HEADY'S COMMENT ON D. READ
"GENERATIVE CROW-OMAHA TERMINOLOGIES"

PATRICK HEADY
MAX PLANK INSTITUTE FOR SOCIAL ANTHROPOLOGY
HEADY@ETH.MPG.DE

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Read's work on the generative logic of kinship terminologies constitutes one of the most distinctive and stimulating series of publications in the contemporary anthropology of kinship. His intention to produce a universally valid explanatory (i.e. causal) theory of kinship terminology is highly ambitious – but also appropriate and intellectually refreshing. An important feature of his theoretical framework is that it allows for an interaction between universal cognitive processes and local cultural ideas. Another distinctive feature is that Read usually models whole terminologies – and specific features, such as crossness and generational skewing, are understood in the light of the terminological system as a whole. Read has been continually testing and refining his conceptual apparatus, and in this paper he brings it to bear for the first time on Crow-Omaha systems – offering us an exploratory case study that is intended both to show the insight that the generative logic approach can bring, and to investigate the specific logical features that may give rise to the phenomenon of skewing.

Before starting to write this comment, I had a chance to look at the comments by McConvell, and by Trautmann and Whiteley. The discussion seems to be broadening out beyond the specifics of Read's treatment of Omaha terminologies , to include more general comments on his generative logic approach, the various ways in which the distribution of terminology types can be explained, and the kinds of evidence that should be used to assess these explanations. In this comment I will also look at Read's approach in quite a general way, embedding my specific comments on his analysis of Thonga kinship within this more general review.

In Read's system (see pages 11 to 13), any kinship terminology is a layered structure. At its base are the primary kin terms used to refer to fellow members of the family space. These terms, or a subset of them, form a generative set, whose members can be combined in relative products to define secondary kin terms, and then applied again to those secondary terms, to define tertiary terms and so on. The process is constrained by certain formal properties, including reciprocal reference, and by the need to follow a certain order in the generation process (generating ascending terms first, then descending terms, then allowing for gender differences, and finally extending the system to include affinal terms).

If I have understood the argument so far, this generative apparatus is not itself cultural, but part of the deep cognitive equipment that is shared by all human beings. However, the terminologies which it generates *are* cultural systems, because culture affects the choices made at two or three key points in the generation process. To start with, the definition of the generating set of primary kin terms expresses cultural emphases on the relationships concerned. The same is true for the

relative product equations which define the relationships expressed by second- and higher-order kin terms. Finally, the kinship terms can be modified to fit other aspects of the culture which were not incorporated in the generative process.

Evaluating Read's system using aggregate data

Though Read develops and tests his approach by making detailed analyses of particular terminologies, the system has some general implications which can be tested on more limited data aggregated from a wide sample of societies. Since the full terminologies are derived from generating sets which refer to relationships within the conjugal family space, there should be some correlation between the form of these primary kinship terms, and that of the higher order terms which are generated from them. The correlation need not be very strong – since the structure of the higher-order terms also depends on the set of relative products used to define them – but it should be there.

Now, sibling terms are probably the most variable category of terms for relationships within the conjugal family space – which means that the differences between sibling terminologies should capture much of the variation between Read's basic generating sets. So, if his model is right, one would expect to find a correlation between the forms of sibling terminologies, and the structure of various higher-order terminological categories including, for instance, cousin terminologies. And this is something we can test. Indeed it has already been tested – by Murdock (1968) in an article about the forms and distribution of sibling terminologies, derived from data on the societies in the Ethnographic Atlas. The data on the combined pattern of sibling and cousin terms was set out in Table 4 of Murdock's article.

Murdock himself concluded that the two sets of terms were almost independent of each other. However, his conclusion was premature. The table in this comment is derived from the data in Murdock's own table, but I have rearranged it to make it easier to identify the structure. To start with I have added a column at the left of the table for data on societies with "descriptive" cousin terms, which Murdock provided in his text but excluded from the table. Secondly, I have treated the sibling-terminology-types as the basic data (with the total for each type in the right hand column), and presented the figures for each combination of sibling and cousin terminology as a percentage of the total number of societies with the sibling terminology concerned. Finally I have changed the order of the sibling terminology rows – placing those with higher percentages of Descriptive or Eskimo cousin terms near the top of the table, and those with a greater proportion of Hawaiian or Iroquois terminologies lower down.

In order to appreciate the picture that emerges, it is probably best to start by grouping the cousin terms into three main categories Descriptive+Eskimo; Hawaiian+Iroquois; and Omaha+Crow+Anomalous. It can be seen that "absolute sex" sibling terms (e.g. brother and sister), are most conducive to Descriptive and Eskimo terminologies, whereas the main classificatory cousin terminologies – Hawaiian and Iroquois – are associated with sibling terms that make relative distinctions. They are particularly frequent with sibling terminologies with

relative age distinctions for both brothers and sisters ("relative age, sex"), and "relative sex" terminologies which distinguish between same and opposite sex siblings but do not rank siblings by age.

Table. The distribution of cousin terminologies for each kind of sibling terminology identified by Murdock (1968) – note: each row adds to 100%

		COUSIN TERMINOLOGIES:							
		Descriptive	Eskimo	Hawaiian	Iroquois	Omaha	Crow	Anomal-ous	TOTAL = 100%
TYPES OF SIBLING TERMIN -OLOGY	absolute sex	34	24	17	11	8	5	2	126 =100%
	one sibling term	16	8	39	18	11	6	2	62 =100%
	relative age	0	17	49	17	10	5	1	81 =100%
	relative age, older sex	0	14	30	23	23	4	6	70 =100%
	Siouan: all distinctions	0	11	31	33	17	6	3	36 =100%
	relative age, sex	4	8	38	37	7	5	1	159 =100%
	relative sex	0	5	36	37	5	13	3	190 =100%
	OVERALL	**8**	**12**	**34**	**27**	**9**	**7**	**2**	**724 =100%**

Murdock identifies four more kinds of sibling terminology – though all of them are less frequent than the big three just mentioned. Terminologies which distinguish relative age, but without taking account of gender, resemble the two relative terminologies just mentioned in generating a high proportion of Hawaiian or Iroquois cousin terms– with a particular emphasis on Hawaiian. Terminologies with a single undifferentiated sibling term generate proportions of cousin terminologies that come mid-way between those generated by the relative age and absolute sex sibling systems.

The remaining two sibling systems are the "relative age, older sex" system, which differentiates male and females in the case of ascending siblings, but lumps the descending siblings together in a single category; and the Siouan-style system which has relative distinctions for both age and sex, as well as absolute sex distinctions. An interesting feature of these two sibling systems is that they generate the highest proportions of Omaha cousin terminologies. Crow terminologies, on the other hand, are most strongly associated with relative sex sibling terms – and are less frequent than Omaha terminologies in all other sibling systems.

These findings deserve to be discussed in more detail than is possible here. But even this initial look is enough to confirm the essential prediction from Read's model: namely that the pattern of terms within the basic generating set is associated with the structure of the higher-order derived

terms. Of course, the mere fact of correlation does not establish the direction of causation – but Murdock provides additional information which suggests that sibling terminology is the independent variable. In the article just cited (Murdock 1968, pp 4-7) he writes that it is typical for a majority of societies within the same geographic or linguistic cluster to share a common pattern of sibling terms. This contrasts with the tendency of cousin terminologies to vary considerably even within the same culture area (Murdock 1949 pp 192-196). The greater geographic stability of sibling terms is what one would expect if they formed part of the generative set – since, in Read's system, the generative set provides a fixed starting point from which each society can construct its own system of higher-order terms – allowing the higher order terms to vary between neighbouring societies despite being anchored in a common generative base.

Formulating and testing logical connections between terminological features

Having established the overall compatibility between Read's generative logic approach and the data in the Table, I would like to draw on that data to comment on some of the more specific logical relationships which Read suggests.

In his opening and concluding discussions Read makes a distinction between features of a terminology which are generated by its fundamental cultural logic and those which derive from external factors concerned with various aspects of social organisation. The way this distinction is formulated is a bit perplexing, since Read's description of his own system makes it clear that, while the central features of any particular terminology (the definition of the generative set, and the most basic relative-product equations) will derive from cultural choices, the underlying cognitive logic is universal. As the defining cultural choices are presumably linked to social organisation in a fairly fundamental way, the contrast – generative cultural logic <u>versus</u> social organisation – doesn't really make sense.

However, the distinction can be reformulated in a way that expresses a real difference, and is consistent with the way Read discusses specific examples. The reformulated distinction relates to the way in which the primary kin terms (embodied in the choice of generating set and the most basic relative-product equations) relate to the secondary and higher-order terms which derive from them. All higher order terms can be derived from primary terms by relative-product equations. In some instances that is all there is to it: the terms are defined purely by specific equations which have to be learned on a term-by-term basis. We could describe terms of this kind as 'loosely determined' by the primary kin-term set. However, other terms are more tightly constrained by the formal features of the generative process itself – including the sequence of generative steps and the requirements of reciprocal reference. Features of the higher-order terminology which are derived in this way could be described as 'strictly determined' by the primary kin term set.

An example of 'strict determination' concerns the ways in which the underlying generative logic can give rise to classificatory terminologies. On pages 13 and 14 Read refers to earlier papers in

which he derives the basic classificatory property (that the term for FB = "F"; and hence "FBS" = "B") from the existence of a structure of ascending and descending sibling terms (which Murdock would refer to as 'relative age' terms) . The Table above confirms that classificatory terminologies are indeed associated with 'relative' sibling terminologies – but, as we have seen, they are not limited to terminologies with a relative age distinction. Classificatory cousin terminologies are also strongly associated with sibling terms which are based on a relative sex distinction with no reference to relative seniority. (In the simplest examples of this pattern there are just two terms, one meaning 'same sex as Ego', and the other meaning 'opposite sex to Ego'). It would be interesting to know whether Read's argument concerning ascending and descending sibling terms could be re-formulated in a way that covered all kinds of relative sibling terms. For what it is worth, my guess is that it could.

A central concern of Read's paper is whether Omaha skewing can also be strictly determined by the generative logic embodied in the primary kin-term set. The data in the Table is against it – since Omaha skewing is only found in a minority of the societies in any row, regardless of the type of sibling terminology concerned. Read himself regards Omaha skewing as a superficial feature of the Fox terminology, but argues that it is an intrinsic consequence of the generative logic of the Thonga terminology. I think this argument is mistaken.

Read's analysis of the Thonga terminology

Read sums up the argument on page 23:
> "What has been shown here is that when we view the Thonga terminology from the perspective of kin term logic rather than the logic of genealogy …, we find that the so-called skewing arises for a simple reason, namely only the male-marked terms arise through a generative logic that begins with male self, *tatana* ('father') and *nhondjwa* ('ascending brother') as primary kin terms, whereas, in an asymmetric manner the only generating term for the female marked terms is '[female] self'. This is the logic of a terminology which structurally only recognises patrilines."

This generative process is described on pages 14-to 18, and illustrated in three figures, whose captions are as follows:
> **Figure 3:** Left side: Structure of male marked terms generated through the male marked generators, male self, *tatana* ('father'), and *nhondjwa* ('older/ascending brother'). Right side: the only generating term is female self, so no structure of female marked terms is generated.
>
> **Figure 4:** Female marked terms generated through kin term product of [female] self with male marked terms.
>
> **Figure 5:** Replacement of a male marked and a female marked term with a neutral term when both a male and a female marked arrow points to or from the sex marked terms….

But when we look at these figures more closely, they are distinctly odd. A check with Table 1 shows that of the six Thonga terms in Figure 3, only one – *tatana* – is specifically male. In Figures 4 and 5, we see that in most instances the distinct male and female marked terms that are first created and then merged are in fact one and the same word – as can be seen by tracking what happens to the male, female and neutral versions of *ñwana* (child), as explained in the paragraph at the top of page 17. In fact throughout this part of the generative process, the only positions which are indexed by distinct male and female terms are those in the first ascending generation.

On the face of it, it would have been much simpler to treat the gender neutral terms – or rather the positions which they index – as basic, and then focus on explaining the existence of distinct male and female terms in the first ascending generation. At first I was puzzled as to why Read had chosen to depict a more complicated process. However, I think the answer is given on page 13 where, in his general account of the generative process, he describes the procedure by which the sex marking of kin terms is introduced. The procedure involves the generation of a single structure of ascending and descending terms, then producing a second isomorphic structure, labelling one of these structures as male and the other as female, and finally connecting them to form an overall structure. In the analysis depicted in Figures 3 to 5, Read has carried through this programme. But, since the result has led to a single term in four of the five generations concerned, he has decided that only one of the gendered structures has been filled by an original set of terms, which has then been copied onto the other. For reasons that are not entirely clear to me (but which may have to do with the additional features of terminology and usage described on pages 18 to 22) he decides that the terms were originally generated in the male structure, and that the female versions are merely a copy.

However, all this depends on the assumption that the generative sequence set out on page 13 is in fact universal. An alternative reading of the facts considered up to now would be that this aspect of Read's current model does not fit all societies, and needs to be revised.

Another problem with Read's argument is that, untypically, his analysis leaves out several of the terms listed in Table 1. The terms concerned are *malume, mukonwana, nwingi, namui, nhombe, namu, nuna, nkata.* Apart from the first, these are all affinal terms – and it is also worth noting that Read ignores the possible affinal significance (as WZH) of the three sibling terms included in the analysis discussed above. Given the prominence of affinal relationships in theoretical discussions of Crow-Omaha systems – and Read's own contention that *mamana* 's place in the generative framework is as a secondary affinal term (FW) rather than as a primary generative term in its own right (M) – this inattention to the affinal side of Thonga terminology is rather surprising.

Here again, page 13's account of the generating process suggests a possible reason. Read's comment on the procedure that generates affinal terms is as follows.

> "The next layer is composed of affinal terms. This may involve introducing affinal generating terms, depending on the terminology".

In comparison with the rest of Read's discussion of generative logic, this is very brief, and suggests that this part of the cognitive model may need further development.

Since there are question marks over the analyses of both gender marking and affinal terms, it is not really possible to assess the claim that the elements of skewing in the Thonga terminology are determined by its underlying cultural logic. At the same time, I hope it is clear that these objections relate to this particular application of Read's analytic framework – and to some aspects of the framework which may need to be reformulated – but not to the framework as such.

Formulating and testing associations between terminology and social practice

This brings us to the question of how the existence and distribution of Omaha terminologies can be explained – and of what kinds of evidence should be used to evaluate the explanations. Trautmann and Whiteley end their comment with some advice about how the Thonga terminology (or presumably any other) could best be incorporated into the theoretical discussion.
> "First, base it upon the best data….Second compare it to neighbour systems. Only after, engage in comparisons with other world regions."

McConvell makes a similar point when he advocates paying attention to the historical reconstruction of developmental processes within language groups.

Thinking back to Murdock's data, we can see why this advice is likely to be good – but also why Read might not want to follow it too closely. It is good because focusing on a particular region or language group holds many of the potential explanatory variables more or less constant, including – as Murdock states – the main features of the sibling terminology. But sibling terms are, in Read's framework, a crucial part of the primary generating set. So, if he wants to test whether secondary features – such as the presence of generational skewing – are strictly determined by the structure of the primary generating set, he is also obliged to compare across different regions and language groups. In doing so he would be following the example of several of the great names mentioned in this discussion – including Lounsbury, Leach, Lévi-Strauss, and even Morgan himself – none of whom produced an exhaustive analysis of locally contiguous systems before launching on wider comparisons.

Trautmann and Whiteley make a plea for another kind of evidence – the thick description of ritual, ideas and practice – which can provide a different but complementary kind of understanding to that which emanates from the mathematical analysis of kinship terms. A fuller description, they argue at one point, is equivalent to a better explanation. They seem puzzled that neither the mathematicians nor the "cultures-of-relatedness" people find each others' analyses particularly illuminating. I agree with Trautmann and Whiteley about the explanatory value (in the strict scientific sense) that can be obtained by linking analyses of kinship terms to detailed data on ritual and kinship practice – but there is a difficulty. The thick descriptive data needed for this kind of analysis is easiest to produce in studies of particular societies – but in order to verify the causal relationships that such studies suggest, the comparisons of terminology and practice need to extend beyond individual societies. To make these wider comparisons fruitful –

and interesting from both the mathematical and cultures-of-relatedness viewpoints – we need to construct theories that explain in principle the ways in which terminological patterns and patterns of practice might map onto each other – and I don't think we yet have many theories of this kind. (For more on this point, and some very preliminary ideas about how such a theory might be built, see Heady 2014 and 2017).

In conclusion, the range and richness of the issues raised by the various discussants is a tribute to the originality and scope of Read's theoretical work.

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