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I am very grateful to the eminent scholars who took their valuable time to comment my paper on the structure of Dravidian kinship terminologies. This attention is particularly significant, coming from renowned specialists in the fields of kinship and marriage, mathematical methods and Dravidian culture. As for my credentials in these areas, I can only say that I am an anthropologist specialized in Amazonia, and a self-taught amateur mathematician who has been teaching kinship courses for a few years. I also happen to have Cashinahua friends which address my as *chai,* particularly Sian, to whom I am grateful for lessons. Having said this, I have no excuses for the lack of reference to the enormously important work on Dravidian terminologies and marriage practices in South Asia and South America, by Dwight Read, Douglas White and F. K. Lehman, except those already contained in the initial statement of my limited goals and in the final presentation of my sources in my paper in this issue.

While acknowledging my effort at clarifying "the precise structure of Trautmann's paradigm of Dravidian South Asian kin term structure" (White), and at isolating formally some central features of Dravidian kin terminologies previously described by specialists in the field, White, Read and Lehman have raised important critical remarks on my paper. One main criticism is directed to the supposed inability of my model to account for important features of Dravidian terminologies, such as the relative age distinction or the merging of consanguine and affine terms at generations +2 and -2, an inability attributed either to inherent limitations of my notation, or to a supposed disregard for facts in favor of logical consistence. Other remarks focus on the supposed inability of my model to account for the avuncular marriage. Finally, my approach to affinity was compared to White's theory of sidedness, a subject that involves also more technical points such as the use of an "even and odd" rule to calculate crossness.

My specific answers are given as numbered sections. In section 1, I address the issue of how the theory is related to empirical evidence. Sections 2-4 are devoted to clarification of the notation. This led to an explanation of differences between the "general classificatory" model (with a notation for lateral crossness and affinity) and the "Dravidian" model (with a notation for bilateral affinity only). Sections 5-7 address the issue of how the theory deals with relative age and with the classification of relatives at generations +2 and -2. They also discuss the more technical issues of how an "odd-even rule" works. Sections 8-10 deal with the relation between "sidedness" and "affinity", as well as with the issue of sister's daughter marriage. Section 11 has a more epistemological tone.

Before turning to these points, let me add a few comments of a general nature. I realize that the title of my paper, by suggesting the unwarranted ambition of proposing a theory of actually existing "Dravidian relationship systems", was not best suited to the stated goal of establishing "a calculus for kinship and affinity relationships that generates the classification of Dravidian terminologies proposed by Dumont (1953 and 1958) in the form given to them by Trautmann (1981)". I also acknowledge my bias towards the structural approach to the anthropology of kinship, represented by Lévi-Strauss and going back to Lewis Morgan, including authors as Lounsbury, Trautmann and Tjon Sie Fat. As a matter of fact, some of White's critical comments to the structuralist approach may be traced back to Leach's critique of Lévi-Strauss' analysis of the "Kachin" (Jingpaw) marriage system, while Lévi-Strauss's answers in the 1967 edition of his *Structures Elémentaires de la Parenté* have since long been part of my own intellectual background (Barbosa de Almeida 1993).

After reviewing the contributions of Trautmann, Tjon Sie Fat and Viveiros de Castro to a symposium on Dravidian kinship systems, to which incidentally Douglas White made an important contribution (Godelier, Trautmann and Tjon Sie Fat 1989), François Héran concludes in a recent book:

> "... it appears that Viveiros de Castro's combinatory, just as Tjon Sie Fat's or Trautmann's, implies the unlikely mastering of a great number of classification rules in order to identify the allied or consanguine status of second-degree cousins." (Héran 2009: 386).

As I stated in my opening paragraphs, I attempted to reduce this "great number of classification rules" to a small number of basic principles, expressed in mathematical language, but endowed with substantive content. The paper deals, as announced, with the formal reconstruction of Trautmann's calculus, for which end I was led to develop a language for expressing genealogical paths in a "genealogical space" generated by a genitor term *f* and a opposite-sex sibling term *s* (see Section IV). By the genealogical space K*, which I call sometimes the language K*, I mean the set of all possible concatenations of symbols chosen in the vocabulary K = {*e, s, f,* f^{-1}}. Another way of describing this construction is to say that K* contains, besides *f* and *s,* their inverses (the inverse of *f* is f^{-1} and the inverse of *s* is *s* itself), an identity element *e* and all possible *words* obtained by composing these operators. Since this formulation caused the impression that my goal was to determine by formal means the properties that Dravidian terminologies, ignoring the evidence to the contrary, I thought it necessary to start my Reply by an explanation of my views on this matter.

1. Theory and facts

The goal in constructing this artificial language is *not* to construct a grammar for a subset of natural languages. In other words, the artificial language of kin terms built by means of symbols B, Z, F, M, S and D is *not* intended as a grammar that would generate strings having syntactical or phonetic similitude with strings in any natural language, even in the restricted domain of kinship. This would have been a *linguistic* problem, not a problem in cultural theory. The formal language K* is a means to construct a genealogical space endowed with a very simple structure. The basic hypothesis is that kinship terminologies in natural languages are distinguished in the way they *classify* the paths in the genealogical space. To describe these actually existing classifications is a task of *empirical* research. The task of the theory is to construct a *theoretically-based* classification of the genealogical space that should reproduce the empirically given classification, or some relevant feature of it.[1] I give now a more precise formulation of the goal of the theory of kinship terminologies in the above sense.

Morgan invented the following method of studying kinship terminologies.

(1) Start with a list *S* of expressions denoting *genealogical paths* in plain English (Morgan's equivalent to kin type language or KTL).

(2) Look empirically, for each genealogical path in *S,* the word corresponding to it in the sub-set of kinship terms L_k of a natural language such as Seneca or Tamil. The result of operation (2) is an *empirically defined* mapping $\varphi: S \to L_k$ of the set *S* of genealogical paths into the terminological set L_k). For instance, $\varphi: S \to L_{seneca}$ or $\varphi: S \to L_{tamil}$.

(3) Form the *classes of genealogical paths* (sub-sets of *S*) that are associated to the same *kinship term* in the natural language in question. This is the inverse mapping $\varphi^{-1} : L_k \to S/\varphi$ where S/φ is a sub-set of $\mathcal{P}(S)$. For example, $\varphi_{seneca}{}^{-1}$ (*hä-ni*) = {♂F, ♂FB, ...}, and the set {♂F, ♂FB, ...} is *empirically* given in Morgan's table, being a an element of taxonomy S/φ_{seneca}. The classes in S/φ are equivalence classes. They are a *classification* of the genealogical paths, obtained *empirically* and described by examining the mapping φ.

[1] While the syntax of natural languages is clearly non-associative (see Ballonoff in this issue), it is not evident that the same applies to the underlying semantical structure of the kinship domain. An hierarchical three tree is a semantical structure for the theory of lineage systems, and the Klein group is a semantical structure for Kariera-like or Panoan section system. In mathematical logic, set-theoretical structures are "semantical structures" in which formal languages are interpreted.

This can be seen as an *extensional* definition of an (implicit) taxonomy contained in the natural language L_{seneca} (properly speaking, a sub-set of the Seneca language).

The task of the theory proposed, in an approach indicated by Lounsbury and Trautmann, among others, can now be formulated.

(4) Compare the taxonomy induced by distinct terminology vocabularies L_k , and group distinct kinship terminologies together when they share their taxonomies of the genealogical space S. At this point, we obtain "*families*" of kinship terminologies, as those kinship terminologies that induce the same taxonomy of the genealogical space S. Thus, $\{L_{seneca}, ..., L_{crow}, ...\}$, where L_k is the set of kinship terms a natural language, is a "family" to which corresponds the same classification of S, that is to say: $S/\varphi_{seneca} = S/\varphi_{crow} = ...$

(5) If possible, give a theoretical description of the taxonomy associated to each *family* in the sense above. In other word, express the extensional taxonomy *S/*φ by means of theory *T*. In our version, theory *T* is be given by a set of axioms. Thus, in the case of classificatory terminologies, the axioms C should induce a partition *S/T* of the set of "genealogical paths". We should obtain

$$S/T \approx S/\varphi$$

where the relation "≈" suggests a reasonable correspondence between the theoretical and the empirical classifications of the genealogical space: *S/T* obtained by means of the proposed axioms/rules of T, and the empirical family S/φ obtained by means of fieldwork techniques resulting in the mapping φ.

We summarize this by saying that the goal of the theory is to reproduce the empirical extensional classification of S obtained by the empirical mapping φ by means of a theory *T* means that we expect that a reasonably close fit holds between the two. We then say that the theory *T* "save the facts".

An interesting of this formulation is that my proposed approach oriented towards a structural realism is consistent with an empirical-formalistic position from the point of view of a pragmatic theory of truth (cf. Da Costa and French 2005). This means that the theory *T* may be extended into a theory *T'* which include, say, hypothesis on cognitive mechanisms underlying the formal classification *S/T,* but are not accepted by an empirical-formalist. Notwithstanding, since both theories *save the same phenomena, T* and *T'* can be said to said to *pragmatically agree.* On can also say that T and T' agree on the sense of the pragmatical truth.

In my opinion, this is the field of studies created by Lewis Morgan, which may be labeled "classical kinship studies" as opposed to "new kinship studies" in the aftermath of Schneider's critique of Morgan. It is not, a repeat, a sub-field of the syntax of natural languages, nor, for that matter, of sociology.

I assume therefore, *contra* Schneider, and in the company of Morgan, that English and Seneca speakers may communicate on kinship issues, in the sense that they can *pragmatically agree* (Barbosa de Almeida, 1999] on semantical distinctions implicated in the underlying *general classificatory space* K*. Ultimately, the means used by me to construct this space imply that the *opposite-sex* distinction (expressed by *s*) and the *same-sex filiation* (expressed by *f*) are somehow semantic universals.

2. The Classificatory Model and its notational features

The set of genealogical paths is represented by the words in the formal language K* generated by term $\{e, s, f, f^{-1}\}$. This representation of the genealogical space is sufficient for a theory of classificatory systems in the sense of Lewis Morgan. In fact, this formal language already implies a central feature of classificatory systems, which is the identification of lineal relatives with collateral relatives. This is so because of our decision not to introduce a separate symbol *b* for *same-sex siblingship,* separate from *e.* The structural behavior of "*e*" is that of an identity symbol. Formally, it has the role of a mark for the absence of a symbol. We used it to represent "same-sex siblingship" in "classificatory" taxonomies because, in these taxonomies, a symbol for same-sex siblings can be formally *erased* when combined with other symbols (this is a more general form of Lounsbury's "merging rules").

A symbol *b,* not identical with the identity, expresses the *colaterality* distinction that is essential in *descriptive* systems. If we had added the symbol *b* in our presentation, "classificatory" systems would be distinguished by the following rule:

$b \rightarrow e$

This rule would express the fact that, in terminological families gathered together by Morgan as having "classificatory systems of kinship and affinity", collateral relatives are merged with lineal relatives. We use the symbol for colaterality in a forthcoming paper on Dravidian, Iroquois and Crow-Omaha kin taxonomies.

The taxonomy of genealogical paths in K* resulting from my axioms C is the taxonomy K*/C, which is the same as [K*] in my paper in this issue.

I am afraid this aspect of my paper was obscured by what in retrospect I see as the unfortunate decision to postpone the presentation of the general language K* and of the classificatory taxonomy [K*] to the Section IV of my paper, where it has the role of making a transition for a forthcoming paper in which Iroquois, Dravidian and Crow-Omaha terminological taxonomies are jointly analyzed.

In the more general approach to classificatory terminologies (see Section IV of my paper in this issue), I introduce a set of abbreviations for selected genealogical paths (described by formal words in K*), which are meant as definitions of same-sex crossness ($\mathbf{x}$ and $\mathbf{x}^{-1}$) and same-sex affinity (a and a^{-1}). With these abbreviations I obtain a language $K^*_{x,a}$ extended with terms for crossness and affinity. The enriched language $K^*_{x,a}$ contains all genealogical paths ("words in $K^*_{x,a}$) but contains also means to rewrite these genealogical paths by means of expressions with *cross* symbols and *affine* symbols. Clearly, this creates an ambiguity in the representation of genealogical paths. We solve the ambiguity problem by means of the affinization rules. This again resulted in misunderstanding, and I therefore proceed to provide a motivation of the affinization rules.

3. The Dravidian Model and its notational features

In our paper in this issue, sections I, II and III deal with the language D* which contains all "kin words" in K*, and in addition all combinations of these words with the added symbol "*a*". In contrast with the more general classificatory model, the Dravidian model uses the single symbol "*a*" to stand for affinity and crossness, and for same-side relations (isolaterality) and for opposite-side relations (anisolaterality).

This intended behavior of "*a*" is accomplished by means of the affinization rules" A1, A2 and A3 and rules D1 and D2. [2]

It could be inferred that the introduction of an affine language would destroy the uniqueness of the representation of classificatory genealogical paths, since now the same genealogical path may, in some cases, be represented either by a "genealogical" word or by its abbreviated "affine" version. This does not happen because axioms A1, A2 and A2 have the form of *directed rules.* Together with the classificatory equations C1 and C2, the affinization rules A1, A2 and A3 (combined with Dravidian rules D1 and D2) result in a taxonomy of D* which we claim to reproduce relevant features of the empirical taxonomy of "Dravidian" languages. The same result could have been obtained with the more general language K* of Section IV, by means of definitions K1-K4, and the Dravidian Axioms DA1 and DA2.

Although the "affinization rules" are not essential to my approach, they were the first result of my analysis of Trautmann's rules, and I add now an explanation of their underlying motivation. The point of introducing the affine mark "*a*" is to make it possible to distinguish kin terms as "non-marked" and as "marked". My technical device for introducing this mark was to use it as an *abbreviation* for certain genealogical paths. Thus, in Dravidian terminologies some genealogical paths (linking spouses through their children, and linking cross-cousins through their parents) may now be represented also by a "consanguine" path *together with a mark "a".* Thus, if we introduce the symbol "A" in the kin type language, the genealogical path ♂FZD (a "cross" path) is represented alternatively as ♂AF (my affine's father, I male), an expression containing now a consanguine term "F"and a affine symbol "A".

We recall here Lehman's remarks on the Fregean dichotomy between denotation and connotation. The denotative redundancy (no matter which language we use to denote genealogical paths or points) may be seen as connotative duplicity: ♂FZD denotes the same genealogical position as ♂AF, but they are different ways of expressing it. The underlying hypothesis is that a system for classifying Dravidian kinship terms must include rules for eliminating redundancy, by providing an answer to the following question: when should a given path in the classificatory genealogical space be represented by an "affine" form, and when should it be represented by a "cross"form? This question is formally answered, in the Dravidian case, by the "affinization rules" in the form of directed rules. The partition induced in the space of genealogical paths (described by formal words in the canonical

[2] In fact, Dravidian Rules D1 and D2 may be derived from affinization rules. And both may be derived from axioms DA1 and DA2 introduced in Section IV.

reduction of D*) should reproduce the empirical classification of kinship words in “affine” terms and “non-affine terms”

The directed rules may be given a simple interpretation, which I offer with Lounsbury’s phrasing, although this interpretation was not intended.

Rule A1 says (I male): *let my father's sister be called my mother's affine.* From a female perspective, rule A1 says: *let my mother’s brother be called my father’s affine.*

The second "affinization rule" A2, in the same style, can be read from the male perspective as: *let my sister's daughter be called my daughter’s affine.* From a female perspective, this reads as: *let my brother’s son be called my son’s affine.* Interpreted in this way, affinization rules assert that, given two alternative ways of representing a relative -- one of them without an "affine” connotation, and the other with such a connotation, Dravidian speakers should ideally select an expression that contains a affine mode of expression. Thus, affinization Rules A1-A2 are just a way of expressing what is to me the central point in Dumont’s 1953 paper on South Indian kinship terminologies, as well as in the important and somehow neglected 1975 book by Joanna Overing on a South American kinship terminology (Overing [Kaplan] 1975).

The third affinization rule A3 has quite a different status. It was introduced, almost as a technical device. It may be read as: *let my children's genitor (of opposite sex) be called my opposite-sex affine.* In kin types, this would read: ♂SM → ♂AZ, and ♀DF → ♀AB. Note that under the Dravidian axioms, ♂AZ = ♀ZA (my affine’s sister is also my sister’s affine). Note also that ♂SM = ♂DM, and ♀DF = ♀SF. Thus, in kin type notation the rule could be written as ♂ChM → ♂AZ, ♀ChF → ♀AB. In this formulation, a spouse is taken as a affine’s opposite-sex sibling.

This formulation assumes the affine relation as a same-sex relation, as in Dumont’s theory, and follows Lévi-Strauss’s view that a marriage is, sociologically and cognitively speaking, an “alliance” between same-sex actors, mediated by an opposite-sex actor: the transfer of a man’s sister to other men to whom she will be a wife, and also, in my gender-neutral language, the transfer of a woman’s brother to another woman to whom he will be a husband.

The rule has also another implication, in that it defines the same-sex relation between affine through a genealogical path that goes down through children and then up again through the opposite-sex parent. It is not cognitively obvious that, even for an idealized Dravidian speaker, that a marriage should be defined as a relation linking two opposite-sex genitors of common children. This rule should therefore be understood in a classificatory sense. It suggests that divorced co-parents should be seen somehow as related as part of a continuing alliance relation between same-sex affine through the (real or potential) children of their own and of their classificatory sibling’s marriages. The definition of marriage alliance through common children may be traced to Macfarlane (1882), with the difference of the role of sex change in my definition. I became aware of Macfarlane’s definition by reading Héran’s book (2009), brought to my attention by Viveiros de Castro.

Section I.3, I introduce finally the Dravidian Rules D1 and D2. These rules have a computational flavor, since they say that, in order to obtain the "canonical Dravidian form", we must not only use the C rules and "affinize" whenever possible by means of A-rules, but we must also systematically clean up all "*a*" (they will crop up as a result of A-rules) by grouping them at the right side of the word (rule D2), and then canceling all pairs "*aa*" (rule D1).

The net result of these rules is that in a terminal ("canonical") word: (1) there will be no occurrence of "cross" expressions such as MB, FZ, ♂ZD, ♀BS, and also no occurrence of "marriage" expressions such as ♂ChM, ♀ChF, all of which will have been replaced with expressions involving the affine "*a*" mark by means of the A-rules; (2) all "a" will have been cleaned up by means of D rules, resulting in a word having a single mark "a" or none at all; (3) the resulting word will be classificatory reduced (it will merge fathers and father's brothers, and so on).

This is an opportunity for making one more remark on the charge of "formalism". I do not see myself as a "formalist", but as a "structuralist" in a sense that I try to explain with an example. I was impressed with the discovery that whole logic of the Dravidian affinity calculus could be encapsulated in two rules A1 and A2 that say respectively: *transpose fs into sfa,* and *transpose* sf^{-1} *into* $f^{-1}sa$. As I have shown in my section on "signed notation", these rules may be expressed also as: *transpose fs into - sf,* and *transpose* $s\,f^{-1}$ *into* $-\,f^{-1}s$. The latter formulation has the form of a anti-symmetrical operation, which is found in realms such as quaternion multiplication or quantum physics. Although I have followed leads such as these, guided by "symmetry arguments" (van Fraassen 1989), the underlying constraint was that results of these exercises should be checked against data.

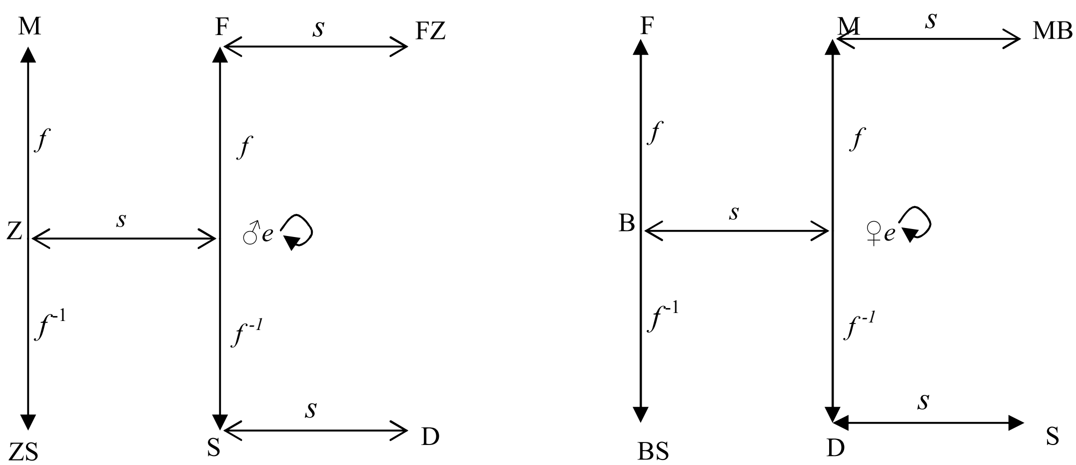
4. How the notation represents genealogical paths

It seems that my presentation gave the false impression that the language D* (or for that matter the language K* can only express same-sex filiation links. The point, as Read puts it, is that the vocabulary K = $\{e, s, f, f^{-1}\}$ has a single term for same-sex child-parent filiation *f* and its inverse (reciprocal) parent-child filiation f^{-1}. As a consequence, the kin type ♂F is translated as ♂*f* and the kin type ♀M is translated as ♀*f*. How are then ♂M and ♀F translated? Or, for that matter, since ♂S is translated as ♂f^{-1} , and ♀D is translated as ♀f^{-1}, how are ♀S and ♂D translated? These are the answer: we express ♂M as ♂*sf* and ♀F as ♀*sf,* and we express ♀S as ♀$f^{-1}s$ and ♂D as ♂$f^{-1}s$.

The difficulty is due to relational character of the language and of the role played by the ordering of symbols. The relational character lies in the following feature: symbols do not "have sex", they only *change the sex-index of the preceding string,* just as "F" and "M" do not have an absolute meaning in kin type language, but only add *one generation to the previous string.* Thus, the symbol "*s*" expresses *change of sex,* so that ♂*s* changes sex from male to female (think of it as ♂Z), and ♂*sf* add a generation level (♂Z is changed to ♂ZM or ♂M). There are only two symbols that may be said to have an absolute meaning in my notation: the marks ♂ and ♀, which must be appended to any string in order to endow it with a definite meaning, and act as the *origin of coordinates* for each genealogical path.

Let me make my point as a proposition: to each genealogical position given by a string of kin types F, S, D, B, Z (preceded by ♂ or ♀), there corresponds one and only string (genealogical path) of terms in the set $\{e, s, f, f^{-1}\}$, preceded by one initial symbol ♂ or ♀. I offer a visual "proof" in Diagram 1 below.

Diagram 1. Graph for words in K* (edges) and kin type (nodes)



In Diagram 1, kin words made with the vocabulary $\{e, s, f, f^{-1}\}$ are represented as paths of connected arrows, and kin types are indicated as nodes. Given ego's sex, to each kin type (node) there is one and only one kin word (path) in K*. This proves the assertion for paths of length 2. I hope the reader will convinces her/himself that grafting a similar graph at each terminal node will preserve the one-to-one correspondence between kin types (nodes) and kin words in K* (paths).

Note that kin types shown in the graph correspond to "contracted words", which are here paths of minimal size. A path such as $f f^{-1}$ goes back to the origin and is thus contracted to the path *e,* which is therefore drawn as a loop. This corresponds to Lounsbury's merging rules ♂FS → ♂B and ♀MD → ♀Z. Diagram 1 looks cluttered, but most of the cluttering is redundant information. We do not need labels on arrows, because it is clear that two-tipped arrows represent "*s*", that upward-oriented arrows represent "*f*", and that downward-oriented arrows represent "f^{-1}". The identity loop representing "*e*" may be omitted – in fact, the loop should have been attached to each node in the diagram, and its omission is already an effect of using the classificatory rules, which say that e may, erased, except when it is the only letter is a word. On the other hand, *s* represents a *change in the sex of the preceding path.* Consider the path *fs* leading to ♂FZ. This path is composed of a generation change followed by a sex change. On the other hand, ♂M must include also generational and sex changes, and this leaves as the only option the path *sf.* Taken literally, the path ♂*sf* is translated as ♂ZM, and this means that by labeling the node at ♂*sf* as ♂M we implicitly define ♂ZM as ♂M. This identification may be understood as a kind of half-sibling rule. In a similar way, ♀BF is identified as ♀F. As the diagram shows, the path to ♂D is ♂$f^{-1}s$ and the path to ♀S is ♀$f^{-1}s$.

If the kin type language and the K* language are isomorphic, why use the K* language at all? The reason is that the K* language lends itself to mathematical operations. The calculus of reciprocals becomes a mechanical operation. For instance, Lounsbury's Omaha skew-rule is written as FZ→ F, but its reciprocal form must be written as ♀BS/♀BD → ♀S/♀D. This asymmetry is not inherent in the subject, but is an artifact of the kin type notation. What is at stake is that FZ→F is an ambiguous expression, which assumes a definite sense only when prefixed by a sex symbol, and thus should be rendered as ♂FZ→♂Z and ♀FZ→♀Z, with reciprocals ♀BS →♀B and ♀BD→♀D. In the K* language, the rule may be written in ambiguous form as $fs \rightarrow s$ and its reciprocal is $sf^{-1} \rightarrow s$ which is obtained by reading each word in the reverse order and taking the inverse of each symbol (the inverse of *s* is *s* itself).

The main computational convenience of the language based on words of the vocabulary K = {e, s, f, f^{-1}}, or words in K*, is that finding the representative of the equivalence class (the "contracted" version of all words in the class) becomes the mechanical operation of canceling all "ff^{1}", "*ss*" and "*e*" in a word, until no further cancellation is possible or "*e*" is the only symbol left. In particular, this method leads to the solution of the *classificatory word problem for kin words,* which is to determine, for two arbitrary kin expressions, whether or not they are classificatorily equivalent in the sense of Morgan. This is done as follows: reducing each word by means of contractions and compare the result. The word problem for words built with the vocabulary D = {e, s, f, f^{-1}, a} is similarly solved with the help of the Dravidian rules. This should dispel the impression that my notation is somehow unable to express the full genealogical space, or that it implies a parallel set of separate terms for "father" and "mother", one for male speakers, and another for female speakers, "against ethnographic facts".

On a less technical side, the use of only one filiation term might suggest that I take same-sex filiation as more fundamental than opposite-sex filiation. Professor Trautmann has suggested in personal communication that if this interpretation was not intended, I should say so. This interpretation was indeed never intended, and the use of two primitive terms to express opposite-sex filiation was a technical device. However, I realize that somehow the notation suggests the logic of unilinear descent systems. I also came to notice a parallelism between Radcliffe-Brown's principles of "solidarity of the sibling group" and "solidarity of the lineage group" and the formal roles of the operators *e* and *f,* connecting respectively male siblings group and the unilinear lineage groups.

5. Representation of relative age

Some commentators criticized my model for failing to represent the relative age distinction, and attributed this omission in part to an inherent limitation of the notation, and in part to the dismissal of empirical evidence. The first remark to be made is that, while relative age distinction is pervasive in Dravidian kin terminologies, it is not a specifically Dravidian feature, being found in all varieties of classificatory terminologies, and beyond them. Thus, one should not expect that the structure of Dravidian terminologies should necessarily hinge on the logic of relative age, but instead that relative age features might be of a more general nature, at the level of the classificatory rules. This is not a problem of notation, but of answering the following fundamental question: *is the relative-age structure part of the genealogical taxonomy that classify genealogical paths on the basis of generation and sex* (*and affine/non-affine status*)*, or is it an autonomous ordering applied to* (*non-affine*) *sibling sets already classified as such*? Depending on how the ethnographic evidence on such questions, the representation of relative age terminological distinctions should be incorporated either as a feature of the basic theoretical description of Dravidian systems, or combined to it as a distinct feature, probably at the same level of generality as the classificatory features under C-rule.

At this point, I want illustrate my assertion that this is not an issue of notation.

First, introduce in the language K* a new symbol for the same-sex sibling *b*. This is a departure from our previous usage of using the *e* symbol to this end, but it is a necessary departure for dealing with more general terminological structures, as I do in a forthcoming paper. Let *b* and *s* stand for, respectively same-sex sibling and opposite-sex sibling. Next, mark these symbols as b_+ ("older same-sex sibling") and b_- ("younger same-sex sibling"), and similarly mark s_+ and s_- to stand form "older opposite-sex sibling" and "younger opposite-sex sibling". The language K* for genealogical paths, enlarged with a notation for relative age, can be mapped into the vocabulary of kinship terms containing relative-age distinctions. The problem now, as mentioned above, is whether these symbols lead to a structural calculus. I use now Morgan's data for Seneca kin terms merely as a means of suggesting the nature of the problem (Morgan 1997[1871]).

(1) "My elder brother (male speaking)" (♂B_+; ♂b_+) is mapped into *ha-je*

(2) "My father's brother's son, older than myself (male speaking)" (♂FBS^+; ♂$(fbf^{-1})_+$ is mapped into *ha'je*.

This implies that the two genealogical paths 1 and 2, marked for relative age, are classified together by the terminology. How should this empirical fact be expressed in as theoretical rules acting on the terms of the language K* enriched with relative age marks? The crucial point here is that the specification "older than myself" seems to apply to the genealogical path "My father's brother's son" already classified as a "brother". To put it differently, assigning a relative-age position to the genealogical path ♂$(fbf^{-1})_+$ does not depend on "*b*" at the second generation (in which it "brother" is unmarked, as suggested by my notation ♂fbf^{-1}), but instead on the age ordering of the actual classificatory sibling set at ego's generation. If this is the case, it seems to be necessary, first, to use the transformation ♂fbf^{-1} → ♂*b* and *then* choose ♂*b*→♂b_+ or ♂*b* → ♂b_- on the basis of information on the actual age order within the classificatory sibling set.

Morgan's tables give for Tamil:

(3) “My elder brother (male speaking)” (♂B_+, ♂b_+) is mapped into {*en tamaiyan, annan*} (“Systems”, Table III, 17)

(4) “My father’s brother’s son – older than myself” (♂$(FBS)_+$, ♂$(fbf^{-1})_+$ is mapped into {*en tamaiyan, annan*} (Table III, 63)

(5) “My father’s brother (if older.)” (♂FB, ♂$(fb)_+$) is mapped into {*en periya takkappan*} (Table III, 61)

(6) “My father’s brother (if younger than my father)” (♂FB_- , ♂(fb_-)) is mapped into {*En seriya takkappan*} (Table III, 61).

In (4), the mark for relative age seems to be applied to the expression (fbf^{-1}) as a whole, suggesting that relative age is measured from ego’s perspective. In (6), Morgan adds that relative is measured from the perspective of ego’s father. In (5), the meaning is not clear to me.

The Cashinahua have alternate generations, and ego’s grandparents and ego’s grandchildren are his name-sakes and classificatory siblings. In particular, the zero generation affine term *chai* is applied to grandparents and to grandchildren of the opposite moiety, but with linguistic modifications that may express relative age distinctions (Sian Kaxinawa, personal communication). This could be expressed as a rule

$ff \rightarrow b_+$

$f^{-1}f^{-1} \rightarrow b_-$

Another context where relative age might be operative across generations is avuncular marriage, particularly the marriage with one’s sister’s daughter. The equation expressing this kind of marriage could run as

♂$s_+ f^{-1} \rightarrow$ ♂as (let my older sister’s daughter be called my affine’s sister),

It is far from my intention to suggest that formal manipulation can replace empirical research. My point is that the inclusion of relative-age features in a *theoretical* model of Dravidian terminological structure (by means of sets of axioms), as opposite to a *descriptive* model (as in an extensional listing of taxonomic categories) is not a notational problem, but an empirical problem. Another tentative conclusion is that the relative age feature belongs to the “classificatory” level, and is not a specifically “Dravidian” feature.

6. Classification at +2 and -2.

Read asserts that I contradict the empirical evidence, in favor of mathematical self-consistency, because under rules C-D-A (or the more general axioms and definitions in my Section IV) grandparent's and grandchildren's generation are classified in affine/consanguineous classes as in Trautmann's Model B. I quote:

"In effect, de Almeida assumes his formalism captures the essence of a Dravidian terminology and if, mathematically, it leads to a Model B terminology, then a Model B must be the essence of a Dravidian terminology (...) Here formalism has taken precedence over ethnographic evidence."

Not being a specialist in Dravidian culture, I do not claim, even remotely, to have captured "the essence of a Dravidian terminology" by means of a "formalism". Let me re-state my claim: to have proved that the set of C-A-D rules generates a complete classification of the infinite set of paths in the genealogical space (phrased in K* language) in two opposed sex classes and two opposed "non-affine/affine" classes for each generation. This is the content of my Proposition 1, which says that every word in D* is reduced eventually to the "Dravidian canonical form" by the rules C-A-D. But the "Dravidian canonical form" does not specify any limit for generational depth, and applies virtually to an infinite number of generations! This means that my theoretical model, does not specify any generational boundary.

Rules C-A-D (or the methods I explain in the Corollaries, Rules of Thumb, and in Section IV) can be used unambiguously to determine all Model A properties in a simple way: *first*, apply the rules to determine the "consanguine/affine" bipartition of any kin word at the three medial generations (G^{-1}, G^{0} and G^{+1}), going up and down as many generations as is needed, or going down and up as many generations as needed, and, *after that,* collapse the classification between non-affine and affine bipartition at generations G^{-2} and G^{+1} (more on that later on), and just forget the existence of any further generational level. This is how my set of rules generates all properties of Model A. The same procedure can be applied to generate all the features of Model B (in this case, do not collapse the distinctions at generations G^{+2} and G^{-2}). One can see that there is no real difficulty in obtaining all features of Models A and B (ignoring issues such as relative age and oblique marriage).

Note however that, in order to make the method work for *any genealogical distance* (e.g. without having to *set a memory-span limit* as to how far can we "go up" before going down, or how far can we "go down" before "going up" again), we need to be able to apply the calculus to an unspecified number of generations, as an intermediate step. This is the technical explanation for the fact that my calculus deals with an infinite number of generations.

Going up and down along a genealogical paths out the range of the three medial generations, going up, say, to G^{+2} and then going down to G^{0}, is a procedure implied in the genealogical diagrams published by Trautmann and Barnes (1998, p. 31, Figure 2-1), and by Tjon Sie Fat (1998, p. 70, Figure 3-4), where generation G^{+2} is represented as part of the genealogical path that leads to points at generations G^{+1}, G^{0} and G^{-1}, but it is itself left unclassified. Notwithstanding, it is precisely at genealogical paths that *go through intermediate links at generations* G^{+2} that lies the source of the difference between Iroquois and Dravidian crossness. The same situation applies in my theoretical model to down-and-up genealogical paths.

I do not venture to know about the actual cognitive mechanisms involved in the process of calculating affinity/crossness for kin that can only be reached by a genealogical *detour* of two or more generations. What I may say is that my method deals with all these cases, and also with *any ascending-descending* path, as well as with *any descending-ascending* (or "affine") path, and thus account for the Iroquois/Dravidian different ways of classifying relatives *on the basis of simple axioms* (the difference between Dravidian and Iroquois rules is the subject of a forthcoming paper).

I do not presume that in cultures that make the "Dravidian" systematic opposition between non-affine and affine, people do use rules C-A-D along a complex genealogical path in order to classify relatives (just as we do not use the definition of "sum" to sum large numbers). My axioms/rules are structural statements, which may or not have immediate cognitive interpretation. Corollaries 1 and 2, applied to ascending and to descending generations, already give simple practical rules to classify genealogical paths.

The issue of how people actually do the reckoning, and whether it is consistent with the consequences of my axioms, is an *empirical* one, and I welcome all the information available on this (however, I do not think that rules stated by native speakers are evidence enough on how they actually classify genealogical paths, just as Portuguese "rules" stated by me as a native speaker insufficient evidence on how I actually use my Portuguese language).

Technical points on which I have doubt include: (1) should we apply *first* the unlimited form of the model to reproduce correctly the classification, and *then* apply "generation limitation" and "distinction-collapsing" rules? (2) Is there an *upper limit* or a *lower limit* for the number of generations used in intermediate calculation steps?

Trautmann's rules 8A and 9A were intended to generate a "double classification" of the same genealogical path as affine/non affine (expressed in his notation by the presence/absence of the symbols "A"). "Double classification", or classificatory inconsistency, is a mechanism to neutralize the affine/consanguine opposition at generations +2 and -2. Tjon Sie Fat has shown how inconsistent classification might be traced to non-associativity. Note that none of this authors addressed the issue of how to set a limit to generational depth, having only addressed the issue of how to neutralize oppositions at generations +2 and -2.

Since it was never my intention to assert that it was "true" Dravidian terminology, let me repeat what I think I did prove: that axioms C-A-D imply a bipartition of paths in the genealogical space into consanguine/affine paths for any generational level. More precisely: Proposition 1 says that every genealogical path (word in K*) is reduced eventually to the *Dravidian canonical form* by means of rules C-A-D, and each word in this form either has a single mark "*a*" for affinity or has not such mark. My definition 4 of the *Dravidian canonical form* sets no upper limit to the generational exponent *k.* The implication is that the rules generated the affine/non-affine partition model for any generation (see footnote 12 to my Proposition 5 at page 29). In Table 1 (page. 20) for Model B structure and in Figure 2 (page. 21), I represented five generation levels, in order show that, up to +2 generation and down to the -2 generation*,* my model and Trautmann's intended model B agree (my Table 2 at page. 22).[3]

[3] The first version had mistakes, corrected with the help of Read's comments. I am to blame for any remaining mistakes.

This is a good opportunity to explain a methodologically important point that goes beyond the technical explanation given in my previous paragraphs for this feature of my model. Far from expressing a dismissal of facts, the lack of generational limitation in my canonical form reflects my respect for empirical reality. As stated above, it would have been easy to devise ad hoc formal rules to generate the *empirical* cases. This is how this could be done, in a formulation that encompasses A-Models and B-Models and other possible models.

(I) Apply all rules (C, A and D) to transform the initial word (a path in the genealogical space K*) in the canonical form (a selected path marked as representative).

(II) If the final generational index k is 2 or -2, then erase the affine mark at this level and at all higher levels (this leaves every expression unmarked for affinity).

(III) Suppress all generation levels above levels +2 and -2 by replacing the generation index k with 2 or -2 according to whether k is positive or negative.

I think that my analysis calls the attention for the need of empirical research on two structural features implied in the distinction between models A and B. The first feature is the collapsing of the "cross/non-cross", "affine/non-affine" oppositions at a certain generational distance. The second feature consists in setting a limit to the number of generations that are included in terminological set. *Double classification* remains a valid hypothesis, even if rules 8A and 9A are not the way to give it a precise form, and Tjon Sie Fat's thesis on the role of "partial non-associativity" to account for it is an interesting approach to the problem.

I have addressed in a cursory manner two possibilities for generational rules in Section III.4 of my paper. At this point I suggested: a Dravidian generation-limitation rule (telescope all generations into five levels) and a Kariera generation-limitation rule (use alternating generations to collapse all levels in two levels). The suggested "forgetting rule" for the Dravidianate case was inspired in the ethnographic evidence for a generational memory span under 2 generations for lowland Amerindians, with exceptions (see review in Viveiros de Castro 1996), and also on Evans-Pritchard notion of a fixed, "structural" lineage depth set at about five generations for Nilotic Asia (Evans-Pritchard 1940:199).[4] Amazonian lowlands also provide a second solution in the form of the alternating generation principle of the Cashinahua and other Panoan-speaking people (a summary in Hornborg 1988:167). To sum it up, leaving the model underspecified as how to account both for (1) the collapsing of the affine/non-affine opposition and for (2) the limited number of generations, left an open door for ethnographically grounded hypotheses presented in section III.4, and for other such hypotheses. Viveiros de Castro has proposed that in the context of Amerindian affinity, as genealogical distance increases, and/or the spatial-sociological distance increases, all paths become merged as *affine.* Affine status would thus the default, or unmarked, status This line of thought could be given a *metric* formulation, and has far-reaching implication both for

[4] The first anthropologist to apply Dumont's "Dravidian" model to Amerindian populations, in his splendid monography on the Piaroa, was Joanna Overing [Kaplan], 1975.

kinship theory and for other domains of cultural theory (Viveiros de Castro 2002, chapters II and VIII).

7. Calculation methods for crossness. Odd-even rules

White sees a possible conflict between his method and Trautmann's notion of affinity, and Lehman writes that "counting up is never made clear and certainly not explicit or commented upon" in my paper, implying that this operating is the fundamental rule for determining the affine x non-affine opposition, and that my notation could be a hindrance for expressing such a rule. This is one example of how White states his sidedness rule for relatives connected by and ascending-descending genealogical path:

> "They are *cross*-sided if the combined total of their female links $\boldsymbol{F} = \mathbf{f_m} + \mathbf{f_w}$ **= even**, as with ♂MBD, ♂FZD and ♂ZD (where $\mathbf{f_m} = 1$, $\mathbf{f_w} = 1$ and $\boldsymbol{F} = \mathbf{2}$)."

This specific example is a good opportunity to illustrate how my own methods assign a cross status to ♂MBD as well to ♂FZD and ♂ZD, thus agreeing with White's rule at these particular instances, but not on more distant relations where Iroquois crossness and Dravidian crossness do not coincide.

First, translate ♂MBD as ♂$sfsf^{-1}s$. .

The method of rules.

1) ♂$s(fs)f^{-1}s \rightarrow$ ♂$s(sfa\,)f^{-1}s$ (Affinization rule A1)

In this instance rule A1, the string ♀*fs* (♀MB) is replaced with ♀*sfa* (♀FA), taking care to change ♂ into ♀ because the initial ♂ is transposed with an "*s*". This means that (I male) my sister's MB is her FA.

2) ♂$(ss)fa\,)f^{-1}s =$ ♂$efaf^{-1}s = faf^{-1}s$ (Classificatory rule: ♂ZB = ♂B).

3) ♂$f(af^{-1})s \rightarrow$ ♂$f\,(f^{-1}a)s$ (Dravidian rule DA2)

This rule says: an affine's genitor is a genitor's affine.

4) ♂$ff^{-1}as \rightarrow$ ♂$eas =$ ♂as □ (Classificatory rule)

The irreducible word is affine. It reads: ♂AZ=♂W, my affine's sister who is also my wife (I male).

The axiomatic method

A more direct way to arrive at the "canonical form" is to use directly the definitions and axioms at Section IV:

1) $sfsf^{1}s = s\,(fsf^{-1}s) = s\mathbf{x}$ (definition K1 of direct cross-cousin)

2) $s\mathbf{x} = sa$ (Axiom DA1)

This is fast, and gives more information: the expression is both cross and affine.

The permutation method

1) Check the generation index of $sfsf^{-1}s$. Answer: 0. To obtain the answer, cancel every pair f and f^{-1}, and take the exponent of the remaining generation term (if none is left, the exponent is 0. This means that we can apply indifferently the two forms of the rule: transposing all "*s*" to the right side of the word, or transposing all "*s*" to the left side of the word. We chose the transposition to the left.

2) Write the permutation that shifts every "s" to the left:

Perm $(sfsf^{-1}s) \rightarrow sssff^{-1}$

3) Count *for each "s"* how many transpositions ($fs \rightarrow sf$, $f^{1}s \rightarrow sf^{1}$) were used in the above permutation. The resulting number is 3. I explain in detail how the counting is done. The first "*s*" at left needs 0 transpositions to be shifted to the left. The second "*s*" must be transposed once to the left, contributing 1 transposition to the total. The third "*s*" transposed twice, first past "f^{-1}" and then past "f", thus contributing with 2 transpositions. Total sum: 0+1+2 = 3.

4) Write $s^3ff^{-1}a^{0+1+2} \equiv s^1f^0a^3 \equiv sa$ □

Note one important point. When we count the number of "cross" terms in $sfsf^{-1}s$ we find *one* case, which is *fs* (♀MB). In this example it would be enough to *count the internal changes of sex* in the formal word. This is what says my proposed Iroquois Rule of Thumb. The more complicated Dravidian Rule of Thumb is necessary to distinguish the Iroquois crossness from the Dravidian crossness, which in this example is not operative. I am not sure White's rule makes this distinction.

Observe that neither the algebraic method nor the permutation method made use of "rewriting" rules. However, both of these methods are intimately interwoven with the "rewrite rules" approach, since they were arrived at in the course of demonstrating that the "rewrite rule" method gives the same result, no matter the order in which the rules are used, or how the symbols are associated before applying the rules. The long and tedious proofs of my Propositions I and II are aimed at these simple results, and I apologize for having taking so much space with them in my paper, and also for eventual slips in the proposed proof. I hope that these remarks justify my claim that I did more than rehash the "rewrite rule" method.

The graphical method.

This is shown in Figure 2 of my paper in this issue. Along this graph, drawn with PAJEK software, follow the path indicated by a kin word (there is a univocal correspondence between such paths and words), and check the characteristic marked on the final vertex. Such graphs can be called s-graphs, by analogy with p-graphs introduced by White and Jorion, and which I use myself to represent and analyze empirical social networks in my Amazonian research.

Iroquois and Dravidian Rules of thumb

The C-A-D "rewrite rules" method, the axiomatic-algebraic method, the computational-combinatorial method and the s-graph method are four alternative, equivalent methods for checking Dravidian "affinity" (or equivalently, Dravidian "crossness"). Rules C-A-D have a cognitive, ego-oriented flavor of their own, while definitions K and Axioms DA1 and DA2 suggest a more structural, "sociocentric" view of the structure.

I will give an example of the difference between the working of the Iroquois and the Dravidian "up-and-down" rules (see Table 1), using the "rules of thumb" of my paper in this issue. In the case of zero up-and-down steps, Iroquois and Dravidian rules classify "*s*" in the same way as non-cross (Table 1, line 1). In the case of a single generation step up, followed by one step down, Dravidian and Iroquois also agree on the character "cross" which for Dravidian is equivalent to "affine" (line 2). The Iroquois "rule of thumb" says that there is *one* "s" to take out of fsf^{-1} and since 1 is odd, the expression is "cross". The Dravidian rule of thumb says that *one* transposition is needed to take "s" out of the expression, and since 1 is odd, the expression is affine. Therefore, both rules agree for n=1.

In the case of two generation steps, I give two paths. The first is ♂$ffsf^{-1}f^{-1}$ (♂FFZDD). Here, my Iroquois rule of thumb says that there is *one* "*s*" to take out of the expression, and since this is odd, the expression is cross. My Dravidian rule of thumb says that *two* transpositions are needed to take "s" out of the expression, and since this is even, the expression is non-affine. Now consider the second example of an expression going up level 2. Here, the my version of the Iroquois rule of thumb says that *two* symbols "s" must be taken out of the expression, and since 2 is even, the expression is non-cross. In this case, my Dravidian rule of thumb says that *one* transposition to the left extracts the first "*s*" out of the expression, two transpositions extract the second "s" to the left, and four transpositions extract the third "s" to the left, in a total of seven transpositions. Since 7 is odd, the expression should be marked as affine.

The difference between what the Iroquois and Dravidian rules say makes sense: for ♂FZD are opposite-sex cross-cousins and therefore "spouses" under Dravidian rules, which make *their* sons siblings and therefore *parallel* (these siblings are related as ♂FFZDD and ♀MMBSS); under Iroquois rules, opposite-sex cross-cousins will *not* marry, and their children will not be "siblings" (brothers and sisters), which mean they will be *cross.*

	Iroquois	Dravidian	Kin types
n = 0	s (non-marked)	s (non-marked)	♂s ≈♂Z, ♀s ≈ ♀B
n = 1	$fsf^{-1} \rightarrow \mathbf{x}s$ (marked as cross)	$fsf^{-1} \rightarrow as$ (marked as affine)	♂FZD, ♀MBD
n = 2	$f(fsf^{-1})f^{-1} \rightarrow \mathbf{x}s$ (marked as cross)	$f(fsf^{-1})f^{-1} \rightarrow s$ (unmarked)	♂F(♂FZD)D, ♀M(♀MBD)D
n = 2	$fs\,(fsf^{-1})f^{-1}s \rightarrow s$ (unmarked)	$fs\,(fsf^{-1})f^{-1}s \leftarrow sa$ (marked as affine)	♂FZ(♀MBS)♂D (=♂FMBD) ♀MB(♂FZD)♀S (=♀MFZS)

Table 1. Iroquois and Dravidian crossness

8. Sidedness and affinity I

I take the opportunity to try to clear up the connection between sidedness and Dravidian crossness-affinity. I start this part of my answer with the following quotation taken from Douglas White's first version of his Comment (cf. also similar remark by Lehman on the odd-even rule).

> "The network in Fig. 1 (see my Diagram 2 below) is not consistent with Barbosa de Almeida's or Trautmann's definition of sidedness because the **product** of signs in each cycle is not positive, which requires both viri- and uxori-sidedness. The number of female links (if coded negative) is positive for every cycle (thus viri-sided), which requires an **even** number of negatives. A uxori-sided network would require an **even** number of male links in every cycle."

I admit I had a hard time in trying to understand both White's definition of sidedness and his rendering of my concept of affinity. However, I am very grateful to White for providing an empirical example in the form of the beautiful graph of a marriage network. My goal now is to apply my own definition of "affinity-crossness" to this example, thus facilitating the comparison between the two concepts. White wrote:

> "Trautmann's rules regard marriage 5 (ZD as non-Dravidian when it is actually Dravidian *viri*-sided..."

Let me reconstruct the point with the help of Diagram 2, which contains a crude reconstruction of Figure 1 in White's first Comment.

Marriage 5. This marriage connects 1 and 2. There are two paths linking these nodes: the path 1-5-2 and the path 1-2. In my notation, these paths are:

♂sf^{-1} (♂ZD)

♂$f^{-1}sf$ (♂DM = ♂W)

We reduce both to the "Dravidian canonical form":

♂sf^{-1} → ♂$f^{-1}sa$ □ (Affinization rule A2. ♂ZS → ♂SA)

♂$f^{-1}sf$ → ♂sa □ (Affinization rule A3. ♂W → ♂SA, ♂AS).

The conclusion is that, both these forms being "canonical" (because irreducible by any other rules) and having an affine mark, the relation at issue is an affine one. As far as our version of Trautmann's rules is concerned, both paths agree in "sidedness": they change "sides".

Marriage 10. It corresponds to the path 3-10-13 or ♂WB♂ and also to the path 3-9-2-6-1-11-4-13, which is written in kin types as ♂ZHZHZHF♂ when we look to a marriage as a same-sex relation between affine relatives.

The first path ♂WB♂ is written in my notation as ♂f^{-1} sfs and this is by definition (K2) just ♂a, an affine relation as we expected.

Consider the second, longer path. Remember now that ♂ZH is translated as ♂$s(f^{-1}sf)$ = ♂a^{-1} by definition K3 (see Section IV). Using this, we reduce ♂ZHZHZHF to ♂$a^{-1}a^{-1}a^{-1}f$ = ♂$a^{-3}f$. The odd number of "affine" terms marks this again as a Dravidian affine relation. Axiom DA1 says that $a^{-1} = a$, and aaa=a. Therefore, we obtain finally ♂fa. Both paths agree *for males.* There is however a generational gap, and this will lead to an inconsistent "affinity" as soon as we adopt a female point of view.

Let as now check marriage 10 again, now following the links 3 to 13 directly, *starting with a female,* is ♀3-10-13♀. This should be ♀BWZ or ♀sas= ♀a which is affine as before. However, the longer path gives now consanguine result:

♀$s(f^{-1}sfs)$ $(f^{-1}sfs)(f^{-1}sf)fs$♀ → ♀$s\ a^{-1}\ a^{-1}\ (as)fs$

♀$s\ a\ a\ (as)fs$ → ♀$saaas\ sfa$ = ♀$safa$ = ♀$sfaa$ = ♀sf □ (♀F)

The inconsistence in the paths from a female point of view results from the avuncular marriage 3-10-13. Maybe this inconsistence of paths, seen from the female point of view, is what White means by his own diagnostic: "viri- but not uxori-sided".

If normal Dravidian axioms are taken as premises, then an avuncular marriage in the path above brings about a "double classification" of relatives. My point, however, is merely that I think that a behavioral-statistical approach to marriage alliance could be combined with the logic-axiomatic method, and that my calculus provides a mechanical procedure to test hypothesis of logical consistence of rules.[5]

9. Avuncular marriage

White writes that Trautmann's model decrees ♂eZD marriages as being "outside normal Dravidian rules". Let me consider again this point by means of my proposed notation. I use equalities throughout the reasoning, since I am not concerned here with the *uniqueness* of terminal expressions, but only with the equivalence classes involved.

1) ♂ sf^{-1} = ♂$f^{-1}sf$ (♂ZD ≡ ♂W). Statement of the rule.

2) ♂ $sf^{-1}s$ = ♂$f^{-1}sfs$ = ♂a (♂ZS ≡ ♂WB). Add "s" to the right side of 1.

3) ♂f^{-1} = ♂$sf^{-1}sf$ = ♂a^{-1} (♂S = ♂ZH. Add "s" to the left at 1. K4.).

Let us now assume now the Dravidian Axiom DA 1 ($a = a^{-1}$) which equates a ♂WB to a ♂ZH (♂$f^{-1}sfs$ = ♂$sf^{-1}s$), and let us equate expression (2) with expression (3):

4) ♂a^{-1} = ♂ f^{-1} = ♂$sf^{-1}s$ = ♂a.

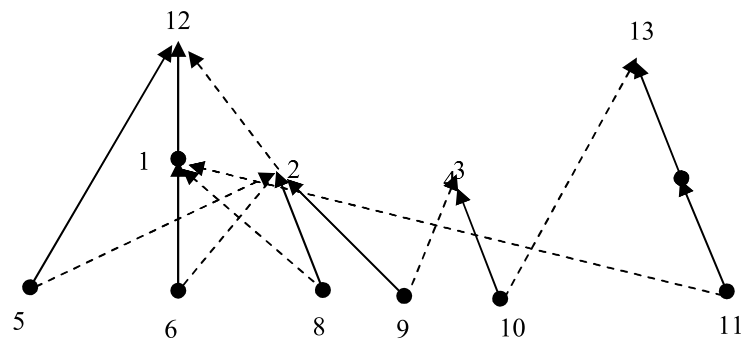
Now add f to the right of all the expressions in (4):

5) ♂fa^{-1} = ♂e = ♂$fsf^{-1}s$

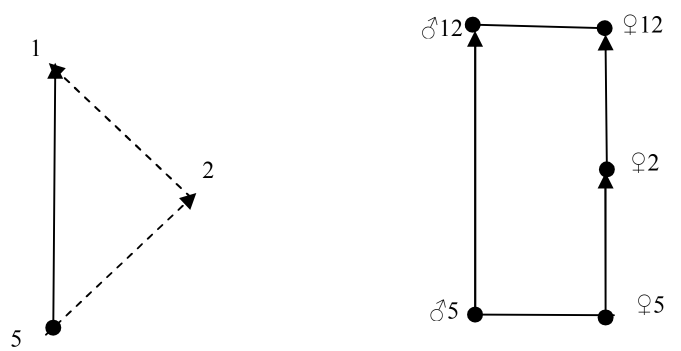
[5] I call the attention Marcio Silva's use of "upward-downward paths"/"downward-upward paths" terminology in his computational approach to affinity (Dal Poz and Silva 2009, and personal communication 2009).

Diagram 2. Sidedness and affinity

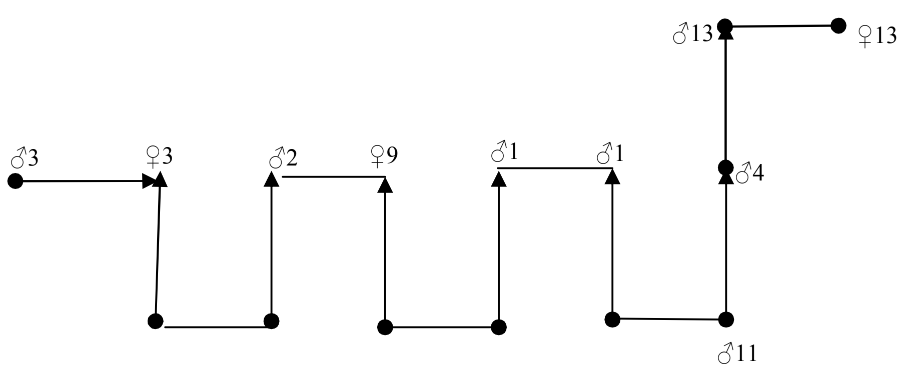
2.1. A network of connected marriages (White)



2.1. Marriage 5 in two notations



D2.3. Marriage 10 in ps-notation.



This says that my father's affine (his sister's husband) is a brother to me, and is also my patrilateral cross-cousin (I male). However, this implies that the parallel/cross divide is neutralized a consequence of assuming both marriage rule with a sister's daughter and a symmetrical rule of marriage.

Suppose a system having a rule of marriage with the patrilateral cross-cousin

1) ♂$fsf^{-1} \rightarrow$ ♂$f^{-1}sf$ ♂FZD → ♂W

Assume also a Omaha skew-rule in the following form:

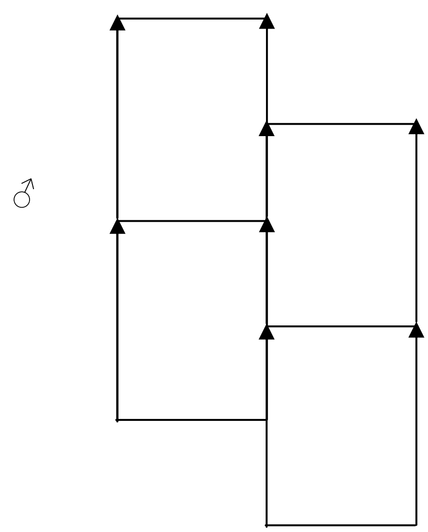
2) ♂$fsf^{-1} \rightarrow$ ♂sf^{-1} ♂FZD → ♂ZD

An immediate consequence of such a system would be a marriage rule as:

3) ♂$sf^{-1} =$ ♂$f^{-1}sf$ ♂ZD → ♂W

Such an identification has a consequence similar to that of a patrilateral cross-cousin rule in that *women are exchanged between a "viri-lineage" at alternative generations.* This seems to have been Lévi-Strauss's view on the issue, but this interpretation is contentious.

Diagram 3. Marriage with the sister's daughter.



The point of this exercise is just to illustrate that the uses of the notation and of algebraic reasoning is not limited to the axioms already introduced. I remind the reader that the formal analysis of oblique marriages was amply discussed long ago by Tjon Sie Fat, who provided a mathematically and anthropological illuminating analysis (Tjon Sie Fat 1990, Chapter 3, especially pp. 168-182).

10. Sidedness and affinity II

I would like to conclude this long answer with an attempt at comparing in a more precise way my idea of affinity and White's concept of affinity, with the goal of suggesting that they be complementary rather than mutually contradictory. I will articulate provisional new definitions not for the sake of it, but because I do not presume to have understood fully White's meaning.

Say that a viri-connected group of houses, or just a viri-group, is a set of houses (marriages) connected by viri-filiation (parent-son, represented in a graph by thick arrows) and not by uxori-filiation (parent-daughter, represented by dashed arrows). Male filiation conserves viri-groups. Viri-groups may be connected to other viri-groups by female filiation, when a woman in a viri-group marries a man in another viri-group. A uxori-connected group of houses (or just a uxori-group) is a group of houses connected only by female filiation (this may also be thought of as a uterine descent group). Female filiation conserves uxori-groups.

Let us focus the attention on viri-groups. Suppose S is a set of viri-groups. We say that a partition of S divides the set S in two *viri-moieties* called S_0 and S_1 if and only if: (1) the whole set S is connected (by male or female filiation links, so that every house may be reached from any other house by a filiation path); (2) one viri-filiation link conserves the set S_0 and conserves the set S_1; and (3) one uxori-filiation link takes one viri-moiety into the opposite viri-moiety. The sides S_0 and S_1 are said to be alliance partners. White states a proposition equivalent to the following one: an odd number of uxori-filiation links changes viri-sides, while an even number of uxori-filiation links conserves viri-sides.

Let us now make an attempt at translating the above summary into corresponding statements of the kin language K*. Consider each house now as represented by a male, and assume that ego is male. From now on, we look for kinship words that relate male a house head relates to other male house heads. From the definition of a viri-group of houses, we conclude that males in a viri-group are terminologically connected by words generated by f and f^{-1} and only by them (this includes "brother", that is classificatory contraction of ff^{1}). In a viri-group, every man is either a son, or a father, or a brother, of each other. Men from one viri-group may be terminologically connected to men of other viri-group by kinship relations involving a change of sex. These are ways for connecting men to men by a path involving sex changes and one filiation (there must be two sex changes): a descending filiation link through a woman (♂$sf^{-1}s$ or ♂ZS), or an ascending filiation link through a woman (♂sfs or ♂MB). These paths are canonically expressed as ♂$f^{-1}\mathbf{x}$ or ♂$a^{-1}f^{-1}$, and ♂fa or ♂$\mathbf{x}^{-1}$ (Definitions K1-K4, Section 4 of my paper). This means that: if a Dravidian terminological structure is at play, then *connected, distinct viri-groups are to be related by a chain of affine relationships* a^{-1} or a. Note also that, since we have not used Dravidian axioms, but only definitions K1-K4, a similar proposition can be phrased in terms of crossness: *connected, distinct viri-groups are related by one cross relationship* ($\mathbf{x}$ or $\mathbf{x}^{-1}$)

Let us now consider the definition of viri-moieties. Suppose S_0 and S_1 are a partition of a set S of viri-groups into moieties. then, the three conditions above mean that (1) all men in the whole set S are connected by words generated by f and s, (2) words having an even number of *a* terms conserve viri-moieties (the respective canonical forms have no term "*a*"), (3) words having an odd number of "*a*" terms change viri-moieties (their canonical representatives have only "*a*").

If this reasoning is correct, a Dravidian terminology is indeed consistent with the partition of houses into viri-moieties. The same reasoning can be used to make a corresponding statement for uxori-moieties. Furthermore, if the above definitions of viri-moieties and of uxori-moieties are a reasonable translation of the concept of viri-sides and uxori-sides, this suggests a close connection between a social-statistical, graph-theoretical representation of facts and a logical-axiomatic theory of a semantical field. Without loosing from sight a clear distinction between the theory of social groups (and its graph-representation) and the theory of kinship terminologies as semantic spaces, it should be possible and even fruitful to combine both approaches.

11. Concluding remarks

Ideally, says Douglas White, models of Dravidian systems should account for the logical structure of terminologies, for the actual actor's behavior, and for the actor's own statement of their rules. Such models are called by him "formal empirical models". They are goals in the important research program led by White himself. White presses his expectation that I "would modify [my] current model into a paradigmatic form that accords historically and ethnographically with Dravidian terminologies, and variants". After trying to show how I think that my basic approach can be adapted to different specifications, I welcome this advice, although I plan to apply it to the more familiar realm of Dravidianate South America Lowlands (DSAL) terminologies. However, I do not think that such a research program, in South America Lowlands at least, should be limited to adding detail to the Dravidianate model. In this case, I agree with specialists who see Iroquois, "Dravidianate" (with the added Kariera/Cashinahua cases) and Crow-Omaha "features" present in the Amazonian ethnographic landscape. Such features involve relative age, generational rules (skewing rules, alternating generation, "collapsing" or "forgetting" rules), and marriage types (symmetrical, asymmetrical/matrilateral, avuncular/patrilateral), as well as different forms of crossness and affinity (including Viveiros de Castro's theory that includes a "metrical" view of the classification process that takes into account the genealogical distance).

Underlying much of the critical remarks, there is general view that models should reproduce behavior or statistical phenomena, and that cognitive structures should be derived from them. This is how I interpret the "empirical formalism" advocated by White. I do not see my position as formalistic, but I understand that my style in the my paper in this issue may have caused a confusion between a concern with stating general propositions and proving them (which I assumed would be part of the business of "mathematical anthropology") and "formalism". I do not see the analysis of theories (e.g. my analysis of Trautmann's model) as a reduction to "formal languages". My view of the axioms that are part of the theory T in the sense explained above is that they should provide a description of "structures" that play the role of models for the theory (which I call "ontologies" in this context), by saying "what there is" at the theory level (van Fraassen 1980, Da Costa and

French 2003; Barbosa de Almeida 1993). The fact that different axioms describe a family of alternative structures allows not only the testing of a specific model (e.g. the "Dravidian" model of Trautmann, or "Crow-Omaha" model of Lounsbury), but also to conjecture many other possible structures.

K. E. Lehman wrote long ago the following passage:

> "A competence model or theory is a characterization of what people know about their culture that enables them to assign a categorical interpretation, a meaning, and such values as appropriateness to actual behavior and the world of things in an indefinite number of situations – a system of indefinitely productive well-formedness conventions imposed upon "reality". A performance model or theory is intended to directly characterize behavior and its products, and perhaps the mechanisms producing or accounting for the behavior." (Lehman 1974:xii).

Lehman added that competence models should not be understood in the sense of "systems of categorical rules" intended to characterize "what someone needs to know in order to *behave* according to the rules of his society" (Lehman 1974: xiii). I interpret Lehman's stricture in the sense that mathematical models must not be taken as models of inner psychic reality, but as methods for generating previsions about behavior.

Even if we assume this position – which express Lehman's departure from the semantical-structural position --, there is the issue of how to connect *behavioral-statistical* models with *logical-mathematical models* which deal with *rules for behavior*. Lévi-Strauss wrote in 1967, in answer to critics that charged his models of lack of agreement with actual behavior, that "logical consistence" is a property of models, and should not to be mistaken with behavioral consistence. More to the point, he proposed that the link between "rules" and "behavior" may be open to a "degree of indeterminacy" (Lévi-Strauss 1967: XXI). Rules associated with inconsistent behavior might be described by a fuzzy-rules model. I interpret, perhaps abusively, the impressive statistics quoted by Douglas White as suggesting the possibility of such an approach, to which I also relate the statistical-computational approach of Marcio Silva (Dal Poz and Silva 2009; Silva personal communication). In any case, I believe that the axiomatic approach may contribute to the empirical-formal research program by splitting complex models into a set of simple, features, expressed by separate and independent axioms and their variants (this is a point also made by Trautmann).

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