopted, the question then becomes how does abstract type “A” evolve into abstract type “B” and how does “B” evolve into “C”? Or, what would need to happen for “A” to evolve into “B” and for “B” to evolve into “C”? Note how these questions would be unlikely in the absence of a linear evolutionist paradigm.

Actual ethnographic cases come into play as the factual data to explain the speculated sequence of transformations from “A” to “B” or “B” to “C.” A normative model—the product of ethnographers’ summary of individuals’ or inter-community variation in genealogically-viewed classifications or the product of only one or a few ethnographically interviewed subjects’ classifications (Douset 2012; Moore 1988)—is used to represent the genealogical notation of kin terminology for an entire cultural

grouping. This creates the image of bounded, internally homogeneous populations. The normative models are also synchronic; they are not diachronic evidence of change over time. Once adopting a synchronic model for a cultural grouping, a logical assumption on their use for diachronic purposes is needed. The assumption is that a synchronic model of a cultural grouping's normative kin terminology contains both archaic remnants of a former system—clues to the principle sought—in addition to the system used during the ethnographic present—the neoevolutionary outcome of the principle completed or in process. Since most ethnographic cases do not perfectly match the abstract type they may be envisioned as a transformation in progress from one abstract type to another. Now the questions become which elements are carried over from the previous system and how many previous systems can be interpreted from the synchronic model. This is why Kronenfeld's "overlay" concept (Kronenfeld 2012; McConvell 2012) is so useful, though it could be equally useful with any historical-oriented theoretical perspective on change (as opposed to evolutionism). Another problem arises with the synchronic, normative cultural grouping kin term models. Nearly all were recorded after the impacts, displacements, and reorganizations from colonialism (e.g., Ensor 2013b, 2016; McKnight 2004). This begs the question of just how old the interpreted archaic elements really are. Do they represent "deep time" (prehistorical) transformations thousands of years ago, as usually assumed, or changes brought about by recent events? Often, the interpreted "prehistoric" changes turn out to be the product of recent colonial reorganizations or depopulations (e.g., Ensor 2011, 2013b:61-64; Haviland 1973; McKnight 2004). Another common problem is data concordance. For example, we know that the peoples who became the Crow had abandoned the "Crow" system in the 18th century when they migrated into the Western Plains, becoming more mobile and more reliant on hunting and gathering and more agnatic-oriented through horse ownership and trade with Europeans, which is why they adopted bilocality or patrilocality and bilateral descent. Yet Dziebel (2021) ignores history and uses a Hawaiian trait in their 20th century terminology as a clue to the Crow type's evolution; his principle lacks cultural and temporal concordance. As an alternative to ethnographic cases, sophisticated databases are used for more robust, global (e.g., Dziebel 2007) or regional (e.g., McConvell 2012) cross-cultural analyses of association on elements, in addition to mapping their geographic distributions, to interpret the causal principles for how abstract type "A" evolves into "B" and how "B" evolves into "C."

Additional lines of evidence are commonly included. For example, if a society with lineage exogamy and Iroquois terminology also had moieties only for ceremonial purposes, then it is *assumed* that the moieties previously regulated marriages (i.e., classificatory cross-cousin marriage) and that Dravidian was the earlier kin terminology (e.g., Trautmann and Whiteley 2012b:292). Although moieties often have no role in marriage, they do in the "simplest" marriage systems making such an interpretation logically consistent with simple-to-complex neoevolutionary schemes. As another example, Eggan (1934) hypothesized cross-cousin marriage and exogamous moieties for the Pre-Columbian (pre-16th century) Maya based on kin terms recorded in an 18th century Spanish dictionary. Roys (1940) later hypothesized the ancient Maya had double descent because of 16th century records of individuals having both patronyms and matronyms. With the rise of 1960s neoevolutionism, the combining of these reportedly led Lounsbury (in an unpublished presentation I have never seen) to interpret Kariera kinship, which was later used as the model for "Proto-Maya" (e.g., Borodator and Kozhanovskaya 1999; Hage 2003). Although cross-cousin marriage is extremely unlikely in Mesoamerica (Haviland 1973); although Eggan's evidence was out of social context; although Roys' observations actually suggests the Spanish naming system; although both sources were from different times and places; although the Maya

were incredibly diverse in modern, historic, and prehispanic times; and although social organizational studies suggest variable but widespread impacts from depopulation, Spanish reorganizations, and subsequently capitalism, the Kariera interpretation is logically consistent with simple-to-complex neoevolutionism (for a history of ancient Maya kinship research, including its treatment of Omaha, see Ensor 2003b: 15-68).

In reading this literature, I often get confused: is the ethnographic case being used to prove the proposed neoevolutionary sequence or is the sequence being used to explain the ethnographic model? It appears to be both: the ethnographic normative and synchronic models are simultaneously used to construct the specific neoevolutionary scheme and as evidence to support that neoevolutionary scheme. Scholar A uses ethnographic models to logically infer how evolution should occur from one abstract type to another and at the same time claims those same ethnographic models are evidence for the validity of the linear evolutionary sequence. Because of cultural variation in Crow-Omaha kinship, Scholar B—e.g., Dziebel (2012) as one example—inevitably points out other ethnographic data to critique Scholar A’s argument while those same ethnographic data are used to both build and support the alternative scheme. But this does not make the original ethnographic data used by Scholar A disappear. Lacking is an independent means to test the validity of each proposed model. That requires actual observations of one term system becoming another. For example, there is abundant evidence for depopulation, colonial reorganization, expanding capitalism, and dependence on wage labor to break down corporate descent groups leading to the adoption of bilateral descent, bilocality for property owning groups or neolocality for proletarians, and correspondingly Hawaiian or Lineal kin terminology (e.g., Haviland 1973; McKnight 2004), not that these are the only reasons for cognatic practices. The bottom line is that no ethnographers or ethnohistorians to my knowledge have independent data—let alone on a repeatable global basis—that actually evidence transformations into Crow-Omaha kin terminology. As I see it, all of the neoevolutionary schemes to explain Crow-Omaha are equally logically plausible, equally logically refutable, some arguments are more convincingly constructed than others, some are more appealing to a given audience, but none are testable through ethnology.

In *Crow-Omaha*, the “problem” is explaining both crossness and asymmetrical generational skewing. From a neoevolutionary perspective, the question is from what principle or preceding abstract type does both crossness and generational skewing “evolve” from? Using selective ethnographic analogies, the participants make their cases. The extent of contextualizing terminology within social organization and/or marriage varies. Attention is logically drawn to Dravidian and Iroquois for two reasons: Crow-Omaha shares with those bifurcate merging and the particular version of Crow-Omaha adopted for the volume includes crossness (that crossness comes into question below). Being perceived as more complex (not referring to complex marital alliances) the skewing is logically—according to the neoevolutionary model—the evolutionary addition to crossness, or rather, the overlay onto crossness (Kronenfeld 2012). However, this still does not provide the underlying principle for a transformation from Dravidian/Iroquois to Crow-Omaha: i.e., that which causes the skewing overlay. Dziebel (2021) proposes a principle—self-reciprocal. But that also does not tell us what type must proceed and evolve into Crow-Omaha; though I would have thought it obvious that Dravidian/Iroquois and self-reciprocal are mutually inclusive.

Many *Crow-Omaha* contributors contextualize Crow-Omaha terminology within community-specific social organization and marital alliances (though some misrepresent those alliances [below]). Rather than integrating the kin terminology with social organization and marital alliances, Dziebel

(2021) uses numerous examples of kin term elements from different world regions. His argument leaves us with an element of Crow-Omaha—self-reciprocal—serving as a principle to explain Crow-Omaha.

What most of the neoevolutionary arguments within *Crow-Omaha* lack are mechanisms for triggering change, perhaps because the neoevolutionary paradigm simply assumes directional change through mysterious forces will always occur and therefore no explanation is required. That is, neither the underlying principles nor the preceding abstract type are the mechanisms for change. McConvell (2012) provides a solution by deviating from the neoevolutionary scheme. He contextualizes the emergence of Omaha within “downstream spreading”—based on 1) empirical evidence for the geographic distribution of term traits (a historical particularist or diffusionist method), 2) interpreting histories of trait spreading through overlay, and 3) a functionalist narrative on how patrilineal group exogamy during migration serves to establish alliances among the local and migrating groups. Dziebel (2021:19) is only concerned that McConvell did not centralize his reciprocity principle.

Late 20th-Early 21st Century Theory

From my perspective, the often referenced “demise” or “fall” of kinship (Sousa 2003) was really a decline in interest in kin terminology research. That research had by the 1970s become a topic onto itself and was (and still is) mostly framed within evolutionist perspectives. As such, it has little practical value to prevailing interests in social anthropology since the 1970s. Likewise, the “revitalization” of kinship since the late 1990s—also responsible for advances in methods and databases—is perceived by kin terminology specialists who actively responded to the crisis by re-engaging with one another but not with mainstream social anthropology.

Other dimensions of kinship are very much relevant to the theoretical paradigms, interests, and questions of the past four decades of social anthropology where there has been *continuity* rather than decline and resurgence. By the 1960s, functionalist treatments of kinship were emphasizing corporate groups—groups whose memberships collectively share resources (contiguous or dispersed) with members’ rights to those means of making a living (collectively or apportioned). More recent understandings of corporate groups extend to an exclusive source of mutual support for comembers, communal ancestor veneration ceremonies that reproduce the collective group identity, and/or the obligation to sponsor community-wide ceremonies (e.g., Fox 1967; Keesing 1975). The common identity is what Sahlins (2013) much later referred to as “mutuality of being.” Descent was no longer a cultural ideology; but rather, a criteria or avenue for corporate group membership (e.g., Fox 1967; Keesing 1975; Scheffler 2001). Most explanations of kinship practices emphasized social and ecological contexts (e.g., Eggan 1966; C. Ember and M. Ember 1972; C. Ember and Pasternak 1974; M. Ember 1967; M. Ember and C. Ember 1971; Murdock 1949) rather than evolutionism (though the two were not necessarily incompatible). Feminist anthropology contributed a Marxist approach to kinship, as demonstrated in the theme and contributions in *Toward an Anthropology of Women* (Reiter 1975) and other works (e.g., Leacock 1972, 1973). Whereas Dziebel (2007) sees Marxist approaches as unilinear evolution (inspired by Engels [1972] and Soviet-era schemes), most Western Marxist treatments turned to the non-evolutionist *Pre-Capitalist Economic Formations* (Marx 1964) perspective where kinship was central to social relations of production with contradictions leading to crises but without predetermined transformations (e.g., Gailey 1985; Gailey and Patterson 1988; Ensor 2017a, 2018). By the 1980s, kinship was decidedly the substance of social relations of production—a major theme in multiple contributions to *Marxist Analyses and Social Anthropology* (Bloch 1984) and across social anthropology. “To pursue

the relations of production to their heart only to find structures of kinship is by now predictable” (Modjeska 1982:51). “Put simply, through kinship social labor is ‘locked up,’ or ‘embedded’ in particular relations between people” (Wolf 1982:91). Since the 1990s, kinship was central to the most sophisticated understandings of the social impacts from expanding global capitalism (e.g., Ellison 2009; Hutchinson 1996; Peletz 1995). A greater emphasis on the negotiated gender dynamics of kinship was added (e.g., Blackwood 2007; Dube 1997; Peletz 1995; Sendón and Manríquez 2021; Stone 2010; Tsing and Yanagisako 1983). Political economic structure, practice, agency, negotiation, and identities continue to be the focus. “While studies of kinship as a terminological system and as a symbolic system ‘in its own terms’ have both waned, studies of kinship in terms of social relations among variably situated actors engaged in the practice of social reproduction within broader political economic contexts have become central to contemporary anthropology” (Peletz 1995:366). Whereas neoevolutionism accepts normative synchronic models and calls them cultural “systems,” contemporary theory sees agents negotiating relationships and identities by their different situational contexts. This contemporary perspective is exemplified in the very first article published in the new journal *Kinship* (Sendón and Manríquez 2021).

Though these contemporary trends focus on social relatedness and identities, kin terminology is not entirely absent. I feel safe in suggesting that John Moore would entirely disagree with Trautmann’s (2012:40-41) and Dziebel’s (2021:10-11) normative characterizations of Cheyenne terminology. To address debates over what is the Cheyenne kin term system, he critiques normative characterizations and indicates there was no singular system (Moore 1988). In the process he highlights the problems in traditional genealogical notation and redirects observation to group-based kin terminology (“filiocentric” notation). There were different systems practiced by Cheyenne having different historically-contingent political economic contexts. In the early 19th century, some communities involved in farming and increasingly involved in buffalo hide processing (feminine roles) for the European global fur trade relied more heavily on women’s gender roles and emphasized matrilocality. Disregarding genealogical relations, all people within the localized and collectivized group were simply distinguished as GF or GM in the eldest generation; F or M in parents’ generation; H (husband); Z or younger sibling in one’s own generation; and D and S in the youngest generation. Patrilocality was adopted among mid-19th century northern bands of Dog Soldiers for whom raiding and horses had become important for the survival and wealth of their groups while women’s status declined as a result—as among most Native Americans who adopted raiding or whose men were favored for employment or trade after forced into dependency on those means of survival (e.g., Bonvillain 2001:12, 189-191). Also disregarding genealogical relatedness, all those within these local groups were distinguished as GF or GM in the eldest generation and F or father-in-law or M or mother-in-law in parents’ generation. In ego’s generation, men were distinguished as H, elder B, and the newly invented opposite sex cousin term and women were distinguished as older/younger Z and the newly invented “co-wife” term. All in the local group in the youngest generation were D and S. In the late 19th century, the forced allotment and imposed Anglo nuclear family organization and male inheritance policies fundamentally restructured families, though extended residential groups persisted. The policies created land scarcity and reduced tribal membership so that Euromericans could use reservation lands, and the declining conditions led many Cheyenne to adopt cognatic residence and bilateral descent—just as socioeconomic insecurity and demographic declines usually result in cognatic practices (e.g., Eggan 1966; C. Ember and M. Ember 1972; Ensor 2011, 2013a:281-282, 291-293, 2013b:57-68; Haviland 1973). The local groups varied considerably

in compositions. Some had neither or both sets of grandparents. In parents' generation ego recognized all those who contributed to their socialization as F or M (Hawaiian-like)—disregarding genealogical relations. Those with or without genealogical relations to ego who later joined ego's local group were A or U (Lineal-like). All those in ego's generation—with or without genealogical relations to ego—were siblings (B or Z). Varying residence—the results of varying historically-contingent political economic circumstances—required the manipulation of kin terminology, which had little basis in genealogy (Moore 1988). Elsewhere, Moore (1991) demonstrates how the Cheyenne, just after relocation to the Oklahoma reservation in 1880 with new imposed gender roles, used kin terms to denote social relations of production among genders and age groups. Sibship nomenclature was an “idealized labor schedule.” As another example, McKnight (2004) centralizes capitalism, displacement, and poverty to illustrate the varied negotiation of kin terminology under ongoing colonialism.

Delightfully deviating from neoevolutionism are three chapters in *Crow-Omaha* with contemporary social anthropological perspectives. Coehlo de Souza (2012), Dousset (2012), and Turner (2012) illustrate how skewing is a form of transformative agency within specific social formations—a contemporary sociolinguistic perspective reminiscent of McKnight's (2004) and Moore's (1988, 1991) treatments of kin terminology. The conclusions chapter (Trautmann and Whiteley 2012b) appears to point toward these contemporary perspectives as the direction forward. Unfortunately, Dziebel's (2021) only comments on this promising direction incorrectly associates sociolinguistics (pragmatics) with Kroeber and Sapir's ideationalist perspective that kin terminology derives from language/thought. But Dziebel's characterization is the opposite of sociolinguistics (pragmatics)—the study of linguistic manipulation by social contexts, as exemplified by Coehlo de Souza, Dousset, and Turner who place kin term manipulation clearly within the structure of dynamic social relations.

An Integrated Model of Crow-Omaha Kinship

This section describes Crow-Omaha kinship—as an abstract holistic type—from contemporary anthropological perspective and taking both genealogical and filiocentric perspectives on kin term notation. In the process, I need to correct the misrepresentations of the marital alliances. My basic characterization of Crow-Omaha genealogical notation differs from the standard adopted for *Crow-Omaha* (Trautmann and Whiteley 2012c: figures 1.2 and 1.3) but can better explain the different variants including that used as a standard in *Crow-Omaha*. From a filiocentric perspective, the exercise results in the conclusion that 1) Crow-Omaha skewing is a simple corporate group-based reference system that also takes into account the marriage prohibitions, 2) that group-based skewing *is the cause* of genealogically-perceived crossness, and 3) that some genealogical crossness where it occurs is not group-based but individual-based overlay onto the basic Crow-Omaha system. I think it a better abstract model that simplistically integrates Crow-Omaha social organization, Crow-Omaha marital alliances, and Crow-Omaha terminology. More importantly, the simple principle applied to explain it provides direction on how to approach the realities of ethnographic variability in Crow-Omaha terminology (and any other terminology) if the goal is to explain its elements and expressions in varied idiosyncratic practice.

Social Organization

Crow-Omaha social organization is unilineal and segmentary. In the Crow version, corporate matrilineal extended families are nested within corporate matrilineages, which are nested within corporate exogamous matriclans (sibs). In lieu of matriclans, sometimes the largest group is an exogamous matrilineage. The matrilineal extended families usually hold collective property established by the group's ancestors (that may or may not be apportioned matrilineage property). In general, but certainly not always, matrilocality is adopted to reproduce the membership of the matrilineal extended families, matrilineages, and matriclans—to keep the future members (the children) at their matrilineal extended family estates. The corporate matrilineages often have property (collectivized or apportioned, contiguous or noncontiguous) and perhaps an ancestral ceremonial theme. Exogamous matriclans are the largest scale of corporate groups. They may or may not have collectively owned property but often have specific ceremonial themes and always are a source of comember mutual support and provide an important shared identity. The Omaha version is the mirror image—corporate patrilineal extended families nested within corporate patrilineages, nested within corporate exogamous patriclans. In either Crow or Omaha, people share collective ownership of ancestral resources, have responsibilities to produce with and maintain those resources for the perpetuation of their corporate groups, are encouraged by elders or leaders to procreate for the perpetuation of their corporate groups (with implications on gendered conditions and status negotiation [Ensor 2013b:51-56, 109-113; Stone 2010]), and benefit from the mutual assistance provided by their corporate groups. They do not get these rights from other clans (though some secondary rights in those clans may or may not be extended to them).

At this point I should set the record straight about the Fox. Within *Crow-Omaha*, and citing Tax (1937) the Fox are used as an example of how Crow-Omaha terminology is not always associated with unilineal descent groups (Barnes 2012:74; Trautmann and Whiteley 2012:9). However, like their Potawatomi neighbors in Southern Michigan, the Red Earth People (Meskwaki) had Omaha social organization: patrilineages nested within exogamous patriclans (though seasonal summer residence was sometimes matrilocal) (Callendar 1978:639). There were at least eight exogamous patriclans, one of which was named Fox (Callendar 1978:639). The tribal story holds that “Fox”—adopted by the French for the whole of the Meskwaki people—was the name of the one Meskwaki patriclan whose members the French first encountered in the 17th century (Daubenmier 2003:5). Those people gave the French their clan name. This illustrates the importance of collective clan identity—in fact, even the subclan lineage names were derivatives of their respective clan names (Callendar 1978:639). The Meskwaki—then the “Fox” in the eyes of Europeans—allied themselves with the Sauk (Sac) in the early 18th century and despite being independent politically and territorially (Callendar 1978:636) were subsequently viewed and treated by the US government in the 19th century as one grouping: “Sauk-and-Fox.” Although in the Iowa-Wisconsin area in the early 19th century the Sauk-and-Fox were placed on the same reservation in Kansas, initially maintaining Omaha social organization. One group among the Fox split and returned to Iowa, where they were granted a reservation and after allotment emphasized extended residential units rather than their previous social organization. And that community is where Tax's (1937) ethnography took place. The acceptance of the Fox as not having unilineal descent groups illustrates the problems with normative, synchronic depictions used for evolutionism that assumes indige-

nous people do not change. The perspective fails to consider the historically-contingent political economic contexts that influence kinship variation and change. How many other cases of Crow-Omaha terminology in the absence of unilineal descent groups were likewise impacts of colonialism?

Membership in corporate kin groups—of any kind—is the means by which people make a living, find social support, and structure gender dynamics and conditions. Group-based spiritualities and identities are holistically entwined, socially reproduced, and negotiated within the strategies used for corporate organization. Marriage is needed to perpetuate groups. When adopting matrilineal or patrilineal membership principles (which can include flexible affiliation to accommodate irregular circumstances [Scheffler 2001:120-159]) then exogamy usually becomes necessary. At the same time, exogamy provides socially-beneficial alliances with other corporate groups. Marital alliance systems are the means by which communities cultivate reciprocal or competitive relations among corporate groups. Social organization and marriage are part of the same political economic social formation (e.g., Ensor 2013a: 197-225; Rosman and Rubel 1971). Though Crow-Omaha social organization need not be coupled with Crow-Omaha marital alliances—simple exogamy will suffice—I proceed to illustrate what happens when they do co-occur.

Marital Alliances

Marital alliance theory (e.g., Fox 1967; Lévi-Strauss' 1965, 1969) was introduced within a neoevolutionary framework that has biased the way the marriage systems are viewed and explained. From a neoevolutionist perspective, Crow-Omaha alliances are viewed as “semi-complex”—an evolutionary stage between Elementary and Complex alliances. Therefore, neoevolutionists seek to explain it as having evolutionary origins in Elementary systems. Barnes (2012) and Kronenfeld (2012) characterize the Crow-Omaha alliance model as a system of direct symmetrical exchange. Allen (2012) depicts it as indirect asymmetrical exchange (Purum-like). The only apparent reason for doing so is to speculate on how it might develop from fictitious Tetradic kinship. However, this could only happen by assuming a society with only five intermarrying clans and even then it would only apply to an individual's perspective (e.g., Fox 1967:226). Of course, with 10 clans, as in the case of the Omaha, one would have a more difficult time making such an analogy. Yet Crow-Omaha is not a combination of Elementary and Complex alliances. Crow-Omaha marital alliances are Elementary-like only in the sense that they involve group-based rules. But the rules are proscriptions only—the systems lack the reciprocal symmetrical and asymmetrical prescriptions or cross cousin marriage of Elementary systems that neoevolutionists force upon Crow-Omaha to make it fit their evolutionary schemes. Crow-Omaha marriage is Complex-like only in the sense that individuals' genealogy determines which *groups* are prohibited. But in either Crow or Omaha, the vast majority of individuals within each of the prohibited clans have no genealogical relations to ego. It is not the *individual-based genealogical-only* prohibition of the Complex system. In short, the system shares little with Elementary (prescriptive) and differs considerably from Complex (genealogy-only). A squid is neither a whale nor a fish.

Barnes (1984, 2012) mischaracterizes Omaha marital alliances. It comes as no surprise that his analysis of the Omaha people's marriage practices did not adhere to that which is not the Omaha marriage system. He claims that Omaha marital alliances are *prescriptive restricted exchanges among sub-clan lineages*. That ignores the overly apparent clan-based exogamy and would result in cross-cousin marriage, which is abhorrent to those with Crow-Omaha alliances. The mischaracterization is apparently

based on Lévi-Strauss' speculation on what *might* happen with small populations. But Barnes' characterization is not, and never was, the Omaha type (Fox 1967:225). His characterization is not, and never was, the normative depiction of Omaha marriages (see Fletcher and La Flesche 1905-1906). The same mischaracterization—for prescriptions—is found in Kronenfeld's (2012) chapter, so it is no surprise he too found that the Fanti do not practice what has never been the Crow alliance system.

There are no prescriptions in Crow-Omaha marital alliances. The alliances involve clan *proscriptions* only (Allen 2012:57-58; Ensor 2002, 2003, 2013a:212-216; Fox 1967; Lévi-Strauss 1965). There are at least two rules; both are proscriptions. In the Crow version, one must not marry a member of one's own matriline (matriline exogamy) and one must not marry a member father's matriline. There is sometimes a third prohibition—that one cannot marry a member of mother's father's matriline. In the Omaha version, one cannot marry a member of one's own patriline (patriline exogamy), a member of mother's patriline, and, if there is a third rule, a member of father's mother's patriline. These three prohibitions subsume most of Barnes' (2012:76) numerous prohibitions he lists for the Omaha. In the absence of clans, the same prohibitions may be used by corporate exogamous lineages. Crow-Omaha marital alliances are simple and easy to follow. All one needs to know is what clan they and their parents belong to. To know how and in what ways to interact with others (or who to avoid interacting with) they only need to know what clan those people belong to. Give your clan name and any person knows whether they can have a conversation, meet again, court, or disengage and hope no gossip saw you together.

The Omaha people in the Late 19th century adhered to the Omaha marital alliance system (Ensor 2003). To begin, I should note that there is no rule for moiety exogamy among the Omaha proper, whose moieties had cosmological, ceremonial, and gendered themes also represented in their clans' ceremonial themes (Ensor 2002; Fletcher and La Flesche 1905-1906). Marriages were independent of moieties—some marital alliances were with clans in the same moiety and some were with clans in the opposite moiety (Ensor 2002, 2003; Fletcher and La Flesche 1905-1906). So, we should rid ourselves of any notion of moiety exogamy and classificatory cross-cousin marriage. My analysis of Barnes' (1984) data on marriages prior to 1883 indicate that 100 percent of marriages practiced clan exogamy (Ensor 2003). My analyses of the 1886, 1894, and 1904 Bureau of Indian Affairs census rolls demonstrated that 96 percent of the 169 marriages among individuals whose clans were identified adhered to clan exogamy—this was at the outset of allotment policies that over time usually lead to violations of marriage prohibitions (Ensor 2003). Among the 42 marriages where spouses' mothers' clans were identified, 93 percent of the marriages adhered to the prohibition against marrying members of those clans. I also confirmed patrilineal descent and patrilocality. Among the 39 widows with children, 77 percent remained in their children's location and 91 percent of divorced women with children remained in their children's location—that of the children's deceased or divorced father's location—regardless of remarriage. This indicates children's patrilineal identities despite allotment's dismantling of some corporate group functions. I also found that 72 percent of first marriages involved patrilocality (deviations were primarily due to a lack of living parents of husbands) and 53 percent of second marriages practiced patrilocality (second marriages are more complicated due the children born from prior marriages) (Ensor 2003). The late 19th century Omaha still practiced Omaha marital alliances, still had patrilineal identities, and generally adhered to patrilocality.

Marital alliances are either reciprocal or competitive, which determines the amount of surplus production, ceremonial investment, and collective group prestige needed to attract marriages. Elementary alliances—symmetrical or asymmetrical—are reciprocal alliances (Fox 1967; Lévi-Strauss 1969). Each exogamous group has a prescriptive marriage pool from which to seek spouses for its members. The reciprocal alliances ensure a means to reproduce the groups and also provide reciprocal access to group resources or territories (e.g., Godelier 1984). Without Elementary prescriptions, there is competition to attract marriages from other groups. At stake is the reproduction and perpetuation of the corporate groups. With Crow-Omaha alliances each clan's members must find spouses from all other groups, keeping in mind their individual members' prohibited clans. The clans with greater ceremonial prestige or more enviable ancestral resources (collectively owned by the clan or its subclan lineages) attract more marriages than clans with lesser assets. This competition accounts for 1) the unequal ceremonial responsibilities and prestige among clans, 2) the disparities in surplus labor for hosting ceremonies, feasts, and material gift exchanges among clans, 3) the unequal proportions of marriages among clans, and 4) the resulting disproportionate populations among clans (Ensor 2002, 2013a:197-216, 2017a, 2018; Rosman and Rubel 1971). In the case of the Omaha, those clans sponsoring the greater numbers of more important ceremonies grew in population from 1886 to 1904 because they attracted more numerous marriages while those with the least attracted fewer marriages and declined in population (Ensor 2002). Apart from clan investments in ceremonial prestige, investments in resources and surplus production may also cause disproportionate marriages and population growth among clans (e.g., Ensor 2002, 2013a:214-219, 2013c, 2017a, 2018; Friedman 1984). A clan that does not successfully compete to attract marriages may not survive. These are the political economic dynamics of marital alliances—an example of the structure that contemporary social anthropology seeks to address contemporary questions.

I hope readers note that this narrative illustrates how unnecessary is the traditionally sexist 'wife-giver/taker,' 'woman exchange' perspective (e.g., Fox 1967; Lévi-Strauss 1969) that most audiences for the past several decades find repugnant. That traditional way of describing alliances is from an androcentric, husband-only point of view biased by Western notions of marriage and individual men as exclusive inheritors and owners of property, titles, wives, and children. Besides, neither men nor women, or any other recognized gender, are 'given' to another group. They remain participating members of their natal corporate groups, with the exception of some patrilineal societies that transfer wives' memberships to their husband's patrilan as a practice to collectively disempower women (Ensor et al. 2017).

Kin Terminology

Because it is also viewed within a neoevolutionary framework, the characterizations of Crow-Omaha terminological types have been biased by that perspective to make them to appear as an evolutionary outcome of Dravidian and/or Iroquois. Crossness is used as the link between those and Crow-Omaha. I describe here how the genealogical perspective on crossness—and the notion that skewing is overlay on crossness—fails to understand the fundamental differences between Dravidian/Iroquois and Crow-Omaha terminology.

For the purposes of generalizing on kin terminology models, I propose they be well integrated with social organization, marital alliances, and a parsimonious principle based on group references. Getting back to basics, that parsimonious principle is that kin terms reflect who is important to one's life, who

is less so, and what relations are possible. This principle is the plankton supporting squid, crustaceans, fish, and whales. This basic understanding is most easily expressed when we take a filiocentric perspective on group-based kin terminology (after Moore 1988) and consider Heady's (2017) emphasis on the dialectic interaction between terminology and social practice to express identity, which itself is similar to Turner's (2012) objective-subjective cognitive "schema." The following filiocentric perspective—indicating how individuals apply terms not to genealogical relations but to entire groups in which they have a genealogical connection—is actually common to contemporary textbook explanations of kin terminology and is exemplified in *Crow-Omaha* by Trautmann's (2012:43-45) more elaborate presentation of group-based terms by gender.

To illustrate the universality of this parsimonious principle, the following provides examples of non-Crow-Omaha terminology followed by a discussion of Crow-Omaha terminology. For simplicity here, I do not engender Ego, use fewer generations, do not consider elder/younger distinctions within generations or affines. Unless otherwise indicated, I use the kin term codes adopted for *Crow-Omaha* (Trautmann and Whiteley 2012a:xi).

One example illustrating this principle is the co-association of wage labor, neolocality, bilateral descent (writ small through genealogical amnesia), Complex marital alliances, and Lineal kin terminology. Proletarians—those dependent on wage labor—have no productive property of their own with which to make a living. Neolocality is the norm because they own no productive property to retain extended kin or support extended kin. The nuclear family (biological or not) is the residential group. It is noncorporate. It is temporary. Unlike all other kin groups, it does not perpetuate itself across generations. It lasts only half a lifetime or less. Children are consumptive agents rather than productive assets; without corporate group productive property to perpetuate through labor and reproduction. Children are symbolic continuity in individual lines of descent; though upon adulthood, children are usually expected to assist their parents (if the parents can't take care of themselves or if the state does not do so). In the absence of corporate kin groups, bilateral descent is emphasized. Recognized kindreds are small because people gain little from those outside the nuclear family—begrudgingly seeking or providing assistance—because each nuclear family has little to provide and does so at its own expense. So, the most important people are those restricted to the nuclear family who are assigned intimate, generational, and gendered terms: F and M in ego's parents' generation (in this case only two people), B and Z within ego's generation, and once establishing with a spouse a new neolocal residence, D and S only for ego's children. The kindred relations outside the non-corporate, miniature, temporary neolocal group, are lumped by generation. GF and GM, regardless of side, are the most useful—because of their support for their own D or S (ego's F and M). A and U—regardless of side—might provide a lesser degree of assistance in ego's upbringing but not to the extent of F and M, so they are distinguished from F and M. Cousins (C) are of less use to ego's livelihood and usually prohibited by the Complex marriage system making them so useless that all, on both sides, regardless of parallel or cross relations, are equated (merged). Children of B and Z are not in ego's household or ego's responsibility so they are distinguished from D and S as Nephew (Ne) and Niece (Ni). The children of cousins are so unimportant to ego that they are merged with cousins (skewing). In the English variant, cousins are all so equally useless that gender is not even distinguished by the term. Ego may love (or not) these bilateral relations and may even occasionally assist them (or not) but these are beside the point.

The Hawaiian type is similarly explained. With bilocality (especially with bilateral descent), bilateral kindreds are more useful and people recognize more genealogical and affinal relations and generally adopt a Complex marriage system. All genealogical kin are potentially coresidents, all provide substantial support, they come together for temporary action groups, and they all require ego's support but are likely prohibited by the Complex marital alliance system for marriage. All in ego's kindred are therefore assigned intimate, generational, and gendered terms: F and M, B and Z, and D and S.

In addition to Moore's (1988) three examples of Cheyenne group-based nomenclature, we can understand from this perspective why the bilocal Akimel O'odham and Tohono O'odham (Pima and Papago) had elements of both Hawaiian and Lineal terminology. Whereas neoevolutionism might try to interpret a transformation from one to the other, contemporary theory would seek an explanation in the social groupings of the O'odham peoples. Ego applied Hawaiian equations to all those in the bilocal farmstead—e.g., F, M, B, Z, D, S regardless of what type of genealogical relations they had, even if they lacked a genealogical relation to ego—giving the appearance of Hawaiian. However, Ego distinguished any genealogical relations who resided in other local groups as A, U, C, Ne, Ni (Bahr 1983; Dunnigan 1983; Parsons 1928), giving the appearance of Lineal. In these situations, an observer would have a difficult time producing a normative genealogical chart because the specific genealogical and affinal relations in one local group, and for different members of the same local group, may differ considerably. Most importantly, genealogical notation would misrepresent the basis for kin nomenclature.

The Group-Based System

Using the same principle, Crow-Omaha social organization and Crow-Omaha marriage can explain a common variant of Crow-Omaha terminology, which I suggest serves as a better holistic model than the standard adopted for *Crow-Omaha* (Trautmann and Whiteley 2012c: figures 1.2 and 1.3) because it integrates all three. I am in no way claiming this is a "more accurate" type or even the most frequent type cross-culturally. Nor am I claiming that Crow-Omaha Terminology must be associated with Crow-Omaha social organization and Crow-Omaha marital alliances. What I am suggesting is that it is a better explanatory model *when* Crow-Omaha terminology, social organization, and marital alliances coexist, creating a coherent whole. Moreover, we can better understand the different variations of Crow-Omaha terminology by using this model as a basic form underlying all variants.

In this version, the intimate, generational, and gendered terms are assigned to all those within ego's most important group—the clan (encompassing all members of all the subclan lineages and all sublineage unilineal extended families subsumed within the clan). Those within the corporate clan may or may not share collective ancestral resources with ego (with which to make a living), do share mutual responsibilities with ego, do provide mutual support for ego and vice-versa, and usually share collective ceremonial affiliations and collective identity with ego. Ego cannot marry any of them (regardless of the fact that few within the clan share genealogical relations with ego). In the Crow version, all those within ego's all-important matrilineal are M and MB, B and Z, and D and S (Figure 1). The reciprocal terms for B and Z applying only to matrilineal parallel cousins—cannot be understood genealogically but rather as a corporate group-based referencing system. This is because most members are not found within one's kindred network. In the Omaha version, all those within ego's all-important patrilineal are F and FZ, reciprocal B and Z, and D and S (Figure 1).

Skewing is also a clear and simple system for referencing group members. With Crow social organization, ego does not have rights to father's matriclean's groups' ancestral resources for making a living. Ego does not have rights to the collective mutual support of father's matriclean's membership. Nor does ego live at the estate of father's matriclean, subclan matrilineage, and/or sublineage matrilineal family. Ego gets little of importance from father's matriclean. Moreover, in some societies, ego has a guarded relationship with all members of father's matriclean—regardless of the fact that very few have known genealogical relations with ego—because of the group-based marriage prohibition (though ego's few genealogical relations within that clan may be treated differently; a point I will return to below). The easiest way to assign kin references to such a group is by merging them. All of them. The

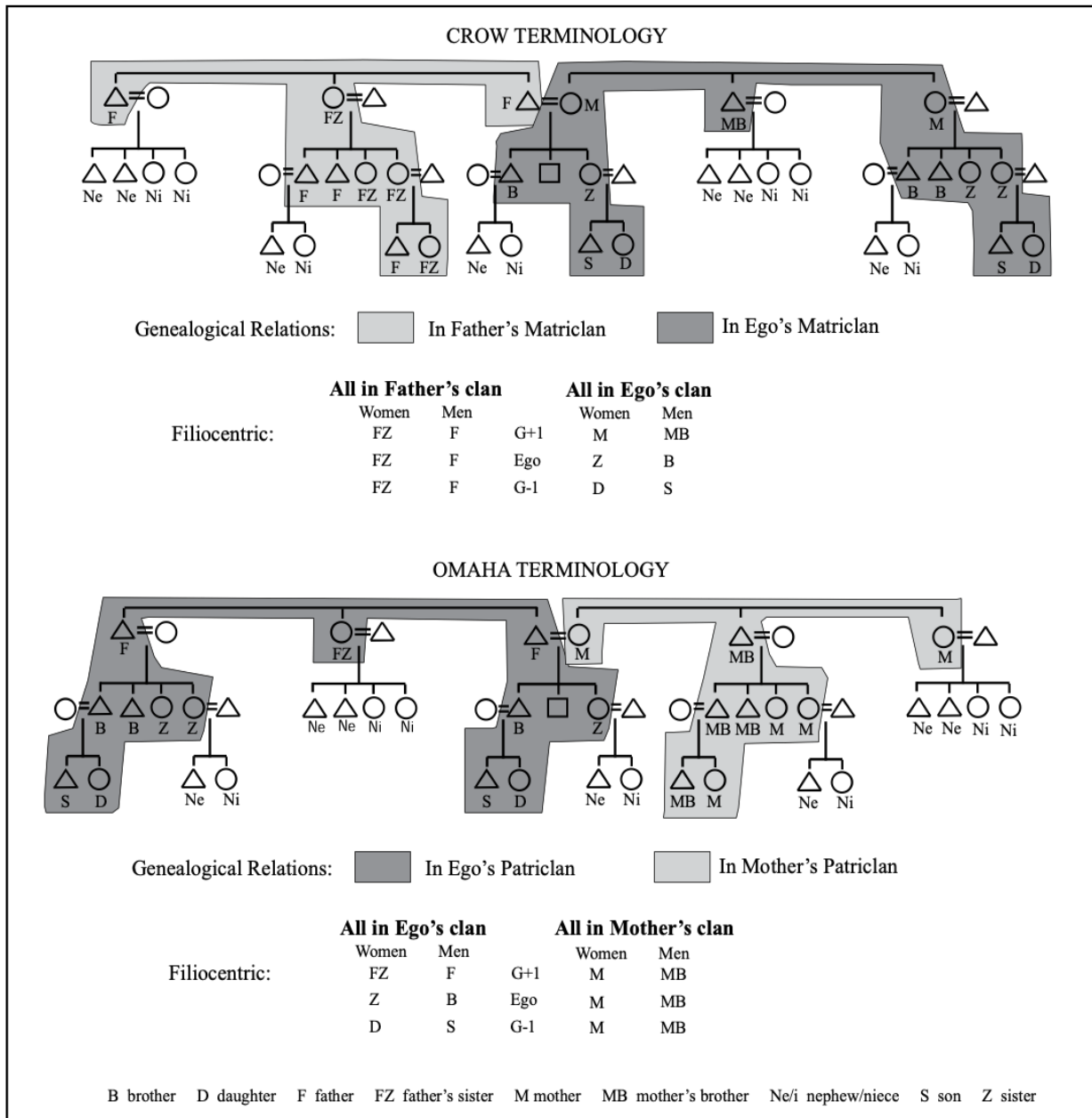


Figure 1. Genealogical versus filiocentric (group-based) perspectives on Crow-Omaha kin terminology.

only distinction is gender. Everyone in ego's father's matriline—regardless of generation and genealogy—is either F or FZ (or A, in the notation used in *Crow-Omaha*) (Figure 1). In the Omaha version, everyone in ego's mother's patriline—who ego gets little from and has a guarded relationship with—is merged. The only distinction is gender. Everyone in that entire patriline—regardless of generation and genealogy—is designated M or MB (or U, in the notation adopted in *Crow-Omaha*) (Figure 1). One could not ask for a more elegantly simple system of kin references.

Additional skewing is observed for third groups—any clans other than ego's matriline and father's matriline in Crow, and any clans other than ego's patriline and mother's patriline in Omaha. In the Crow version, the children of MB belong to MB's wife's matriline, which cannot be MB's (ego's) matriline or MB's father's matriline. Although in ego's generation, MB's children are therefore Ne and Ni. At the same time, the children of FB (F=FB=FBS)—also in ego's generation—belong to FB's wife's matriline—a different third clan. They are therefore Ne and Ni. By the same token, the children of FZS (F=FZS)—in ego's children's generation—who belong to FZS's wife's matriline—another third clan—are Ne and Ni. And, the children of MZS (B=MZS)—also in ego's children's generation—belong to MZS's wife's clan—another third clan—and are therefore Ne and Ni (Figure 1). When viewed in this way, the only crossness in Crow-Omaha is for those in third groups who are skewed. This echoes Dousset's (2012) point that we need to think of crossness and skewing in the same way. The filiogenic perspective explains both the skewing and crossness appearing in the genealogical notation.

Genealogical Marking Creating Variation

I argue that all Crow-Omaha variants have the above group-based nomenclature. To make sense of the different variants (types), we need to consider term applications that are genealogical-only: terms applied to ego's genealogical relations in a group rather than ego applying those to everyone within that group. Those genealogical-only-based classifications provide a variable layer over the base Crow-Omaha model in Figure 1. Examples of genealogical-only classifications are matrilineal-patrilineal parallel cousin equations (crossness)—a common but not universal manipulation of Crow-Omaha. Another example is the equating of matrilineal cross cousins in Crow, and patrilineal cross cousins in Omaha, with D and S (skewing)—a less common manipulation of Crow-Omaha. These layers do not change the group-based system underlying all Crow-Omaha variants. Yet the genealogical-only term applications are not apparent unless we contrast genealogical notation with the underlying Crow-Omaha group-based system. Different communities simply recognize or do not recognize important genealogical relations within a given group—but not the entire group—and therefore mark or do not mark those genealogical relations as different from the rest of that group.

Contributors to *Crow-Omaha* may object to the distinctions above for parallel cousins. In some characterizations of Crow-Omaha kin terminology, both matrilineal and patrilineal parallel cousins are equated as B and Z (e.g., Fox 1967:252-253; Keesing 1975:112-116; Lounsbury 1964; Pasternak 1976:133-135), which is the variant adopted as the standard for *Crow-Omaha* (Trautmann and Whiteley 2012c: figures 1.2 and 1.3) perhaps because this promotes the Dravidian/Iroquois origins and classificatory cross-cousin marriage/ prescription theme in neoevolutionist explanations for Crow-Omaha. Parallel cousin equations are also an example of self-reciprocity that Dziebel (2021) uses as a principle for Crow-Omaha terminology. I should point out that some normative Crow systems for communities do not equate parallel cousins (e.g., for the Choctaw and to a lesser extent the Chickasaw and Creek

[Eggan 1937], along with some accounts of the highly variable Navajo and Cherokee). However, the Omaha and many other normative Crow-Omaha systems do equate genealogical parallel cousins.

According to the principle used here, patrilineal parallel cousins in the Crow version could be B and Z only if they belonged to ego's matriclan. Those parallel cousins' mother—who is not a member of their father's matriclan—would have to be a member of ego's matriclan and that could only occur with direct symmetrical exchanges. Such prescriptions contradict the Crow marriage prohibitions. Interestingly, the Tlingit's normative Crow version did not equate matrilineal and patrilineal parallel cousins despite encouraging elite (aristocratic) individuals to renew the inter-clan marital alliances of their parents—even if they grew up in the same avunculocal residential group (Emmons 1991:27-30). In the Omaha version, matrilineal parallel cousins could be B and Z if they belonged to ego's patriclan, which, again, would require direct symmetrical exchange, violating the Omaha marriage prohibitions. In this integrated model considering Crow-Omaha social organization and marital alliances, $MZCh \neq FBCh$ and $FBCh \neq MZCh$.

Now I should explain how, where it does occur, parallel cousin equations do not violate the principle used here or the filiocentric model in Figure 1. In the Omaha version, the group-based terms for ego's patriclan and mother's patriclan are not changed by parallel cousin equations. But the extension of B and Z to ego's MZCh is not to all the members of MZ's husband's patriclan of ego's generation; it is only extended to genealogical MZCh. *The genealogical notation system ignores by exclusion the group-based terms for members of MZ's husband's patriclan—the majority of that clan—who lack a genealogical relationship to ego.* Although they do not belong to ego's clan, the marriage between ego's father and mother establish strong relationships between ego and MZ's children but not with their entire clan. As Fletcher and La Flesche (1905-1906:313) explicitly state, the Omaha do this because of sororate and levirate *obligations*. Essentially, those genealogical relations would be socially “like” those ego has with B and Z within ego's patriclan. Those equations are only for ego's genealogical relations in MZ's husband's patriclan—not all in MZ's husband's patriclan. Incidentally, I did identify several instances of the levirate among the Omaha—all involving a spouse with children from the original marriage (Ensor 2003:11). In the Crow version, the group-based (filiocentric) terms for ego's matriclan and father's matriclan in Figure 1 are not changed by parallel cousin equations. If B and Z are extended to FBCh it is only to those *genealogical* relations—not to all members of FB's wife's matriclan of ego's generation. Although they do not belong to ego's matriclan, the marriage between ego's father and mother establishes a strong relationship between ego and FB's children—not with their entire clan—and they may very well become like members of ego's clan—from ego's perspective—through the levirate obligation. Moreover, just because ego refers to patrilineal parallel cousins as B and Z does not mean that the majority of ego's clan-mates—who lack a genealogical relation to ego—would refer to them as B and Z.

The genealogical notation system ignores by exclusion the group-based terminologies. In a marriage system with cross-cousin marriage rules or preferences it is often the case that bilateral parallel cousin equations and bilateral cross cousin equations distinguished from the parallel are corporate group-based (or section-based) term systems. A genealogical term schedule does not—on its own—indicate that group-based terminology. But the notion that Dravidian (and Iroquois if cross cousin marriage) terminology is corporate group- or section-based was worked out long ago. If we assume through neoevolution that the same explanation applies to the terminological phenomena in Crow-Omaha then we misunderstand Crow-Omaha. In Crow-Omaha, bilateral parallel cousin equations are

a layer of genealogical-only extensions of group-based terms that do not override that system of group-based terminology. As such, that one genealogical-only extension does not explain a marital alliance system; but rather, individual social alliances established through specific marriages following that system. Only the group-based terms reflect the marital alliance system. When social relations add a layer of importance to genealogical relations in a system that otherwise is based on group importance, then the terminology should reflect both. From this perspective, *skewing is not overlay onto a prior system*. Instead, genealogical-only bilateral parallel cousin equations in Crow-Omaha are overlay onto the group-based terminology of Crow-Omaha. Neoevolutionist assumptions and genealogical notations obscure the nature of Crow-Omaha terminology. Neoevolutionism forces us to use fish and whales to explain squid while ignoring the plankton that explains all marine life.

The standard adopted for *Crow-Omaha* goes further by equating matrilineal cross cousins in Crow, and patrilineal cross cousins in Omaha, with D and S (non-bifurcate) (Trautmann and Whiteley 2012c: figures 1.2 and 1.3). The Omaha did not equate cross cousins with D and S (Fletcher and La Flesche 1905-1906:315-317). None of the types in Whiteley's chapter (2012:86-88) and some types in Wheeler and colleagues' chapter (2012:126) do not equate cross cousins with D and S. Anecdotally, early normative accounts of Choctaw and Cherokee terminology and most of Buchler's (1964) normative systems do not equate matrilineal cross cousins with D and S. In the Tlingits' version of Crow, matrilineal cross-cousins were also not equated with D and S (Emmons 1991:30). In this integrated model considering Crow-Omaha social organization and marital alliances, $MBD/S \neq D/S$ and $FZD/S \neq D/S$. Cross cousin merging with D and S could happen if ego's cross-cousins were members of ego's clan, which could be possible with direct symmetrical exchange, which appeals to neoevolutionism. But the Crow-Omaha marital alliance system prevents such marriages. As with parallel cousin equations, I suggest this form of cross-skewing applies only to ego's genealogical relationships in those third groups—not to all within those third groups—whose social relationship to ego is similar to that of G¹ members of ego's clan.

Crow-Omaha terminology is group-based nomenclature. As an abstract model it is the essence of simplicity. It becomes a "problem" if we approach it from a genealogical-only perspective to make it appear similar to Dravidian/Iroquois and mischaracterize the marital alliances to make it appear like an Elementary system—for the sole purpose of forcing Crow-Omaha into a neoevolutionary scheme rather than understanding it on its own terms. When people have corporate exogamous clans they reference their comembers—all of them—using intimate, generational, and gendered terms because those are the most important social relations and shared identities. Those in other clans—who do not share ancestral resources, mutual obligations, or a common identity with ego—will be referenced with different terms (except, sometimes, for ego's genealogical relations in those clans). If adopting at least the second marriage proscription (making it Crow-Omaha—to prevent concentrating marriages among fewer clans) then all those belonging to the clan of the parent who is not of ego's clan—with whom ego's relations are guarded—are most likely to be classified together through skewing or other means (except, sometimes, for ego's genealogical relations in that clan). If genealogical relations in other clans are important for some reason, then they will be equated with the category of one's group members who have a similar relationship—extending as an overlay onto the basic system the latter's terms to the former.

Variation in Crossness and Skewing

Of course, abstract models, including mine, are often not reflected in many cultural groupings' normative schedules. The variation described among contributors to *Crow-Omaha* testifies to this variation in practice. Often elements of different systems may be inferred. Rather than viewing elements of different abstract term systems within a cultural grouping's normative schedule as evidence to build and support neoevolutionary models—with the assumption that some elements are archaic and others are more recent—contemporary social anthropology would instead attempt to explain the elements of different types by examining the social relations in which they are used (as per Coelho de Sousa 2012; Dousset 2012; Moore 1988; McKnight 2004; and Turner 2012).

Given cultural groupings' idiosyncratic versions, I suspect social explanations require more than simplistic comparisons of a given cultural grouping's normative kin term schedule, normative social organization, and normative marriage system. For example, to explain the use of Omaha terminology among matrilineal extended residences without lineages or clans, Turner (2012) had to explore the shifting social relationships and obligations through the life course. As Dousset (2012) and Turner (2012) demonstrate, in-depth ethnographic investigation into how relationships are formed, used, and reformed by communities and perhaps even age groupings are needed (e.g., McKnight 2004). I encourage kin term scholars to use this approach to explain some major contemporary issues. For example, as unilineal corporate groups increasingly need to recruit members in any way possible to survive the ravages of capitalism (e.g., Ellison 2009), how are Crow-Omaha terms applied to people who are affiliated with unilineal groups but who lack a unilineal genealogical relationship to their comembers? Perhaps a generalization can be made from historical absorption of outsiders through marriage and adoption. And when capitalism does break down the corporate functions of descent groups, what is the impact on how people use the former Crow-Omaha terminology and in what social contexts? An additional line of inquiry might investigate the implications for skewing, if any, comparing patrilineal descent groups that transfer wives' membership (associated with extreme gender inequality [Ensor et al. 2017]) with those that do not. Extending from the conclusions in *Crow-Omaha* (Trautmann and Whiteley 2012b:296-297), attention to the thick of social relations and meaningful groupings—without forgetting the specific historical contexts for them—is a far more productive way to explain kin terminology than neoevolutionary abstract types-on-a-ladder speculation or using elements of a kin term system as underlying principles to explain the kin term system.

Prehistoric Kinship (Deep Time)

The linear neoevolutionary model is one approach to using ethnographic and historic data for speculating on prehistory (or “deep time”). But it is not the only method for using ethnographic and historic data for speculating on prehistory. Dziebel (2021:2) has a curious reaction to Trautmann's statement that “we may hope that ultimately kinship analysis will join with archaeology and primatology to elucidate the deep history of kinship systems” (2012:48). Ignoring both archaeology and primatology, Dziebel accuses Trautmann of overlooking the synthesis of kin term, genetic, and linguistic mapping for phylogenetic evolutionary modeling of deep time—known commonly elsewhere as “The Synthesis.” At any rate, Dziebel states “the progress that has been made in the past 30 years by geneticists and linguists imposes high standards on kinship studies in terms of typologizing, mapping, quantifying and historicizing kinship terminological variation, and it's time for students

of kinship to catch up with these developments” (2021:2). What Dziebel ignores is that the students of kinship have been using sophisticated typologizing, mapping, quantifying, and historicizing of kin term lexicology and semantics for the past 20 or more years (e.g., Allen et al. 2008; Dyen and Aberle 1974; Jones 2003; Jones and Milicik 2011; Korotayev et al. 2019; McConvell et al. 2013), including contributors to *Crow-Omaha* (Ehret 2012; McConvell 2012; Wheeler et al. 2012; Whiteley 2012). Because Dziebel points us in the direction of the Synthesis, and discusses Ehret’s chapter taking this approach, I address that theoretical model here. I briefly summarize the numerous reasons why social anthropologists, linguists, archaeologists, and biological anthropologists object to it, and why kin terminology scholars would do best to avoid it. Because archaeology and genetics are brought up respectively by Trautmann (2012:48) and Dziebel (2021:2)—but addressed by neither *Crow Omaha* nor Dziebel—I then describe approaches in archaeology, bioarchaeology, and paleogenetics—three fields with data actually dating to prehistory. The purpose is to clarify what those approaches actually do (or in the case of paleogenetics what they should do), rather than what Synthesis advocates want them to do, and how kin term scholars may find productive ways to engage.

The New Old Synthesis

Because Dziebel (2021) points us in the direction of the phylogenetic Synthesis, I summarize the numerous objections here that many kin term scholars may not be aware of (or blindly ignore to their detriment). There has been insufficient attention to what exactly it is we are assuming when synthesizing population biology, language groupings, and kinship. The Synthesis Dziebel (2021) appears to call for is the assumption that linguistic populations are biological populations with immutable kinship and that their clustering can indicate where and how long ago they branched from a common ancestor. The origins of the Synthesis are in the racist evolutionism of the 19th to early 20th century that assumed biological populations have immutable social and cultural characteristics—the pseudoscientific foundation for the eugenics movement (Weinstein and Stehr 1999). By studying the relationships between languages, it was assumed that one could understand the evolution of “races” by interpreting the similarities and differences in languages as clades (as per biological macroevolution) (Campbell and Poser 2008:7-10; Trigger 2006:213-241). In the resurrected version today, “race” has been replaced with “genetic ancestry” (or worse, “genetic identity”) but the new label has not changed the concept or its use. And increasingly, we find the assumption that distinct biological populations not only have immutable language but also immutable kinship practices to justify phylogenetic interpretations on deep history (e.g., Dziebel 2021; Fortunato 2010; Fortunato and Jordan 2011; Jones 2003; Korotayev et al. 2019). In response to Jones’ (2003) bio-linguistic-kinship cladistic populations, Wildcat et al. ask why these old ideas re-emerged:

Particularly troubling seems the new return of the old discredited evolutionary assumptions about culture, history, and humankind...it seems to exploit the fact that few social scientists familiarize themselves with modern genetics, and likewise geneticists seem largely ignorant of what social scientists know about the way humans build their communities and imagine the past, as well as how social scientists in turn represent these notions. This mutual ignorance seems to increasingly produce unquestionable mutual belief...Science never occurs in a vacuum. Is it worth asking why this ‘old’ story clothed in the ‘new synthesis’ is so important and to whom does it matter so much? At present, ‘new synthesis’ advocates would do well to more clearly sort out their assumptions, evidence, theories, and knowledge. (2004:641)

First, let us sort out “to whom does it matter so much.” The rise of the New Old Synthesis is best understood by considering the social contexts of its advocates. In the 19th century, the old Synthesis was an ideology justifying European colonial abuses (Trigger 2006:213-241). Until the end of World War II, it was also a tool to promote racial nationalist identities to divert blame for workers’ hardships away from the elite power structure to simultaneously prevent revolutions and unify classes behind aggressive state policies (Trigger 2006:248-261; Weinstein and Stehr 1999). Elsewhere it arose in various countries and at different times whenever there were efforts to create a nationalist identity (Trigger 2006:261-278). Surveys from the 1960s—1980s demonstrate that racial essentializing beliefs were correlated with biological sciences and more broadly by scholars of privileged backgrounds (Lieberman 1965; Lieberman and Jackson 1995; Lieberman and Reynolds 1978; Lieberman et al. 1992; Littlefield et al. 1982). More recent surveys indicate the belief is common in biological sciences but rare among anthropologists and the minority of anthropologists who maintain it are predictably Whites of privileged socioeconomic status (Wagner et al. 2017). In post-Soviet Eastern Europe it is associated with efforts to create nationalist identities. Surveys demonstrate that Eastern European social and biological scientists far more strongly believe in races and essentializing than their counterparts elsewhere (Kaszycka et al. 2009; Kaszycka and Strza³ko 2003). Today—in the midst of another era of rising racial and ethnic nationalisms promoting the same immigration policies of the old eugenics movement on both sides of the Atlantic—we find a return to the same old Synthesis using new tools including genomics and statistical techniques developed for biological sciences. The intent of Synthesis scholars today may or may not be the same as the first eugenics movement’s but the implicit assumptions are the same as the explicit assumptions behind racial and ethnic nationalist ideologies of the past and present.

Now let us sort out those assumptions. For good reasons, the New Old Synthesis has been intensively criticized across social anthropology, linguistic anthropology, biological anthropology, and archaeology (e.g., Armelagos & Van Gerven 2003; Bateman et al. 1990; Campbell & Poser 2008; Clendon 2006; Ensor 2017, 2021a; McKnight 2004; Moore 1994; Sims-Williams 1998; Steele & Kandler 2010). It assumes “primitivism”—that in the time before history (including 20th century indigenous peoples without written histories) human cultures were so simplistic and uncreative as to have been guided by biological evolutionary processes (Moore 1994; Sims-Williams 1998). Indeed, this colonialist mentality on indigenous subjects gave rise to significant resentment toward anthropology by the 1960s. The phylogenetic model unrealistically assumes isolated biological populations with cultural homogeneity over thousands of years, with daughter populations splitting, isolating, and evolving homogeneously over additional hundreds or thousands of years (Moore 1994): the “isolated pristine tribe” mentality. There are relatively few historical examples of briefly isolated communities resisting interaction but those are not analogous to prehistoric peoples who interacted on broad geographic scales (Sims-Williams 1998). There is no reliable way to determine, and even less agreement over speculation on, the timing of the assumed linguistic movements and branchings (Sims-Williams 1998). The population trees overlaid with linguistic trees are dubious taxonomies (with easily disputed categories), which cannot be clades unless ignoring all the evidence for gene-flow and horizontal language spread across the imagined groupings. The more robust linguistic and genetic information from Europe demonstrates little correspondence between the two (Sims-Williams 1998). Even the Bantu Expansion Synthesis (e.g., Blench 2006) unravels in the face of genetic continuities across the presumed clades (Alvez et al. 2011). The model contradicts historic evidence (e.g., there was never a “proto-German” [Moore

1994]) by assuming fewer languages going back in time until all populations spread across a region had only one “protolanguage.” Dixon (1997) points out that if we assume only one language 100,000 years ago, speculations on branching rates suggest the imagined ‘proto-Indo-European’ (PIE) should have spawned 10 million billion billion languages by now. Often, the people within the imagined bounded cultural groupings do not even identify with the Western constructions of their cultural/ethnic categories (e.g., Alejos 2006, 2009). Phylogenetics ignores language death and language shifts for most ancient languages (Campbell and Poser 2008; Sims-Williams 1998). It ignores the fact that 70-80 percent of world’s population and that nearly 100 percent of tribal populations are multilingual, not endogamous, and often form hybrid linguistic groups (Briggs 2002; Brown 1964; Campbell and Poser 2008; Moore 1994; Sahlins 1999; Sims-Williams 1998; Wobst 1978). The New Old Synthesis ignores mixture by assuming a vertical-only transfer of language (akin to the inheritance of Y-chromosomes) rather than the omnipresent evidence for horizontal spreading of languages (Campbell and Poser 2008; Sims-Williams 1998). The Synthesis is also responsible for over a century of identity politics—the Western biologizing of identities for nefarious colonial purposes and other profiteering when indigenous peoples around the world base their identities and ancestries on cultural practices and cultural participation (e.g., Churchill 1999; Moore 1994; Sims-Williams 1998; Stone et al. 2007:232-233).

Even more unsettling is the notion that the imagined bio-linguistic clades have immutable kinship practices. As with any phylogenetics, we are asked to believe that the geographic distributions of kin term phonology or semantics of communities today (or even in the brief duration of written history) can inform us on “proto-kinship” of ancestral societies thousands of years ago having completely different social, political economic, and ecological contexts than their modern descendants (e.g., Holden 2002; Holden and Mace 2003; Holden et al. 2003; Fortunato and Jordan 2010). For example, Fortunato and Jordan (2010) dubiously assign distinct residence practices to entire Indo-European languages: e.g., Hindi, Persian, Ukrainian, and Portuguese were coded as prevalingly virilocal and Spanish and Italian as prevalingly neolocal. Because the languages on the tree were predominantly coded as virilocal, the ancestral state—the interpreted “proto-residence” for PIE—was inevitably going to be virilocal no matter how sophisticated the statistics employed. The assumptions that bio-language groupings each have one homogeneous form of residence that never changed since it branched apart ignores all ethnographic and historical evidence on the social factors creating variation and change. For example, residence strategies in 15th to 19th century central and northern Italy demonstrates enormous variation by socioeconomic contexts—within the same local communities—including some that favored matrilineal choices (Kertzer and Brettel 1987). Likewise, once examined with historical evidence, the dogma of homogeneous German and East versus West European “ethnic patterns” of residence practices falls apart. Practices historically varied by socioeconomic contexts within and across the imagined boundaries (Heady 2017; Szołtysek 2008, 2012, 2015; Szołtysek and Ogórek 2020; Szołtysek et al. 2014). These findings are not restricted to European societies but also to indigenous peoples worldwide (as above). Close scrutiny of Jones’ (2003) bio-lingual-kinship categories reveals that practically all are actually internally heterogeneous in language, biology, and kinship practices. Some geneticists absurdly ask us to believe that haplogroups indicate immutable language and subsistence strategies (Beau et al. 2017; Fernandes et al. 2018; Goldberg et al. 2017; Hervella et al. 2012; Le Roy et al. 2016) when subsistence and population biology are demonstrated to have little influence on language spread (Campbell and Poser 2008:330-363). Moreover, Wheeler and colleague’s (2012) tree productions demonstrate that Crow-Omaha variants do not conform with language groupings. Instead, the clustering indicates similar

social, economic, ecological, and political contexts as well as sociopolitical alliances among those sharing Crow-Omaha variants, which cross-cut language groupings. Notable for its absence is that Dziebel (2021) makes no reference to Wheeler et al.'s *Crow-Omaha* chapter.

All the disproven assumptions underlying phylogenetic evolution are implicit in Ehret's (2012) "reconstruction" of "Proto-Nilo-Saharan" branching, which Dziebel (2021) supports and elaborates upon. Ehret's (2012) "reconstruction" of kin term branching is used to suggest a global scheme of unilinear evolution: Iroquois to Crow, back to Iroquois, Iroquois to Omaha, and ultimately Omaha to Hawaiian/Sudanese/Eskimo, suggesting a relatively late evolutionary origin for Omaha. Again, Dziebel's (2021) only concern is that Ehret did not consider self-reciprocal terms with "clear proto-Nilo-Saharan roots" that he suggests indicate a slightly older development of Omaha and in more branches. Of even greater concern is that both Ehret's (2012) and Dziebel's (2021) arguments are based on etymological analyses of modern (ethnographic) cultural groupings' kin term phonology—with only selected examples of their semantic applications. The same phonological constructions can be applied to numerous different meanings (e.g., Eggan 1937; McConvell 2013; McKnight 2004). Perhaps it is worth reminding readers what serious historical linguistic research concludes on proto-language lexical comparisons for regional and global scales—conclusions equally applicable to kin-term lexical comparisons like those exhibited in Ehret (2012) and Dziebel (2021). "They are an artifact of too much freedom of choice and the loss of control" (Bender 1993:203). "To be credible, geneticists—and archaeologists and linguists—need to admit the uncertainty of our knowledge about the prehistory of even so well-known a linguistic family as Indo-European, and to detach themselves from the 'Neanderthal Loan-words in Proto-World' school of philology" (Sims-Williams 1998:524). In this regard, Campbell and Poser (2008:392-393) comment;

What can we find out or reasonably hypothesize about the earliest human language (or languages) from looking back from evidence in modern and attested older languages? Answer: very little. That is, we can speculate a lot, perhaps even reasonably in some cases, but we can "know" extremely little. What can we find out from lexical comparisons? Answer: essentially nothing...at best a hopeless waste of time, at worst an embarrassment to linguistics as a discipline, unfortunately confusing and misleading to those who might look to linguistics for understanding this area.

Prehistoric Kinship Using Data Dating to Prehistory

Although kin term scholars have access to ethnographic normative nomenclature schedules for cultural groupings (alongside normative residence, descent, and marriage practices) they lack data that actually date to prehistoric times—defined as the time in any region, or for any specific cultural grouping, before there were written descriptions about those populations. For this very obvious reason, to make deep-time (prehistorical) interpretations, they require preconceived evolutionary models with which to organize their ethnographic kin term data. These are modern data in the sense that their ethnographic sources only existed for a century and a half and, depending on the region, are sometimes accompanied with patchy historical evidence (non-ethnographic but derived from historical [written] sources that extend from the modern back to the protohistoric-prehistoric divide). Those evolutionary models are speculations on how human societies change. Those are neither built upon prehistoric kin terminology nor are they tested with prehistoric kin terminology because evidence for prehistoric kin terminology—i.e., before writing—does not exist.

In the interests of improving interfield communication I should point out some phraseology common to kin term literature that archaeologists, and increasingly bioarchaeologists, find objectionable. Most kin term phylogeneticists claim they are “reconstructing” prehistory when in fact their models—no matter how sophisticated the methods—only result in patterning of modern data that are interpreted according to the model’s assumptions. This is not “reconstructing” what happened in prehistory it is *interpretation in accordance with the model*. Some linguists claim that their results—i.e., interpreting similarities and differences as evolutionary clades—constitute evidence for phylogenetic evolution (e.g., Alpher; Bowern [in Clendon 2006:50-51]) but that is an exercise in circular logic. Some evolutionists claim that their results are “prehistoric data.” But again, those are interpretations in accordance with the theoretical model of the resulting patterns among modern data. In general, archaeologists and now most biological anthropologists reject the belief that one can “reconstruct” the past because theoretical perspectives’ assumptions guide methods and interpretation (aside from the issue of data limitations). References to “reconstructing” the prehistoric past by evolutionists who lack data dating to prehistory are even more disconcerting. Furthermore, it might be helpful to make kin term scholars aware that diachronic change observed in archaeology and bioarchaeology has not been considered “evolution” for decades. To these fields, “evolution” implies dismissed notions of nomothetic processes and linear trajectories.

Archaeology, bioarchaeology, and paleogenetics have access to data that date to prehistory. Archaeology lacks kin terminology (with the exceptions of Classical Archaeology, which is historic text-based interpretation of the material culture in literate state societies, and Historical Archaeology, which tacks back and forth between texts and material culture in historical societies). But Prehistoric Archaeology deals with time before writing—our present concern—and therefore bases interpretation of how patterns in material culture reflect social relations using ethnographic analogy, cross-cultural research, and sometimes logic-based speculation. Bioarchaeology has access to patterns in prehistoric biological data that are interpreted according to social anthropological understandings of how societies variably influence human biology (adopting the “biocultural” concept). The high-profile paleogenomics is a new development with the same potential and limitations as bioarchaeology. Importantly, these three fields provide the time depth denied to ethnology. They also call attention to understandings that social anthropologists traditionally neglect, like the varied ways that kinship spatially distributes within and across settlements kin group members, alliances, and kindreds in life and the spatial distributions of biological relatedness in death.

Archaeology

I should first point out two inappropriate uses of archaeological data. First, the New Old Synthesis has sought cooperation with archaeologists but few among the latter have engaged because of their revulsion to the essentialism. The idea is that if language groupings equate to racial groupings, and if each imagined biolinguistic grouping can be mapped out geographically with its movements over time, then archaeologists could use their pottery styles and other materials for the same purpose (e.g., Blench 2006). Thus, pottery styles are assumed to be immutable, homogeneous characteristics for the imagined biolinguistic groupings. Though not uncommon in the past (e.g., Rouse 1992 is a late example), I have only seen this assumption in recent times by Eastern European archaeologists (e.g., Ceka 2005; Pavlù 2016; Schroeder et al. 2019). One problem with this approach is that artifact stylistic attributes are well known to not conform with linguistic, biological, or even ethnic groupings

(e.g., Bowdler; Clarkson [in Clendon 2006:52-53]; Hodder 1982; Moore 1994; Rodríguez Ramos 2010). Geographic distributions of artifact styles tell us a lot about interaction on regional scales but tell us nothing about residence and descent. A second misuse of archaeological data stems from mid-20th century functionalist associations between subsistence and kinship (e.g., Gjessing 1975; Haury 1956). This is exemplified today among Central European biological anthropologists and paleogeneticists who claim that archaeological evidence for cultigens (indicating farming), domesticated livestock (indicating pastoralism), and wild plants and animals (indicating foraging) constitute “archaeological evidence” for patrilocality (e.g., Bentley 2013; Bentley et al. 2012; Goldberg et al. 2017; Rasteiro and Chikhi 2013) or that evidence for conflict is “archaeological evidence” for patrilineal descent groups (e.g., Schroeder et al. 2019). These associations were discredited half a century ago through cross-cultural research (Aberle 1961; Divale 1974; C. Ember 1974; C. Ember et al. 1974). Artifact style distributions, subsistence, and presence of conflict are not archaeological evidence on kinship.

Archaeological approaches to kinship are independent of theories on how human societies change; archaeologists do not need kin term or language evolutionism to make interpretations. The approaches are diverse. Direct historical analogy (Steward 1942) projecting the earliest historically-recorded kinship practices of a descendant cultural grouping deep into the prehistoric past—has always been a temptation. This can be problematic for assuming continuity when all things cultural, including kinship, are known to change. Direct-historical analogy can be appropriate when continuity is demonstrated—to use independent empirical patterns on kinship in the archaeological record to explain the development into the early historic pattern as Ware (2016, 2019) and Whiteley (2016) have done for the Hopi and Zuni. In most cases, there is too much evidence to doubt continuity or the historical kinship practices are known to have been dramatically transformed under colonialism (e.g., many Eastern Pueblos) (Ensor 2013a:65; Ensor et al. 2017:758; Peregrine and Ember 2002). “Ceramic sociology” (e.g., Deetz 1965; Hill 1966; Longacre 1964, 1966, 1968; McPherson 1967; Whallon 1968) assumed that women produced pottery and that each woman learned specific motifs exclusively from her mother, and so the concentrating of motifs within residential areas was interpreted as matrilocality (and therefore matrilineal descent) and a distribution of motifs across residential areas was interpreted as patrilocality (and therefore patrilineal descent). The problems were the assumptions that women universally make pottery, that specific motifs are exclusively learned from individual mothers, that post depositional trash over abandoned houses were representative of the former occupants, and that descent can be predicted from residence (Allen and Richardson 1971). A new possible trend is to use direct historical analogy to project gender roles onto more numerous prehistoric artifact manufacturing activities. As with “ceramic sociology” it is assumed that the spatial distribution of feminine- and masculine-assigned activities within and across settlements reflect matrilocality or patrilocality (e.g., Micon et al. 2019; Sanger et al 2020). A major problem is the assumption of immutable (and binary) gender roles. The further back into prehistory, the less confident we can be in the classification of activities by gender—coeval sex-based burial associations with tools or pathologies resulting from activities are needed to identify gender roles. Another attempt to infer kin groups is to assume that clusters of houses within settlements represent lineages or clans. For example, in reference to the Central European Neolithic, Bogaard and coworkers (2011, 2016) interpret clusters of dwellings and their associated pottery styles and food remains to interpret “clans” having different resources and diets—though these are really only one or few dwellings at a given time (lineages and clans are much larger). Meanwhile, the “house”

perspective in archaeology—inspired by Lévi-Strauss (1982, 1987), Kuper (1982) and Schneider (1984)—claims that corporate groups are not kin groups and that no community practices the kinship described by ethnographers. “Houses” are claimed to have a list of characteristics (these are actually universal to all corporate kin groups, including unilineal descent groups) enabling any interpretation of those characteristics in the archaeological record as evidence for “houses” rather than kin groups. Once dismissing kinship, the school reinvents the concept of corporate groups, identities, and ancestries—claiming we would never understand these without a “house-centric” perspective. To those familiar with kinship, the reinventions are crude because adherents ignore the wealth of concepts in kinship research they dogmatically dismiss (Ensor 2011).

My favored approach—because it is empirical, independent of theory, and avoids the above assumptions—emphasizes inferences on residence and descent using strong cross-cultural correlations between residence and dwelling patterns and between descent and settlement layouts (Ensor 2002, 2003, 2013a, 2013b, 2013c, 2016, 2017a, 2018, 2021a; Souvatzi 2017). These are only useful for communities in sedentary societies. Melvin Ember (1973, see also Peregrine 2001b and Peregrine and Ember 2002), Divale (1977), Brown (1987), and Porčić (2010) consistently demonstrated that large dwellings accommodating multiple nuclear families among communities world-wide (prior to colonial impacts) are strongly correlated with matrilineal residence. There is a myth in archaeology that small dwellings (for one nuclear family each) are therefore correlated with patrilineal residence. But small nuclear family dwellings are associated with avunculocality, bilocality, neolocality, patrilineality, and virilineality (Ensor 2013a, 2021a). Most can be distinguished by their spatial relationships. Patrilineal residential groups tend to have small dwellings encircling, and with entries facing, a small communal space whereas bilocal residential groups have unplanned, haphazardly arranged small dwellings of more variable sizes (Ensor 2021a). Neolocality is reflected in haphazardly and widely-dispersed arrangements of small dwellings—not in clusters for extended residential groups (Ensor 2013a). Chang (1958) first identified very strong global cross-cultural correlations between bilateral descent and unplanned, haphazard village layouts and between unilineal descent groups and planned layouts: a) all residential groups surrounding a central plaza and/or ceremonial structure or b) distinct village segments comprising numerous residential groups. My cross-cultural analysis on North American peoples confirmed those, showing even stronger correlations (Ensor 2002, 2013a). New global cross-cultural tests further confirm these distinctions in spatial patterns between descent groups and bilateral descent (Ensor 2021a). Because interpretation through this approach is independent of theoretical models, it enables testing of functionalist context-based explanations for kinship practices (Ensor 2013a, 2016) and neoevolutionist and phylogenetic models (Ensor 2017b; Souvatzi 2017), as well as tests on continuity across the prehistoric-historic divide to assess the impacts of colonialism (Ensor 2011). As with all generalizable patterns discovered through cross-cultural research there are always exceptions to the rule. For example, this approach to Turner’s photograph of the village of Pykanu (2012: Figure 11.1) would—according to Turner’s description of Kayapó kinship (2012:224-226)—correctly infer matrilineal residential groups but incorrectly interpret either a matrilineal clan with subclan matrilineages or a settlement shared by multiple exogamous matrilineages. However, such exceptions should not overrule strong cross-cultural regularities.

I considered the political economic dynamics of marital alliances (Ensor 2002, 2013a:197-253; 2013c, 2017a). Because Elementary alliances are reciprocal, they require no competition to attract marriages for their members. There is little need for surplus production or ceremonial infrastructure. Crow-

Omaha-like alliances among the inferred unilineal descent groups are competitive—there is no specified marriage pool—resulting in disproportionate numbers of marriages with the more prestigious groups having greater investitures in resources, surplus production, and ceremonial sponsorship—all of which would appear in the archaeological material culture. Under bilateral descent, Complex marital alliances are also competitive. Each corporate residential group (of any kind) competes to attract marriages for its members through property, surplus production, and sponsorship of ceremonies (Ensor 2013:197-253). I had not considered Ware's (2019) suggestion that ceremonial sodalities might emerge to alleviate such competition.

There are now several applications of this approach. My first study (2002) suggested that crises in exogamy resulting from disproportionate growth from competitive Crow-like marital alliances were resolved by clan fissioning in the US Southeast. One study (Ensor 2013a) on 1450 years of change among four Hohokam settlements in the US Southwest demonstrated significant variation in residence and descent practices over time, including exogamous Omaha social organization, but more importantly variation within the same short phases—sometimes within the same settlements among groups having different contexts and histories. Some abandonments and reorganizations could be linked to crises in the Omaha-like political economy. Manipulation of identity with descent groups could be observed, for instance, with co-occurring patrilocality and virilocality. That case study illustrated the founding of Hohokam unilineal descent groups—the cognatic strategies used by founders to build kin groups and the ways that subsequent generations manipulated membership principles to create patri-lineages. The earliest observation of a descent group lasting over 800 years indicated a matrilineage with matrilocality but the strategies changed to avunculocality, then to bilocality, then it became cognatic and ultimately a patriclan with subclan lineages. That last change occurred at the same time a patri-lineage at another settlement shifted its membership criteria to cognatic, while in-migrating groups to the same settlement established bilocal residential groups. I should point out how using the descendent communities' (O'odham) normative kin terminology described above would not predict this variation and changes over time. A Maya case study reviews the numerous problems in ethnological treatments of Maya kinship (Ensor 2013b) and through archaeology demonstrated that residence and descent varied by social class within a local community. This illustrates how kinship strategies were shaped by class-based contexts within the tributary political economy (Ensor 2013b, 2016, 2017b, 2020:193-198). A study on the Caribbean Taíno (Ensor 2013c) identified a late prehistoric shift from matrilocality to avunculocality within matrilineage organization and also suggested that social hierarchy developed through a competitive Crow-like political economy. Another study (Ensor 2017a) explained the development of formally-planned sedentary settlements with substantial size differences, numbers of mounds, and intensive craft production and exchange among foragers in the Archaic Lower Mississippi Valley as the result of a Crow-Omaha competitive political economy. Competition led to disproportionate descent group growth and ultimately crises in exogamy and resources for the largest groups, which was resolved by a reorganization into dispersed bilocal groups and a Complex marriage system. A recent application of this approach to the European Neolithic infers widespread bilateral descent and bilocality, accompanied by a significant amount of matrilocality and neolocality (Ensor 2021a), which contradicts the phylogenetic assumptions of patrilocality. Souvatzi (2017) also applied the approach to Neolithic Greece, linking matrilineal and patrilineal descent groups with tell settlements (remaining at unilineal ancestral locations) and bilateral descent and bilocality with flat settlements that drifted in space over time. The significance of these studies is that they demonstrate variability and

manipulation by social contexts—as predicted in contemporary theory congruent with ethnographic evidence. The inferences do not conform with neoevolutionist and phylogenetic models. They provide evidence that casts further doubts on normative models of homogeneous cultural groupings and provide prehistoric evidence disproving the essentializing assumptions of the New Old Synthesis.

I propose that micro-regional distributions of pottery styles or source-location attributes might be used for interpreting marital alliance systems (Ensor 2013a, 2021a). As per ethnographic observations on significant production for exchange in the ceremonial contexts for marital alliance-making and the continued material exchanges among affinal groups long after marriages (e.g., Fox 1967; Mauss 1967; Rosman and Rubel 1971), I suggest that models for Elementary, Crow-Omaha, and Complex might be made for the distributions of pottery styles and crafts for use on a micro-regional basis (Ensor 2013a, 2021b). Though not tying distributions to specific marital alliance types, Souvatzi's (2017) interpretations on Neolithic Greek pottery spheres as representing affinal relations across settlements is a good example of this idea. Of course, there are other reasons for regional material exchanges. A new collaborative project modeling marital alliances and material exchanges in South America (Queiroz Testa et al. 2021) could illuminate the feasibility of this approach.

Bioarchaeology

For much of its history, biological (physical) anthropology was devoted to a culture historical framework—mapping the changing geographic distributions of biological traits over time to interpret migrations or gene flow among populations. Though long ago rejecting the concept of races, much of the work emphasized clines in traits or, through multivariate statistics, comparing the amounts of gene flow among geographic locations. As with archaeological mapping of the changing geographic distribution of artifact styles, many biological anthropologists reject misusing their data for the Synthesis purposes that Dziebel (2021) advocates (e.g., Armelagos & Van Gerven 2003). But some do adopt the New Old Synthesis perspective when assuming biological populations have immutable kinship practices represented by descendant communities (e.g., Schillaci and Stojanowski 2002, 2003; Stojanowski and Schillaci 2006). Compared to archaeology, biological anthropology is more fragmented in this regard. One is also more likely to find within bioarchaeology the definition of kinship as biological relatedness. At one major bioarchaeology symposium on kinship, 43 percent of presentations maintained the perspective that kinship is the subject of biological relatedness (Ensor 2014b). However, the tide is currently changing to recognize kinship as social relations and cultural participation—the way most indigenous peoples recognize identities. This more recent perspective seeks to illustrate how kinship and kin-based identities distribute biological relatedness (e.g., Ensor et al. 2017; Johnson and Paul 2016).

Bioarchaeology originally developed in the 1980s to address contemporary theory. This field's approaches to kinship are also independent of theories on how human societies change and do not need kin term or language evolutionism. Bioarchaeology combines the observed biocultural phenomena with the archaeological social contexts for interpretation guided by social anthropological models. Bioarchaeologists have used phenotypic (usually dental or cranial) morphological and metric trait distributions within and across cemeteries to interpret residence. Within cemeteries, greater variation (heterogeneity) among males than among females is interpreted as matrilocality/uxorilocality; greater variation among females than among males is interpreted as patrilocality/virilocality; and heterogeneity among both sexes is interpreted as bilocality (e.g., Konigsberg 1988; Lane and Sublett 1972; Schillaci

and Stojanowski 2003; Spence 1974; Stojanowski and Schillaci 2006; Tomzacak and Powell 2003). Alternatively, inter-settlement cemetery analyses consider low variation among males from different sites to indicate matrilocality and low variation among females from different sites to indicate patrilocality (e.g., Aguiar and Neves 1991; Hubbe et al. 2009). Recent proposed revisions to these models for interpretation consider that spouses are not universally buried together—postmortem location and postmarital residence are not the same—and illustrates, using Crow marital alliances, how unilineal descent groups are biologically heterogeneous and that biological relations cross-cut descent groups (Ensor et al. 2017). Despite the sensationalizing of ancient DNA, phenotypic approaches have matured significantly and should continue to inform on kinship.

A major advancement in bioarchaeology is the use of dental strontium isotope ratios ($^{87}\text{Sr}/^{86}\text{Sr}$). Dental strontium indicates the kind of geological landforms on which people resided as children. Although similar landforms may be distant and some site locations may be near different landforms, comparisons of individual ratios with those from where they were buried are increasingly used to interpret postmarital residence (e.g., Alt et al. 2016; Bentley 2013; Bickle and Whittle 2013; Bickle et al. 2011; Knipper et al. 2017; Price et al. 2001). If males have “nonlocal” ratios or greater variation than females, then matrilocality is interpreted. In contrast, if more females have “nonlocal” ratios or greater variation than males, then patrilocality is interpreted. One problem is that in many cases, the majority of the females and males have “local” ratios—the interpretations are based on individuals with unusual histories. Another problem is that more females than males with nonlocal ratios actually suggests cognatic residence with a patrilocality/virilocal bias. Alternatively, more males than females with nonlocal ratios better suggests cognatic residence with a matrilocality/uxorilocal bias (Ensor 2021a). Additionally, some patterns cannot distinguish different kinship practices. For example, avunculocality with matrilineal descent groups would appear the same as patrilocality with bilateral descent or patrilineal descent groups with wives’ membership transfers (Ensor 2021a). Nevertheless, strontium research will continue to be a useful source of information on prehistoric kinship.

Paleogenetics

Paleogenetics also has the potential to provide data on kinship practices that is free of theory on how human societies change—but only if it can break free of an unhealthy association with the Synthesis and if geneticists better understood kinship. The remarkable advancements in ancient DNA (aDNA) extraction, sequencing, and authentication techniques and analytical toolkits are not matched by a sufficient understanding of kinship for appropriate sampling and interpretation of aDNA (e.g., Ensor 2021a, 2021b). In fact, I feel confident in stating that this field has the most impoverished understanding of kinship. The first widespread applications have been oriented toward population biology to test the Synthesis argument that PIE and farming spread from Anatolia into Europe by migrating populations (e.g., Bramanti et al. 2009; Cavalli-Sforza and Minich 1997; Cavalli-Sforza et al. 1994; Dzielinski 2021:15). Mitochondrial (mt) haplogroup distributions from Neolithic cemeteries supported the hypothesis—by assuming some mt haplogroups represent Mesolithic hunter-gatherers and others represent expanding patrilocality, Neolithic, PIE-speaking Southwest Asians (e.g., Alt et al. 2016; Beau et al. 2017; Fernandes et al. 2018; Goldberg et al. 2017; Hervella et al. 2012; Le Roy et al. 2016). However, as the data accumulate—particularly across Europe and as far west as Iberia the evidence for Anatolian waves of migration and for bio-ethnic-linguistic distinctions become increasingly ambiguous (e.g., Cruz Berrocal 2012).

Residence is entertained using the population biology data. Commonly assumed is that Mesolithic mtDNA admixture in Neolithic cemetery populations indicates patrilocality—that indigenous women could only be incorporated into the Neolithic communities if there was patrilocal postmarital residence (e.g., Brown 2014; Knipper et al. 2017; Lacan et al. 2011; Le Roy et al. 2016; Rivollat et al. 2016; Seielstad et al. 1998). This notion is based on Wilkins and Marlowe (2006): a simulation for what should happen if accepting certain assumptions: a) if the expanding agriculturalists are patrilocal, b) if marriages are only with the groups they encounter along the way, and c) if there is no change from patrilocality over thousands of years. Even if using the first of these hypotheticals, which is not a generalizable association, the second and third assumptions are ridiculous (Ensor 2021a). The argument also ignores that women (and men) have been and continue to be adopted or married into communities practicing any set of kinship practices (e.g., Churchill 1999; Scheffler 2001:120-159). Others simply adopt the essentializing argument that because patrilocality is common in Southwest Asia today (ignoring variation by contexts), all the interpreted migrants from there must have been patrilocal 8,000 years ago. Some go so far as to claim that mt haplogroups indicate a racial caste system—assuming haplogroups indicate different racial identities that are maintained and recognized by others for hundreds of years (e.g., Le Roy et al. 2016).

Other problems are apparent (Ensor 2021a, 2021b). Kinship is treated as the subject of biological relatedness (rather than understanding how kinship variably distributes biological relations). There is a widespread assumption that biological nuclear family relationships in coburials indicate patrilocality (when nuclear families are found in all residence practices). Lineages are treated as homogeneous lines of descent (when lineages are large groups and exogamy ensures internal biological heterogeneity). After two decades of this amazing degree of speculation, Western bias, androcentrism, and essentializing in European aDNA research, archaeologists and some bioarchaeologists are responding with scathing critiques (e.g., Brück 2021; Ensor 2021b; Crellin & Harris 2020; Frieman & Hofmann 2019; Furholt 2018; Hakenbeck 2019).

Sampling and resolution are also issues. Most studies select only a small sample from each cemetery for population biology purposes, and often the sample includes few sex-identified adults (with biased sex-ratios). For kinship interpretations, we need larger samples from each cemetery and balanced ratios of sex-identified adults who lived long enough to marry (Ensor 2021a, 2021b). In the absence of haplotype identifications—that can indicate close biological relations through haplotype matching—many treat haplogroups as a substitute for haplotype matches (Ensor 2021a, 2021b). Different haplogroups rule out close biological relations but haplogroup sharing does not indicate close biological relationships.

Given all these problems, kinship scholars should not accept at face value the interpretations in the European aDNA research. Some publications provide reasonable interpretations with the data presented but most do not (as above). As indicated, appropriate samples with a high resolution data—e.g., mt and/or Y-chromosome haplotype matching and autosomal parental to 2nd degree relations presented for adequately sized and balanced samples of sex-identified adults—can be used for more informed analyses. On my part, I am proposing models that can help guide interpretations in this field (Ensor 2021a). However, making paleogenetics more useful will take a large concerted effort by social anthropologists, archaeologists, and bioarchaeologists to shift it toward a productive path.

Is There a Future for Kin Terminology Research?

The debates on Crow-Omaha shine a light on larger theoretical issues and the significance of kin terminology. In kin terminology research today, there is a clear disconnect with social anthropology that needs to be confronted if sustaining the current interest in this dimension of kinship is the most imperative, overarching goal. To ignore that disconnect is to repeat that which caused the last crisis but this time without a sufficient population of future scholars equipped with the knowledge and skills to rescue the topic. The evolutionism echo chamber is not sustainable. It will not persuade future scholars of the importance of kin terminology. At stake is nothing less than the relevance of kin-terminology research. Scholars need to prioritize. Which is more important: perpetuating evolutionism or perpetuating kin terminology research?

The contemporary perspectives like those shared by Coelho de Souza (2012), Dousset (2012), McKnight (2004), Moore (1998, 1991), and Turner (2012) have laid out a more sustainable path forward for kin terminology explanation by 1) not forcing us to accept ideas like neoevolutionism, phylogenetic essentialism, and normative, synchronic and genealogical systems; by 2) embracing the overwhelming evidence that kinship—in all its dimensions—is variably manipulated by agents according to their social contexts; and by 3) adapting contemporary theory more appropriate to seeking and explaining the variation we know to exist within and across cultural groupings. The varying kinds and extent of crossness and skewing found among cultural participants should not be a “problem” when contextualized within specific historically-contingent political economic structures and relational practices. This perspective addresses the kinds of questions social anthropologists seek and is more likely to generate interest in kin terminology among newer cohorts of scholars.

Kin term scholars should also not lose sight of the most important themes in social anthropology today—how expanding global capitalism, global warming, and diasporas impact conditions, lifeways, relatedness, and identities. More ethnographic attention to local variation and change in kin terminology within the contexts of global forces (e.g., McKnight 2004; Moore 1988, 1991) would be most useful to social anthropology and finally address Eggan’s (1937) call for contextualizing kin terminology within changing historical circumstances, though under a more developed theoretical perspective. Overall, researchers could do a better job of situating their synchronic representations of kin terminology within the particular historical contexts, paying particular attention to how resources, resource ownership, and relational identities have altered over time under past and ongoing global forces of change.

I also propose that kin term scholars interested in generalizations need to start adapting their unique knowledge with new techniques to analyze broader patterns in kinship. New alternatives to cultural unit-based databases in European family history research include historically-situated socioeconomic contexts (e.g., Heady 2017; Szołtysek 2008, 2012, 2015; Szołtysek and Orógek 2020; Szołtysek et al. 2014). These could serve as a model for developing databases for the broader range of kinship practices addressed in social anthropology. Wheeler et al.’s (2012) example is a small start. A new collaborative project in South America (Queiroz Testa et al. 2021) looks very promising. I would encourage this alternative path because it better incorporates social anthropological knowledge on how and why change occurs. Moreover, it answers the kinds of questions that most social anthropologists seek, and presumably will seek in the coming decades.

In regards to prehistoric kinship, kin term scholars would do well to reject the New Old Synthesis. Lacking data dating to prehistory is no excuse for essentialism, especially when the ethnographic and

historical evidence overwhelmingly discredit the assumptions of the model on top of the problems of normative cultural characterizations used as data. They would also do well to restrict the speculative neoevolutionism to hypothesizing on late prehistory to test with independent archaeological, bioarchaeological, and paleogenetic analyses. A much better approach—based on the ethnographic and historic evidence for how humans socially interact—is a historical particularist model (e.g., Moore 1994). That approach is best represented in *Crow-Omaha* by McConvell (2012). There are still the problems of colonial impacts and normative term semantics for cultural groupings to consider. But this is probably the best approach to prehistory using normative kin term data. Scholars need to abandon the belief that they can “reconstruct” kinship thousands of years into the past. At best, this approach could credibly provide hypotheses for no more than a few centuries back into late prehistory (assuming the interpreted changes are not the result of historic colonial impacts). For example, a study on the Caribbean Taíno (Ensor 2013c) identified a late prehistoric shift from matrilocality to avunculocality within matrilineage organization. This confirmed Keegan’s and Maclachlan’s (1989) interpretation of avunculocality using kin terminology dating to only a few hundred years after that late prehistoric shift.

Though archaeology, bioarchaeology, and paleogenomics have data dating to prehistory and are rapidly becoming the main source of prehistoric kinship research, those lack the resolution of social relatedness and kin term manipulation demonstrated ethnographically (e.g., Coelho de Souza 2012; Dousset 2012; Turner 2012). As such, assistance is desperately needed with generating basic, low-resolution models that subsume a wide range of variants like those of *Crow-Omaha*. Less specific models would be more appropriate for these fields’ interpretive capacity. This need presents new opportunities for kinship scholars interested in prehistory. At the same time, the more appropriate historical particularist approach to regional kin term semantics (exemplified by McConvell 2012) could provide opportunities to generate hypotheses to test with the late prehistoric data from archaeology, bioarchaeology, and paleogenetics.

Finally, rather than beginning with kin terminology and assuming nomothetic structural-functionalist associations to secondarily interpret social organization and marriage, those interested in prehistory might consider the reverse. For example, where I have inferred matrilineal and patrilineal descent groups and, respectively, competitive *Crow-* or *Omaha-*like marriage dynamics through archaeological data (Ensor 2002, 2013a, 2013c, 2017a) what might we safely assume from that low resolution about kin terminology? Of course, many of the structural-functionalist assumptions are not well accepted (e.g., Goodenough 1970). That reluctance creates opportunities for new cross-cultural testing of the previously assumed but not well substantiated leaps between terminology, social organization, and marriage. For example, although I completely reject the New Old Synthesis perspective by Korotayev and coworkers (2019), they provide the most extensive cross-cultural tests to date on the associations between *Crow* terminology, descent, and residence, and between *Omaha* terminology, descent, residence, and inheritance. Whereas authors in *Crow-Omaha* questioned how well we may assume these associations, Korotayev et al. (2019) demonstrate which assumptions we may embrace with greater confidence. Far more testing of this sort would benefit not only kin terminology research but also a reverse approach from prehistoric inferences on social organization and marriage to kin terminology.

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