

# THE PLACE OF KINSHIP IN THE SOCIAL SYSTEM A FORMAL-AND-FUNCTIONAL CONSIDERATION WITH AN APPENDIX ON DESCENT AND ALLIANCE

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*This paper examines the recent controversy as to whether there is any universally defined domain of kinship in sociocultural systems from the point of view of the philosophy of science, in particular, the classical positivism (e.g., of Radcliffe-Brown and of Murdock) that I show to have motivated the question. It also examines the American version of the controversy, as with Schneider, and shows that, again, the question arises because of essentially the radical empiricism of cultural particularism and its methodological focus. It then proceeds to evaluate the question from a cognitive-cum-formalist perspective, and goes on to argue that Lounsbury's approach is not only also positivist-behaviorist in its foundations but also unwilling or unable to consider kinship as a domain having regard to its function within the whole social system and therewith in fact inadequately formalist, having regard to genealogical organization. I proceed to take especial note of the fact that, uniquely, kinship is a system of social relations that is what I call pure-relational, that being the functional basis of its universal definition. Finally, as an appendix, I generalize the idea of alliance to the structural organization of all kinship systems.*

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I begin by tracing out the 'genealogy' of the arguments that led to recently dismissing considerations of social function from kinship analysis. I begin with the British social anthropologist, Radcliffe-Brown, since Needham's (1971) argument that there is no such domain as 'kinship' is grounded in the Radcliffe-Brownian tradition of anthropological research.

Radcliffe-Brown subscribed, as did much of classical British social anthropology, to the British version of Positivism (see Yourgrau 2005) and followed Frege in reducing meaning (including that of social category words) to referential pointers of observables. Thus, as Murdock said, Radcliffe-Brown persisted in claiming that the very meaning of kin terms was about the place (in the structural-functional sense) of persons categorized by kin terms in descent-groups (clans, lineages, moieties, and so on) or other directly observable (and therefore empirically 'real') collectivities or groups. The same line of argument led Fortes (1959, 1961) to reduce 'descent' to the structural principle of unilaterally organized groups, which meant, for Fortes, that there is no sense in speaking of descent in, say, cognatic societies without clans and lineages. Much like Murdock's argu-

ment against Radcliffe-Brown, Fortes's view of descent is inadequate because it eliminates any account of kinship terminologies for all societies without unilineal groups, as all terminologies incorporate a concept of descent whether or not descent groups are recognized.

Despite his disagreement with Radcliffe-Brown, Murdock was also a Positivist, but in the tradition of American empiricism that served as the foundation for psychologically based, Learning-theoretical Behaviorism. His book, *Social Anthropology* (1949), is grounded explicitly in the Yale school of Learning-theoretical psychology (see his introduction, page xvi). As a result, Murdock ended up making a claim as reductionist as Radcliffe-Brown's, namely, that—and especially in the case of classificatory/merging kinship terminology systems—explanation of the meaning of terms is through the claim that persons are terminologically classed together because they are generally to be found living in 'the same *place*,' hence seen-and-treated behaviorally as if they are the same, thus, in effect, occupying the same position in a behavioristically defined way. The problem here is, of course, an essential circularity.

In Lévi-Strauss's distinction between Harmonic and Disharmonic systems, and in particular in Dumont's (1957) attempt to apply it to the South Indian Tamil kin systems, those Tamil systems that seem to have, say, matrilineal descent groups are considered disharmonic because the royal families of the 'little kingdoms' traditionally were not co-residential; their male members resided for political and control purposes throughout the territory. What is at issue here, though, is the very notion of 'place' that underlies this and Murdock's 'residentialist' account. Cognitively (see Lehman and Bennardo 2003), a 'place' is not defined objectively as a geometrical object; it is, rather, socially/culturally defined. How close must persons reside before they then can be said to be co-resident, hence categorized as being alike? Must they be in the same house, house-compound, neighborhood, or community? The men of those Tamil royal lineages are resident in the same 'place' as soon as we recognize that *place* is the royal domain over which they are distributed!

Examples like this make evident the way that classical attempts to provide accounts of kin-category (term) systems in positivist-behaviorist terms led to the rejection of these attempts and, by extension, to the rejection of any attempt to explain such systems through their social function. This was not the only line of thought, though, leading to disenchantment. Again classically, there has been, at the very least since Morgan (1871; see Leaf, this issue) and/or Rivers (1910), another tradition of defining kinship as a conceptual domain through the way genealogical relations are mapped to kin-term relations. Positivist assumptions were also applied and thereby created similar problems. In British Social Anthropology (in more or less the Radcliffe-Brownian form), there has long been an argument (see, for example, Leach 1961; Ruel 1963) as to whether kinship terms are genealogical words or social category terms. The argument came down on the side of social category words by virtue of the fact that kin terms are often used for non-genealogically connected persons. Rules stating how persons related under these category relations ought to behave to one another then define a social domain in culturally provincial ways. Needham (1971) took this position and argued that the entire domain

we call kinship has no universal definition or features. Instead, it is, simply an epiphenomenon of anthropological work centering on genealogical methods of investigation. Similarly, Leach (see final chapter in Leach 1961) asserts that “kinship” is not about relations amongst people connected genealogically but rather about relations defined (in Sri Lanka) using property/land ownership rights. Similar skepticism about the very existence of a universally defined domain of kinship is echoed in the United States by David Schneider (1972, 1984—see also Carsten 2004 for a very clear account of the way a given people generally talk about kin-relatedness in other than genealogical terms<sup>1</sup>). Note that what is really at issue here is how people actually understand or conceptualize the relatedness, not the relatedness itself! We have long known that, for instance, the substance of reproductive relatedness can be conceptualized quite differently from the way science or Euro-American society and culture understands it. This does not require us to question a genealogical basis for kinship. Indeed, as I have shown elsewhere (see Lehman 1993, 2000), and as Read shows in much of his work (see especially Read 2001, 2007), genealogy is not about the substance of bio-reproduction but is rather *a formal computational matter* whose cultural construction may vary considerably.

Schneider is concerned with the fact that kin categories often contain referents not defined by genealogical relatedness. He bases much of his argument upon the ethnographic work of his one-time pupil, Vern Carroll, in the Polynesian outlier of Nukuoro, where Carroll claims that the people simply do not, in general talk, about categories of relatedness by referring to genealogical connections. My former student, M. Goldsmith (1986-7), did work on Nukuoro and knows the language fairly well. He has been able to show that they are, in fact, quite capable of talking about such categories genealogically. Schneider argues that in such cases anthropologists impose upon their subjects of study a sort of genealogy-talk game (that of, say, Rivers’ genealogical method). But, if we accept that it is, indeed, a game-like method for exploring relatedness categories, how then is it that our subjects immediately know how to play it? Even more, what happens to Schneider’s argument if we recognize that the “game” is not just something analysts impose but rather something everyone actually plays, whether an analyst is present or not, or distorts it or not?

Surely the question turns upon whether human beings, in general, have the cognitive capacity to recursively compute connectedness (see Lehman 2000). This, moreover, is made acutely relevant in view of the utterly uncontroversial fact that all human communities recognize that, for any person there is necessarily a mother paired (whether understood in biological terms of not!) with a man such that he legitimizes her giving birth and is, at least inter alia, called whatever their word is for our word father. This connection is, universally, recursive because each such ‘mother’ or ‘father’ is a person in turn. In addition, consider further the fact that any kin category term may, indeed, have referents without genealogical connection to the speaker. Think of ordinary English ‘cousin’ uncles and aunts, and questions one might ask about them. If I ask someone (an informant/consultant, say, when doing ethnographic work) “do you call so-and-so ‘uncle’?” or “whom do you call ‘uncle’?” I may well get a considerable list of non-genealogically connected persons in response; if I ask, say, “Is so-and-so *your uncle*?” I

am far more likely to get only genealogical responses. This is the sort of methodological point that much of Goldsmith's Nukuoro work turns upon. And, of course, this in turn relates to the distinction between terms of reference and terms of address. Furthermore, if I ask, again using English, or even in my own Burmese language, anything like "why is he called 'uncle'?", I may get in reply all sorts of non-genealogical reasons to the effect that the person is an elder male deserving of the kind of respect I am bound to give to an uncle—who in the first place is a genealogically defined "real" uncle. The claims of Schneider *et al.* that kinship does not exist have, then, to be evaluated in the light of proper ethnographic and linguistic methods.

Above all, (see again Lehman 2000) the computational connection between genealogical categories and kin categories is not, and has never been, that the latter must contain only the former. The requirement is, and has to be just that the set of kin categories, as a properly defined class, is closed under any map from the space of genealogical categories (a space defined algebraically by the aforementioned computational machinery, which must have a more complex logic than simply a set of rules that would link a set of terms to a set of objects) to the space of kin categories. That is to say, only the latter categories are the proper targets for that map, such that (a) every kin category contains at least one genealogical category,<sup>2</sup> and (b) every relevant genealogical category maps into one, commonly only one, kin-category. And, from all the immediately foregoing, it must be quite clear and certain that the currently popular extreme relativist view to the effect that genealogy is not universal for defining the institution of supposed kinship is simply false. Of course, to say kinship as a domain is defined computationally by such a closure map does not entail that all cultures think, talk about or define kinship in that manner.

Let me now turn to Lounsbury's (1964, 1965, 1979) work. He also must share part of the blame for taking any genealogically based account of kinship as a domain out of the framework of social-cultural functions. Let me first point out that Lounsbury, mainly an important linguist of the American Structuralist school of Bloch and Sapir at Yale, was, as an anthropologist, also a pupil and then a colleague of Murdock. He also had a not inconsiderable background in logic-and-mathematics. Therefore, in spite of helping found the development of cognitive work in anthropology, it is unsurprising that he was also, to some extent, a positivist-behaviorist in his philosophy of science and method. So, on the one hand, Lounsbury was concerned with words (here, kinship terms) and their semantic connections, but still quite ready to throw out any appeal to social function in a theory of kinship as an institution. More significantly for this volume, his interest was in specifying systematically what it is that people must know in order to be able to say exactly which kin term<sup>3</sup> should be used for which genealogical kintype, for all the—in principle—infinity of the latter. Being a linguist of the sort I have already mentioned, he seems to have been concerned with knowledge/meaning in a Fregean-referentialist way. So he made no attempt to specify any structure for the genealogical category space despite his including, more or less as an afterthought, a sort of definition using the vocabulary that was common to anthropologists like Murdock—based on Kroeber and others of an earlier era—who were concerned with the classification of types

of kin terminologies (see Read, this issue). We find Lounsbury using words such as, lineality, collaterality, generation, sex and so on; that is, words used as part of the, colloquial way of talking about genealogy when classifying terminologies as, e.g., bifurcate merging, bifurcate collateral and so forth. Indeed, much of this part of Lounsbury's work is motivated precisely by the classical kin-terminology classifications: Crow-Omaha, and so on, which is to say that it is typological in form.

It is important to understand how the logical mechanism of Lounsbury's kinship work is antithetical to any intentional organization of the kinship domain. The reason is that his rewrite rules do not generalize over any algebraically defined structure or space at all. Rather they take each point in genealogical space and map it into its particular target category in a kinship terminology. To say, for instance, that by rewriting FB as F and then reducing any larger kin-type string that contains the substring FB by the  $FB \rightarrow F$  rule is to miss entirely the wanted generalization that, say, for agnates one is, in fact, once and for all, merging non-lineals with lineals. This is an operation on a structural space, not an operation that takes word referents into terminological categories for other word referents. The latter is, essentially, what Lounsbury is doing and it is what allows him to take an *extensionist* perspective, namely that merging extends the *words* for all sorts of agnatic lineals to their co-generational agnatic non-lineals. This latter, as I have tried to show elsewhere (Lehman 1993, 2000 and references therein to my earlier work and to work by John Atkins [e.g., 1974] and so on) is exactly the sort of structural account that implicitly underlies Lounsbury's rewrite rules but is not identified as such by him. No structure is ascribed to genealogical space and, likewise, none is assigned to the organization of any culturally particular kin term system. And, as Dwight Read, in all his kinship work (see now Read 2001, 2011) has shown with great force and clarity, nothing less than a complete structural account can provide a genuinely theoretical account of any kinship category system, in the sense of its *cognitive*-conceptual-computational nature.

But what is the importance of taking into account a possibly cultural-functional basis for the very domain of kinship? The importance lies in the fact that the whole complicated Positivist line of research has led, on the one hand, in British Social Anthropology and in the work of Schneider, among others, to the denial of the existence of kinship as a theoretically definable domain and, on the other hand, in all the purely formal work up to, and including, that of Lounsbury, to the disjunction of kinship theory from social-cultural-cognitive theory. This disjunction implies that Lounsbury's cognitive theoretical intentions are essentially undermined by his behaviorist methodological stance. This paradoxical conundrum—either deny kinship exists or reduce it away from all cultural theory, surely can be overcome. How?

Simply, in fact. The domain of kinship is unique amongst all social systems in that its very definition is purely structural/formal/computational. I have recently (Chit Hlaing 2011) written in detail about this and so I shall quote *in extenso* from that paper here, in explanation.

I must [first] raise the question of why anthropologists have spent so much effort on the domain of kinship and why so much serious mathematical work has been done on this particular cognitive-conceptual system. I propose that the answer is

that in kinship we have a domain of knowledge, or cognition, where we are best able to study its computational properties most readily. Let me explain.

This domain is perhaps uniquely amongst domains of social relations the most *purely relational*. Consider any other social system. To be, say, a student or a teacher one has to know a great deal of the content; one must know how to do all sorts of things. Indeed, there all sorts of things that are required for any individual to actually be/or become either a student or a teacher. To be a student one must know how to study, and all that and one must be admitted by others to this social identity (SI—using the terminology of the late Roger Keesing [1970]). It is necessary that one be seen or understood to be behaving as such and to see authoritative others as having done something constituting one as *admitted* for one to be seen as a student. Now this is true, I claim, in all social domains other than kinship; one has to have learned *how* to be that SI in order to occupy or instantiate it, and it is not sufficient for understanding any such SI to be able just to specify how the different SIs of a domain are related to on another. Teachers are indeed hierarchically above students in terms of *status*, but that is insufficient. Similarly for, say, kings and their subjects, their courtiers and so on. But to *be* a king it is not enough to be just son of a previous king; one must be enthroned, installed and so on. That is, the position can be withheld by others' actions or inactions. What I am pointing out here is that the *role* content of an SI makes all the difference having regard to what I may call individual occupancy of it. And this perhaps explains why Keesing insisted on distinguishing between Social Identity, Role (its enactment, often interactive) and status (what social scientists have usually referred to as the respective rights and duties of SIs to each other in any such system. And so, to know how to *define* an SI in a non-kinship domain requires one to say a lot about role and status. But now consider kinship.

[...] it is a system for which we can most readily study/explore the computational properties because kinship is what one may call *pure-relational*. That is to say that what it *means* (taking this term in its cognitive sense) to be an occupant of a kinship category (call it  $K_{SI}$ ) can at root be defined in purely relational terms. For instance, *child* means immediate lineal descendant (issue, if you will) of a person properly occupying, to oneself, the category of *parent*. So, all one has to be seen as in order to be a *child* is to exist/to have been born! All anyone else has to do is give birth, which, notice, in English at very least (and, for instance, in Burmese and other Southeast Asian languages) is not an action verb, but rather a stative verb. By existence, at least in the default, one is the child of one's mother and a woman is the mother of that child. And, in as much as (see above) it is universally understood that no woman gives birth without in some way or other being paired with a man, that man (whoever he may be) simply *is* Father to that child. That he may be, using English common law terms, a person unknown is entirely inconsequential. One has, in virtue of nothing other than one's existence, a F and a M. Furthermore, since each of those is deemed to exist, each also has a

F and M (F and M falling into the gender-free category of Parent), and so on recursively....

Let us assume that parents may have been identified. If at least the M has another child, it is one's sibling (B or Z/sister), again by virtue of existence alone. The same logic gives siblings, if any of course, to all lineal ascendants, and by inversion, gives us relations of lineal descent from siblings and from parental siblings (say uncles and aunts in English anyhow, i.e. MB, FB, MZ, FZ; PSib), which gives the genealogical specification for English nieces and nephews and cousins. For, the former are just C of Sibling and C of Sib of P, respectively. And by recursion, C of cousin gives us, in English, cousin, we find that *cousin* is specifiable as any lineal descendent of a parental sibling (for the formal feature specification of such ablineals in Goodenough's sense (1965, see Lehman and Witz 1974, 1979), whilst for the ablineals from one's own siblings, by a similar logic, "the parent of a niece/nephew is a niece/nephew unless that parent is a sibling (to self)". So there is no way in which one can avoid saying that genealogy 'feeds' the meanings of categories in KTS. And [...] whilst indeed there have to be all sorts of encyclopedic meanings for every such category constituting what we know *about* them, there is one remarkable fact about kinship that distinguishes it in its pure-relational sense.

That is, a person may be the worst instance of any such category and yet this has no direct bearing upon the relationship in question. True, *custody* of a child may be legally removed from a bad parent, and still they are, both legally and colloquially (using English examples), "parent and child." If, say, we consider the orthodox Jewish custom of 'sitting shiva', where parents of a child deemed religiously "beyond the pale" declare the child dead and arrange the house in the fashion of mourning, we find that though the parents can say "I have no such child," the child, who obviously is not dead, still claims them as his/her parents. Similarly for the less well-understood instances of so-called "disowning" of a wayward child. Note in such cases that whatever relationship is said to be severed, it is not the genealogical connection but rather the jural one involving the KTS, basically, to which is attached all the "rights and duties." Some of these of course are actionable at laws, but none of them define the relation as non-existent. In other words, adopting some categories from the British social anthropologist M. G. Smith (1974), we may think of the space of genealogical relations as the *commission* underlying the *office* of the jural kin relation. Thus we may say that where a commission is not withholdable, its office's jural rights and duties are not definitive of the relationship inherent in that the office cannot *define occupancy*. And the one social-cultural system that fits this rule is, of course, kinship.

The consequence of this line of reasoning is that uniquely in kinship we can study from a cognitive [computational] point of view what a system is all about. That is, the knowledge that people seem to have that makes it possible for them to understand their domain of kinship in the sense of making it definable for them need not work with all the baggage of what I called behavioral "content,"

which here would be everything about how to be a Son, Daughter, Mother, Father” and so on. What remains, then, means we can do a good job as cognitive scientists by concentrating upon the formal, relational properties of the system! Indeed, as I shall say below, much of the history of anthropological work on kinship for the past forty or fifty years has been along these lines. To “know” what it takes to “be a ...” within kinship is not how to enact its role, but rather something more abstractly formal. E.g., how to know such things as that if so-and-so is called Cousin (in English), then necessarily, either one of his/her Parents is to be called Uncle/Aunt, or Cousin, such that, eventually one reaches a Cousin whose parent is an Uncle/Aunt. Notice that this is a computational form of knowledge, and this is precisely the focus of all the work of Dwight Read and his colleagues. To say that people know how to compute such quasi-closed systems of relations is to say that they know its structure. Still, a formal/algebraic description, as in Read’s work, of that structure can be taken only as a *model* of the cognition; nobody is claiming that ordinary members of society have, as their knowledge of the kinship system this notational algebra in their heads—certainly not consciously. The same caveat applies *pari passu* to all the work I shall mention below (including my own) on the obviously cognitive question of what it is in our heads that lets us know which of the infinitely many categories in PGS (Primary Genealogical Space) is to be called what in the KTS, in the map under which the latter is closed; where PGS means *Primary Genealogical Space* of relations, and KTS is the jurally-related, culturally specific Kin Term Space.

Note most particularly that the foregoing view runs headlong in opposition to the Murdockian Learning-Theoretical point of view, and therefore also against extensionism—the idea that, for instance, a woman called “mother” is to be taken as a sort of extended member of the same category and that this is in some sense because one learns to relate to her as a kinswoman by extending one’s primary relatedness knowledge of one’s own actual birth mother. From a Learning-Theoretical point of view it is supposed that one learns how to relate to others based upon having first learned how to relate to nuclear family members. Lounsbury, as Murdock’s one-time pupil at Yale, is similarly an extensionist with his rewrite rules that map, point-to-point (see Lehman 1993, 2000), all the genealogical-categories to kinship categories. From this we supposedly get kernel *vs* extended ‘meanings’ of kin terms. Implicitly, at least, the rewrite-rule account of a given kin-term system rejects any appeal to social motivations, whereas the algebraic accounts in the way Read and I and others have tried to construct them do not, because, for me at least, what any kinship system is intended to do is *define* newborn persons in an initial matrix of relationship with others in society well before anyone needs to “learn” how to interact with the infant as an individual and so well before an infant need “learn” how to interact with anyone! From the standpoint of at least the map from genealogical space to the space of kin-categories and terms—and to use a simple instance having to do with classificatory/merging terminologies—a genealogical FB is to be called by the word for F precisely because merging means collapsing the whole ascendant agnatic part of genealogical space by collapsing the distinction between lineality and non-lineality; say, in the

form of the Category Theoretic map from genealogical space (Lehman and Witz 1971, 1979 and Lehman 2000). This collapsing emerges from having sibling as a generating term (Read 2007). Formally, then, we get:

$$[-\text{Lineal}] \rightarrow [+ \text{Lineal}] / \left[ \begin{array}{c} +\text{Ascendant} \\ +\text{Agnatic} \\ +\text{Male} \\ \hline \end{array} \right]$$

Here the feature matrix following the slash specifies the context within which the feature-change given by the arrow applies, and the horizontal line is the place in that context of the dimensional feature specification being changed by that rule. That is, non-lineals get redefined as lineals just in case they are male, agnatic ascendants.

Finally, please note, there is yet another fundamental social fact involved, namely, that the indefinitely large genealogical space, which has far too many elements in it for a person to be able to keep track of them all and differentiate amongst them with regard to behavioural expectations, is now quite small and, therewith, socially manageable. And it is this latter collapsed space of categories, so to speak, that has to be ‘fitted into’ the algebraic structure of the culturally particular kin-category space of the kind given algebraically in the work of Read. That the latter is not literally generated by the map can be taken for granted because the culturally particular algebraic structure is quite different from that of the genealogical space. Yet it is hardly accidental that it has inherited the basic geometry, so to speak, of the genealogical space: dimensionalities such as up/ascending, down/descending, and the essential recursiveness of the genealogical core relation of generation—basically the parent-child inverse relation through construction kin terms via algebraic products that encompass the recursiveness of the genealogical core.

## Appendix

### The Kinship System (KTS) in Terms of Lineation

My purpose here is, first, to outline the intellectual history of the distinction between “alliance theory” and “descent theory” in the theory of kinship systems in anthropology, and second, to try and show at least some of its consequences in/for the rise and development, over roughly the last half-century, of serious formal-mathematical theory about the domain of kinship. Clearly, this is closely related to, and indeed follows from, the lines of some of my previous work (e.g., Lehman 1993; Chit Hlaing 1999, 2011).

Let me, then, proceed to the rise of what became Alliance Theory, which, because its main roots are in the kinship ethnography of Mainland Southeast Asia, has been of especial importance in my own work, past and current, among the Chin and Kachin (Jinghpaw) peoples of that part of the world. The precise historical sequence here is somewhat

obscure. For, although Leach, who explicitly defined alliance theory (in his 1951 paper—see especially p. vi of his 1961 book, where he shows why the intellectual history is confused), is the most important original figure here, with his two early Kachin-based papers (1945, 1951). Perhaps, at least for Mainland Southeast Asia, the actual first paper in this history is Barbara Ruhemann's 1948 paper (cited by Leach 1951), which, in effect, is the first to provide *comparative ethnographic evidence* needed for a typological distinction. That, in fact, was my own reading, which led me directly from Ruhemann to the importance of Leach's two papers at the end of the 1940s when I was still in graduate school.

Of course, the next step was due to Professor Lévi-Strauss who took the matter up in his 1949 *opus*, but without citing Leach 1945 (published only in 1950!), but taking a rather different (exchange-theoretical) structuralist direction, thereby establishing a somewhat different typological distinction between so-called Restricted and Generalized kin-alliance systems that allowed him to postulate (going beyond Leach here, with a much wider comparative perspective) a universal distinction between alliance systems of kinship and non-alliance-based systems. This allowed him to widen the scope of Leach and Ruhemann to include East Asia (China) through his reading of the French Sinological work of Marcel Granet (1939—see Heran 2011) and of the related (indeed intertwined) anthropological work of Francis L.K. Hsu (1940, 1948) supposedly on Chinese kinship, but in fact on *Minchia* (for the Bai see Zhao 2007).

At this juncture, come several others; certainly Fortes (1959), and Schneider (1965). Fortes mainly criticizes Leach's distinction, because, he asserted, Leach's notion of an alliance relationship between local kin-corporations (lineages, clans) masked a descent relationship between their respective members (namely, between men of one agnatic group and their mothers' brothers and the latter's agnatic kin, which he called complementary filiation). Fortes claimed to reduce what Leach was dealing with to a relationship defined in Descent Theory as per his 1953 paper. I have no intention here to recapitulate this controversy in any detail. Fortes is essentially reducing affinity and relations-by-marriage among kin groups to what he calls *complementary filiation*, which is a sort of vague, inchoate supplementary set of descent relations. If we define descent (and hence descent groups) in terms of filiation (the jural parent-child connection), the man in question has, through his mother (even in an agnatic system of jural filiation such as one finds in Leach's Kachin data and the other Southeast-and East Asian data), a filiative relationship with her brother's agnatic group (his mother's patrilineal kin). What this comes down to is the idea that the man's special relationship with his mother's brother that characterizes the Jinghpaw, Chin, Bai and other South-east Asian can be defined as a sort of descent relationship, which supposedly eliminates any need to invoke a non-descent principle of marriage *alliance*. It is, I think, obvious that there is a basic contradiction (as, in fact, Leach notes) within Fortes's descent theory because to consider descent as filiatively defining corporate group *membership* would presumably leave relations between such groups as affinal relations that he now reduces to a sort of filiatively defined supplementary membership. It is not clear, at least to me, why Fortes wanted to go to the trouble of this convoluted argument.

What are we then left with at this juncture? We are left with a controversy between the claim (alliance) that a jural-ethnographic *preference* for marriage with a MBD (classificatory) is a *prescription* to marry a woman from the patrikin of one's mother, and the claim that it is something else, namely, a vague agnatic relationship through a uterine linkage (a mother) to another agnatic group; and that this entails marriage between the two for its systematic definition and perpetuation. Note in particular that this gets in the way of the principle that regularly links descent group membership and exogamy (see, for instance, Murdock 1949). On the complexity of the distinction between preferential and prescriptive marriage rule (Leach advocating prescriptivity of alliance marriage and Lévi-Strauss advocating a preferential marriage relation [to a MBD] in these systems), see especially the postscriptum (p. *xix*) of Needham's editor's introduction to the English version of Lévi-Strauss (1949), and Lévi-Strauss's 'rejoinder' (his Preface to the French 2<sup>nd</sup> edition), also published as a Huxley Memorial Essay in 1966.

In the final analysis, however, I ask that one look at my attempts to overcome these contradictions and confusions (1999 in particular). I try to show that one must start formally, from a proper definition of prescription as referring to what is left over from what is *proscribed*. Thus, in our present context, if one is claiming that a classificatory MBD is proscribed to be married, one is in fact claiming that exogamically one must marry someone *not* in one's own agnatic kin group. Then if one's mother's agnatic group is within that proscription and if the rules do not proscribe any other marriage (e.g., into a group that is neither one's own, or one's classificatory MB's agnatic group), it follows that, indeed, as Lévi-Strauss argued, the "asymmetrical alliance" marriage is preferential! This in fact turns out to be what distinguishes the asymmetrical/generalized exchange systems of marriage from the symmetrical/restricted kind, namely, that in the latter system all marriages other than those with a woman from the absolute complement of one's own jural consanguines (i.e., all other kin groups in the society) are proscribed, such that one is effectively in a premarital, classificatory, bilateral cross-cousin relationship with all other kin groups that one's own group has married with before and not with any other kin group. This turns out to be a very complicated matter that I cannot recapitulate here, but it is also one that preserves alliance theory against descent theory because, it turns out, it makes the former a general theory of marriage relations even in the cases that Lévi-Strauss denominates as Complex Structures, as against his Elementary ones. For, even in a cognatic or other such system where all one has is rules *proscribing* certain marriages, such as those who are in 'prohibited degrees' of genealogical relationship; e.g., marriage with cousins of a certain degree of closeness (often confused in our literature with a sort of extended prohibition of incest)—what we have, obviously, is *proscription* and so alliance theory applies, as it were, across the board. That is, one is invariably in all systems of kinship obliged/*prescribed* to marry into just the set of non-*proscribed* sets of persons defined by some sort of possible genealogical reckoning!

And now, what of Fortes's *complementary filiation*, the lynchpin of his descent-theoretic argument against Leach and alliance theory? Well, it turns out that if one examines a mathematical map from genealogical space to the space of the kinship categories (terms) of any standard Leach-type marriage system (Chin, Kachin, or whatever), one

finds that it defines the set of maternal agnates (precisely the set of groups containing real and/or classificatory matrilineal cross-cousins); i.e., the set from within which marriage is preferred,<sup>4</sup> quite, as it were, open-endedly. So, algebraically, the set of preferred marriages is indeed vaguely defined and, in fact, is so defined with regard to what has otherwise been called *lineation*. Lineation is in fact the algebraic understructure of what Fortes calls descent. That is, it considers *all and any* lines of successive filiative relations, where these can be notated as to any arbitrary succession of male and/or female parent-child connections, uterine or paternal: so that persons, *x* and *y*, if connected genealogically at all, are related by some mode of lineation—where any immediate linkage defines a mode of filiation. So, e.g., in a kintype string *ppppp ...*, every *p* (= parent) is invariably either *m* or *f*, and in any string involving *c* (= child), such as *...cccc...* any non-terminal *c* is necessarily either a male (*m*) or a female (*f*) and so a filiative relation is defined that is either maternal or paternal. Hence, any such notational string defines a mode of lineation, so that agnatic ~ patrilineality is simply a lineation/string where all such connections are paternal and, of course, conversely for uterine ~ matrilineality. And then, needless to say, cognatic systems are simply defined in terms of the set of all modes of lineation where the filiation mode of all the individual linkages in a given notational genealogical kintype string may differ one from the other. And this turns out to defuse yet another of the pillars of Fortes's argument, namely the idea that the very word or notion of "descent" can apply only to unilineal systems because, supposedly, given any cognatic lineation, no kind of group membership can be, as it were, defined by birth. That is, it cannot define a corporate grouping such that for any person, his/her birth defines him/her as *uniquely* a member of something simply on virtue of one and only one of his/her parents—the male one (*f*) or the female one (*m*). That is, we now have to say that it is lineation that has to be defined genealogically and a set of relatives as a grouping into which one is born. In a cognatic system, one is born into the set of all relatives connected to oneself by unrestricted successions of mode-of-filiation, and hence, *contra* Fortes, one may indeed, by the very logic of his own argument, speak of cognatic ~ non-unilineal *descent!*

One final observation. When we accept the alliance theoretical position, we find yet another consequence with empirical significance. In the case of asymmetrical alliance systems, we find that the terminology for agnates/consanguines is extended to persons in kin groups that are not one's own consanguines also has no inherited marriage allies. This indicates that, in such systems, the focal/central category is that of marriage allies, so that the category of consanguines is, as it were, the residual one: kin groups are hierarchically understood such that allies come first, and everyone else comes second. Contrast this with the situation in symmetric/restricted alliance marriage systems. Here the hierarchy of kin groupings is the other way round; it comes down to "us," and then everyone else, which is to say that the universe of kin groupings is divided into two sets: the central one of consanguines plus other groups into which one *cannot* marry, and ones with which one does marry. Now consider complex systems, where the central organizational consideration is simply exogamy. Such systems, like restricted/symmetrical ones, use exogamy as the central principle for ordering marriage relations, so that, again, we

get “us” against all others (the latter being, in principle, marriageable). It does not matter whether such a society has descent groups or only families to which these organizing principles apply. Thus, for instance, in Euro-American kinship systems, generally speaking, we have first the incest prohibitions and then the prohibited degrees of consanguinity defining non-marriageability, with everyone else defined as marriageable. So, there turns out to be a perhaps unexpected organizational similarity between symmetric-restricted alliance systems and complex systems/non-alliance systems of marriage having to do, essentially with the utter centrality of the principle of exogamy, whereas, in asymmetrical alliance systems, as I have shown for both Chin and Kachin, if there is a chain of alliances — defining marriage allies of marriage allies—taken together with local rules governing the generational span within which a marriage with an allied group must be renewed, or there is a generational span within which lineage fission can occur, such that one is obliged to marry a woman from the “end” of that chain, or one wishes to marry someone and she is within one’s consanguine group at a genealogical distance beyond that span, then one will indeed violate exogamy (see Chit Hlaing 2007 for Kachin, and more generally, Lehman 1970 or, for Chin more specifically, Lehman 1963). So, Asymmetrical alliance ~ generalized exchange stands truly apart in that here the exogamic principle takes second place to the principle of alliance itself.

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<sup>1</sup> El Guindi (this issue) deals nicely with a similar way a people talk about relatedness in terms of what amounts to child-nurture, but is able to show that in actuality they make it clear that the default form of nurture is understood to involve what we would call the birth-family and the birth mother, in particular. Recently, Marshall Sahlins (2011) discusses a similar notion. He is concerned with the idea that kinship has to do with the sense in which a person is defined by belonging, as a person, to, and participating in, the personhood of others. *Belonging* in this sense is not different from what concerns Carsten and El Guindi. That is, kinship is about the creation of a sense of what one might call shared personhood. Now, it seems to me that this falls under my general idea (see below) that kinship creates for a newborn at once social belonging and thus (social) personhood. And it seems too that Sahlins’ argument fits in with the important cognitive science account of the human capacity for sociality and being able to empathize with others (that is, recognize, as it were, oneself in others) and see them as having one’s own sort of motives and so on; i.e., we have a sense of self that includes others’ view of self and so on in a virtual infinite regression! Furthermore, this necessarily involves incorporating others (at least one’s view of them) into one’s sense of self.

<sup>2</sup> Let me clarify what I mean by a genealogical category. It is any point in the structure of primary genealogical space (PGS; see Lehman and Witz 1974), a structure defined by ascending lines, each step being a parent relation, with line inverses from any such point descending, in a series of child relations; the number of elementary (parent-child) steps in a line or line inverse having length  $l$ . Each point defined by a line up and or a line down is what is commonly notated by a kintype string of elementary kin types—P's and C's (with sibling connections arising from C of P and affinal/spouse connections arising from P of C—child's other parent). I shall not go into the difficult question of whether literally every genealogical category defined in this relative product notation, e.g., one's mother's mother's brother's wife's parent [MMBWP] is my (social) relative. Dwight Read and I have been dealing with that and will eventually do so in print. The answer is ambiguous. The individual so labeled in genealogy talk is, in fact usually said to be at once “not a relative” and “a distant relative-by-marriage” of a “relative-by-marriage”, suggesting that the set of kin terms may have to include terms like, say, “distant relative”, as with English “distant cousin”; that is, terms for categories for which no particular rules/expectations exist as for how one interacts with them or addresses them (see Leaf, this issue), or as to whether people commonly consider such distant ‘connections’ to be kin. Clearly, a generationally remote connection such as a great great grandparent is unlikely to be thought of as having what one may call a social relationship with oneself; one will, after all, never encounter such a person! Similarly, and especially for cognatic systems where the infinitely large genealogical space is available for possible kinship, it is common to have social rules for, as it were, writing out any individual from the active status of a kin person just in case one wants to, as in the case of an actual brother who lives ever so far away and is out of touch entirely and, let us say, is the sort of person one would never want to acknowledge. In any case, what takes genealogical categories into the space of kin categories/terms (Read's Kinship Terminology Space [KTS]), is a morphism on the genealogical space, as defined below, p. 13), and it is clear that, under such structural morphisms constituting the map from PGS to KTS, no genealogical category can, as it were, fold over into more than a single point in KTS, with each such point being labeled by a culturally particular kin term. In the rare, but existing, cases where a community allows two different kin terms for a given genealogical category, it is also clear that such a system has alternative maps/morphisms from PGS to KTS.

<sup>3</sup> Note that footnote 2, above, has to make it clear that for very remote genealogical kintypes there may very well be no specific kin-term (other than, say, distant cousin and the like), and that this does not necessarily mean that such persons are not kin. Furthermore, This raises a new possibility, namely that we may need to distinguish between a *kinperson* and a (social) relative! This is not an idle matter; for, in a map between genealogical space and kin-term space, understood mathematically, there can be no specific degree, say, of genealogical distance beyond which the map necessarily has no target. Is it, say, at exactly three degrees of ascending lineality that we suddenly find that, say, P of P of P... is not kin? Certainly not. That is like asking how dead, how long dead, need someone be to not be kin. That has all the logical absurdity of the children's riddle, "How high is up?" And, in turn, we are faced here with yet another question, namely, is there perfect identity between say the space of kin *terms* and that of kin *categories*? Again, no, or anyhow, not without including in the former such residual-omnibus terms as *very distant cousin by marriage* and the like.

Let me make my point a bit more explicitly. I have worked for decades with a people (the Chin of Burma – see Lehman 1979) who have no kin term that refers directly to anyone above two generations ascending. For the latter, they have the grandfather term, *pu*. And I have shown that this term is applied in such a way that it can refer to any relation however remote ascendingly, and that this results from something that amounts to the iterative collapsing, in the map from genealogy onto kin terms, of the more distant ascending generations onto generation 3 ascending. Now, the reason this works is that, after all (as informants argue!), my grandparents are kin by virtue of being parents of my parents, who are kin to start with — the recursivity I mentioned above. Now, I have to ask: Is there any number, *n*, such that this logic stops at generation *n*-ascending? Again, No! If my grandfather, say, is a kinperson for me because he is my father's, or mother's father, he had parents and so on, so there is no way to keep from saying that my grandfather's father, grandfather and so on are kin. The informants who say this are obviously not mathematicians, yet this apparently computational argument is theirs, not just mine. But there is no specific kinship word in the language to specify such remote kin as there is nothing in their language like the English iterative operator "great". All that one can do is to use the genealogical kin-type expression "my grandfather's father" (*ka pu a pu*). This can be taken to specify the way in which a grandfather is indeed a grandfather! More directly, it indicates that a very remote ascendant (with whom one can never have a social relationship) is, indeed, a kin because one can call him *pu*, which is, without any question, a kin term.

<sup>4</sup> In actuality, this preferentiality comes down to the fact that a kin group may not let any significant time (e.g., a generation or so) pass without contracting a marriage to a maternal agnate woman—a classificatory MBD in particular—without substantial penalty.

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