

## KINSHIP AS CLASSIFICATION: TOWARDS A PARADIGM OF CHANGE

ROBERT PARKIN

### I

The aim of this article is to consolidate accrued knowledge and understanding of the ways in which patterns of kinship terminologies may change over time. Study of this topic, as of terminological patterns generally, goes right back to Lewis Henry Morgan's pioneering work in the middle of the nineteenth century, and we now have sufficient data and understanding of the issues involved to attempt grander paradigms than have been possible in the past. I wish to broach this issue here as a first step in this direction, while also acknowledging some of the problems involved. The discussion will inevitably be quite technical and specialized and will assume that the reader already has a degree of knowledge and understanding about these issues. First, however, some preliminaries are in order.

The study of transformations of kinship terminologies clearly invites an evolutionary approach, and Morgan himself counts as one of the great evolutionists of the nineteenth century. Typical of his time, he sought to link such changes to changes in social organization more generally, as well as putative stages in history and prehistory. Although he was less ethnocentric about the superior qualities of the Victorian period than some of his contemporaries, his writings in this respect do consider the question of the emergence of 'civilization' from 'savagery' via 'barbarism' in a manner that would be quite unacceptable in contemporary anthropology. Nevertheless, if more modestly, quite a number of anthropologists since his time have been prepared to identify changes in terminological pattern and to link them, where ethnographically justified, to changes in marriage patterns, given especially the association of various types of cross-cousin marriage with specific terminological patterns—i.e. if one is known to have changed, it is reasonable to investigate change in the other. An important example that is still relatively recent is the collection *Transformations of kinship* (Godelier et al. 1998, based on a 1993 Paris conference), contributors to which pursue such issues without falling into the ethnocentrism embedded in terms like 'civilization'. Although the Boasians were firmly opposed to Morgan's evolutionism in particular on quasi-moral grounds, this collection and other more recent work does prove that it is possible to consider such changes in a non-ethnocentric manner. Nonetheless, to play it safe, there is now an increasing tendency to talk about 'transformation' or simply 'change' rather than 'evolution', as exemplified by the title of the

volume just cited. For Allen, adopting a phrase often used by Marcel Mauss, the phrase ‘world history’ is another suitable and legitimate alternative (e.g. 1998: 318).

Coming between Morgan and Godelier et al., a number of other anthropologists have made their mark on such work (see Parkin 1997: Ch. 14 for an overview). Early figures include Irving Hallowell, Fred Eggan and Alexander Spoehr on Native American terminologies (see Parkin 1997: 162-3), while both W.R.R. Rivers, a diffusionist, and Leslie White, a neo-evolutionist, defended Morgan’s general approach against his Boasian critics during the first half of the twentieth century without contributing significantly to such work themselves. Later figures include Elman Service (e.g. 1971: Ch. 7), Gertrude Dole, whose thesis on terminological change (1957) has never been published, though some articles have been (e.g. 1960, 1969, 1972), Elmendorf (1961), and Dyen and Aberle (1974). At Oxford, many of Rodney Needham’s basically structuralist studies of variations in kinship terminology used the idea of change as a mode of explanation for variation in terminological patterns (e.g. 1980), an approach which inspired both my own work and that of my doctoral supervisor, N.J. Allen, who had himself been taught by Needham initially. Allen’s notion of tetradic society (1986; also 1989a, 1989b, 1998), an extrapolation back in evolutionary terms from four-section systems with bilateral cross-cousin marriage like the Kariera of Australia, has caught the attention of other scholars and has been the partial inspiration behind two recent volumes, *Transformations of kinship* (Godelier et al. 1998, already cited), and *Early human kinship* (Allen et al. 2008), the latter being explicitly evolutionary in the contemporary sense (the volume includes contributions from social anthropology, archaeology, evolutionary psychology, primatology and linguistics).

Not all those associated with Needham’s influence have been equally taken by his ideas in this respect, one notable sceptic being R.H. Barnes (2012), who has criticized Robert Blust’s work (1980) reconstructing early Austronesian kinship and more recently suggested bluntly that ‘the history of evolutionary speculation in anthropology has never produced anything like certainty in our understanding of how and why such patterns change’ (ibid.: 203). However, some of Barnes’s own early work (e.g. 1977: 151-2), together with that of Gregory Forth (1985, 1988, 1990), David Hicks (especially 1981, 1983, 1985, 1986) and C.H. Wheeler (1982) (the first two were students of Needham’s, the last briefly of Barnes), did point in the same direction, though in Barnes’s case already with evidence of the scepticism he was to express more strongly later (e.g. ibid.: 152-3). One figure of note in Allen et al. 2008 and other much more recent publications is Christopher Ehret, who has worked on developmental problems in Bantu kinship in Africa. The work of the late Per Hage is also

significant, especially in his often collaborative attempts to construct paradigms relating to the whole of human history under the umbrella of terms such as ‘Nostratic’ (see his memorial volume, Jones and Milicic 2010).

In addition to Needham and Allen, an important inspiration for my own work in this area has been Stanley Witowski, who applied the methods of Berlin and Kay (1969) on the study of colour terms to kin terms (Witowski’s thesis is unpublished, but see 1972). Berlin and Kay compared colour terms and terminologies from a wide range of societies around the world and postulated their emergence as an evolutionary sequence that was, at least in part, predictable. Thus terms for colours like yellow, green and blue typically emerged after that for red, meaning that if a terminology has a term for one of these colours it will have one for red, but not necessarily vice versa; similarly, if blue has a separate term, yellow and green will also have one each, but not necessarily vice versa, as blue typically emerges later as a separate category in the evolutionary sequence. Witowski showed that the same basic principle of predictability applied to kinship terminologies: for example, the presence of cognate-affine equations based on the practice of bilateral cross-cousin marriage implies the presence of equations between cognates of the type  $FB=MZH$  and  $MB=FZH$  but not necessarily vice versa. However, this is not because cognate-affine equations emerge later but because they break down earlier, possibly but not necessarily because cross-cousin marriage is no longer being followed. Nonetheless this is no objection to Witowski’s adoption of Berlin and Kay’s basic method, which is implicit in the core analysis of the present article, as it indicates that changes to kinship terminology are more likely to take place in one direction than its opposite. Similarly, for example, it is evident that the existence of bifurcate merging or bifurcate collateral equations in generations 0 and/or -1 normally implies the same in +1 but not necessarily vice versa, reflecting a general trend to the Hawaiianization of these genealogical levels (typically 0 before -1, then +1, if these adjacent levels change at all). The main point is not that changes in terminologies are predictable in a deterministic way, but that the parameters within which such changes take place are limited and can be known, occasional ethnographic exceptions notwithstanding. The method is a predictor of trends, not of pre-determined stages or changes.

This approach to the predictability of change can also be invoked in respect of other types of equation and the order in which they typically disappear from kinship terminologies, namely: first alternate generation equations (e.g.  $eB = FF$ ,  $yB = SS$ ); secondly prescriptive equations expressing cross-cousin marriage (i.e. equating cross kin and affines); and thirdly classificatory equations of the type  $F = FB = FFBS = FFFBSS$  etc., which rely on the

principle of same-sex sibling equivalence. Alternate generation equations are central to tetradic theory (mentioned above and explained in more detail in sect. II, below), though they also appear frequently in actual terminologies of bilateral cross-cousin marriage from which tetradic society has been extrapolated backwards (e.g. the Kariera of Australia). The dissolution of prescriptive equations can normally be associated with the decline or abolition of cross-cousin marriage, which such dissolution nonetheless tends to lag behind. The fact that the characteristic ‘prescriptive’ equations between cross kin frequently survive the breach in cognate-affine equations can be related back to Witowski’s observation that the existence of the latter implies the existence of the former but not necessarily vice versa. Classificatory equations can also survive this abandonment of cross-cousin marriage, but not further changes in the direction of generational, zero equation or cognatic terminologies (stages 6, 8 and 9 in the list below). They too are implied by the existence of either or both prescriptive and alternate-generation equations but not necessarily vice versa.

The question of the evidence for such changes should also be addressed, as this raises problems in its own right. Terminologies can be found that are internally logically consistent in their patterning throughout, for example, the Tamil terminologies linked to bilateral cross-cousin marriage recorded by Good (1981: 56), or the highly symmetrical English terminology. That is, one can use logical *consistency* to establish logical terminological types that may correspond to ethnographic realities, but do so rather infrequently. However, a great number of recorded terminologies lack this consistency, which itself has often suggested a terminology in flux and therefore a situation of change (e.g. Allen 1975, 1976; also Parkin 1992: Ch. 7)—that is, logical *inconsistency* becomes a proxy for the study of terminological transformation. Because of these considerations, Needham called for the abandonment of types of whole terminologies and advocated instead a concentration of the features, very often mixed, whereby terminologies might individually be described and characterized (especially 1971). However, while I sympathize with this view as regards analyses of actual terminologies, here I am more concerned with general patterns in transformations. For this reason the necessity is to concentrate on uniform and logically consistent types, not individual and possibly internally consistent actual cases, nor internally inconsistent cases. Nonetheless one must always remember that it is one thing to set out logical transformations step by step, quite another to prove the actual changes they imply historically. Another possible objection is that what appears to be inconsistent actually reflects kinship practice in the society concerned, that is, has meaning in synchronic terms. This latter tension is exemplified by an interesting contrast between the approaches of Gertrude Dole, who put

forward an evolutionary explanation for inconsistencies in the pattern of the Xingu Carib terminology (1969), and Ellen Basso, who rejected this in favour of an explanation couched in terms of actual kinship practice in the present (1970). In essence, though, one can argue that, if cross-cousin marriage has ceased to exist in a society that very probably had it historically, it has also ceased to have present-day meaning. In other words, there is no necessary incompatibility between the two positions: changes in kinship systems take place because circumstances change, along with the meanings attached to them. The fact that the Xingu Carib have a meaningful kinship system and terminology in the present does not mean that they have not changed in the past.

Kryukov discusses possible sources of evidence for change in terminologies (1998: 298-9), namely historical records (e.g. for Chinese), old ethnographic and similar reports compared to more recent ones, myths etc. as presenting older forms, emerging distinctions between the address and reference terminologies in which the former may point to future developments, and using older informants. Other methods identified by Forth (1990) include the comparison of different terms in cognate languages, as well as whole terminologies in cognate languages (cf. Parkin 1992: Ch. 7 on Munda), and redundancy, i.e. where a kin type is covered by more than one term, which itself may be evidence of change, being an instance of the lack of consistency mentioned above (e.g. the examples in Parkin 1998: 264). Kryukov (*ibid.*) also discusses the ways in which terminological change may be effected: in particular, derivations of other terms, periphrastic phrases involving other terms or loans from foreign languages may all be used to introduce fresh distinctions (1998: 300-1). Forth offers a similar list of possible modifications in linguistic terms (1990: 376 n.7): borrowings, new coinings, compounding, and vertical or horizontal extensions of particular terms (i.e. respectively to other generations and, e.g., of terms for lineal kin to cover also cross-cousins).

A related issue is the direction of change and evidence for it. The dominant assumption in the literature is that terminologies expressing bilateral cross-cousin marriage have an evolutionary priority among attested types, i.e. they are the starting point for any paradigm of transformation or evolution, and that all changes away from it are irreversible. While there is some historical evidence for this assumption, it is not accepted by everybody. For example, in relation to parts of eastern Indonesia, where there is ample evidence of change in both terminology and affinal alliance, both Fox (1984, 1987) and Guermonprez (1998) have suggested cognatic terminologies as the starting point, while Blust (1980) has claimed that the Proto-Austronesian terminology was asymmetric prescriptive in type. Fundamentally the theory of an original system of bilateral cross-cousin marriage is still a hypothesis profoundly

influencing the direction of work in this area. Hypothesis-driven research is, of course, perfectly respectable, but we now need to bring together the evidence that supports the hypotheses informing research on terminological change. This is one aim of my longer-term research, though I can do little to take this forward in the present article. Nonetheless, there is a general though not universal assumption that simplicity of structure is made more complex rather than that complexity is simplified: whereas the former may but need not involve the creation of terms, or alternatively their re-sorting, the latter would involve their deletion, this being less likely. In fact deletions certainly occur, but I would argue that they do so at the end of the world-historical process of change, and (in line with Allen, e.g. 2008) would agree that they do not do so at its beginning. The view that deletions of terms are unlikely is also found in Elmendorf (1961: 370-1, 376).

Fundamentally, therefore, I adopt the by now conventional view that symmetric prescriptive terminologies expressing the practice of bilateral cross-cousin marriage are basic among attested forms and that they have changed into other forms, not vice versa. In cases where this seems to be contradicted, i.e. that a speech community appears to have adopted such a terminology from some other type, the explanation is usually that it is the language of the speech community that has changed, not the pattern of the terminology. Thus, in India, there are a number of Indo-European languages with symmetric prescriptive terminologies, which are not generally known in this language family. One of the best known is Sinhalese, whose speech community has probably long been practising cross-cousin marriage, as does most of south India. The assumption here is that at some stage of history the Sinhalese adopted an Indo-European language in place of, probably, a Dravidian one, but retained the old terminological pattern, as also the associated marriage practice, in doing so.

Finally here, we must remember that not all research on kinship terminologies is evolutionary or transformational. This tension has already been mentioned in relation to the differences between Dole and Basso mentioned above. For one thing, there are countless synchronic analyses of particular terminologies in particular ethnographic situations, only a minority of which consider possible questions of change, even in passing. Secondly, there are comparative analytical approaches that, implicitly or explicitly, are synchronic in type. Historically, two in particular stand out, neither exclusively being used for kin terms, though most of the work in both cases has been in this area. One is componential analysis, pioneered by Ward Goodenough (e.g. 1956) and inspired by a combination of Bloomfieldian linguistics and Kroeber's features (1909), like side of family, age and generation—and indeed, more recently the term 'feature analysis' has been suggested for this approach (D'Andrade 1995:

21). The other approach, Chomskyan rather than Bloomfieldian in inspiration, is formal semantic analysis associated primarily with the psychological linguist Floyd Lounsbury and the anthropologist Harold Scheffler, whose work on Sirionó kinship (1971) is typical of the method, as well as having been devised as a foil to Needham's structural analysis of the same system (1961). Neither componential analysis nor formal semantic analysis is an inspiration for myself, largely because they both involve synchronic analyses that end with what is essentially the resetting of genealogical formulae in a different mode. In addition, Kroeber's and Goodenough's features are incidental to the sort of analyses I and my mentors have pursued, while the Lounsbury–Scheffler approach underplays the aspect of cross-cousin marriage that is the one area where terminology and social practice are clearly linked; in particular, Scheffler denies that Dravidian terminologies have anything essential to do with cross-cousin marriage. In fact cross-cousin marriage cannot be ignored in work on terminological change generally, even though it is not relevant to all actual cases, as a lot of such change seems to be associated with changes in the attitudes towards this form of marriage, causing its modification or abandonment (see Parkin 1997: 53-6 for an anonymized account of these two approaches). Dwight Read's work (e.g. 2001) appears to continue in the same synchronic vein, except that, unlike Scheffler and Lounsbury, whose work he criticizes for its circularity of reasoning, he is concerned to find a way of representing kin ties that does not show them as genealogical by invoking what in effect appear to be categories. While I am sceptical whether this is possible, because kin ties between ego and alter by definition involve a link which is stepwise, like genealogical connection, I am not sufficiently familiar with his work to comment on it further here.

## II

In setting out the following transformational trajectories, I use conventional genealogical notation, as in Parkin 1997: 9, List I. Note particularly that PosGC = cross cousins, PssGC = parallel cousins. Reference can also be made to the diagrams in that book.

I also associate different kin terms with distinct categories, one term per category, and further distinguish the category–term, which is emic or ethnographically specific, from the notion of kin type, which is etic or analytical and comparative. For example, the English term 'uncle' defines a category which consists of two kin types, FB and MB, and often two others, FZH and MZH. The Tamil term *mama*, by contrast, unites the kin type MB only with FZH of the above, but additionally covers WF and HF. In Tamil the kin type FB has a separate term

(or rather, two terms, split by age relative to ego), since to equate FB and MB would be a category mistake here with adverse moral implications. The equation  $MB=FZH=HF=WF$  is a diagnostic of a two-line prescriptive terminology associated with bilateral cross-cousin marriage; it is an equation absent from the English terminology.

Finally, I generally use Needham's paradigm of labels for terminological types or features, as set out in anonymized form in my introductory book (1997: 72ff., starting with the last paragraph on p. 72). In particular, my use of 'lineal' follows Needham's usage, not Lowie's earlier usage, which is equivalent to Needham's 'cognatic' (e.g. the English terminology); for Needham and myself, 'lineal' indicates the sorting of category-terms into descent lines, as generally in symmetric prescriptive or what Needham calls 'two-line prescriptive' (below TLP) and its immediate derivatives, three-line (asymmetric alliance) and four-line (symmetric prescriptive specifying marriage to second cross cousins). Here I am ignoring claims sometimes made in the past that, for example, six-, eight- or even ten-line terminologies occur. Usually these can be reinterpreted as two- or four-line (cf. McKnight 1971 on Connell's false analysis of the Wikmungkan terminology in Australia), which, together with three-line, are the only numbers of lines I am recognizing below. In this context Needham's notion of 'line' is a property of the terminology and in itself has an unavoidable unilineal bias (patrilineal or matrilineal): it says nothing necessarily about the mode of descent in the society concerned. For example, many societies in the Amazon, as well as the Sinhalese in Sri Lanka, have bilateral (i.e. non-unilineal) descent but two-line prescriptive kinship terminologies expressing direct exchange or bilateral cross-cousin marriage. Similarly Crow-Omaha terminologies are not always found with the mode of descent the respective vertical equations that characterize them suggest.

In the main I also avoid ethnographically specific labels for types or features of wide incidence. One exception is 'Iroquois', as well as 'Dravidian' when I am comparing it with Iroquois in terms of how they respectively treat cross kin (explained under stage 4 below), otherwise I call 'Dravidian' symmetric or two-line prescriptive or TLP. The distinction corresponds to that between Type A and Type B crossness of Trautmann and Barnes (1998). Another exception is 'Crow-Omaha' (also 'Crow', 'Omaha', separately), for which no satisfactory ethnographically neutral term has been coined. A third, occasionally, is Hawaiian for a type of equation (stage 6 below), for which 'generational' is to be preferred. I do not claim to be entirely consistent in these respects.

As already noted, most though not all writers on this subject seem to accept that some form of two-line prescriptive (TLP; i.e. bilateral cross-cousin marriage) is the base point from



which all other terminologies have derived historically. The basic reason is that there are no obvious signs that TLP is derivable from, or has been derived from, any other type of KT. Only logical reductions seem possible, such as Allen's tetradic society, featuring a terminology with just four terms along two dimensions, one consisting of vertical moieties, the other of horizontal moieties consisting of sets of alternating generations (+2/0/-2 etc. opposed to +1/-1 etc.; see Allen 1986, reissued 2004, for the main statement, reduced to its essentials here). One vertical moiety is marriageable for ego, the other is not; similarly, ego's horizontal moiety is marriageable for ego, that of ego's parents and children is not. Tetradic society can be either considered in terms of its basic four sociocentric categories or from the perspectives of individual egos. It is also sex-neutral, in the dual sense that it does not require the genders to be identified and can also be shown with either patri- or matri-moieties as the vertical dimension (or more strictly, helical dimension; see Allen 1989). The two dimensions it brings together are attested in the real world individually, but not in combination with the intensity and purity the model sets out.

What, therefore, might be the steps away from TLP? I list the main possibilities below and assume that the stage of tetradic society, which is in any case hypothetical and ethnographically unattested, has already been left behind. Stages 2 to 6, at least, should be considered alternatives, not a series. Stages 1 to 3 will normally have a prescribed spouse identified by kin term and a tendency to redefine categorically wrong marriages in accordance with the prescription. They will also not need separate terms for affines, or rather affines will be identified with some sort of cross cousin as marriageable relatives and the latter's cognatic kin.

- 1) *Two-line prescriptive (TLP) or symmetric prescriptive terminologies* expressing bilateral cross-cousin marriage. With or without sociocentric groups, e.g. moieties, marriage classes, sections. For Allen, sociocentric systems have an evolutionary priority, as an example of the whole of society being coordinate with the universe of kin. This accords with tetradic theory, but not all societies that have this world view have such groups, nor are such groups necessarily connected with affinal alliance (especially moieties). Also known as 'Dravidian', which label will be used on occasion below.
- 2) Expansion into a '*Kachin*' *three-line scheme* expressing matrilateral cross-cousin marriage (although the actual Kachin have a five-line terminology; cf. Leach 1945). This involves splitting the category of cross kin plus affines into two classes of wife-takers and wife-givers while preserving both prescription (now unilateral) and cross kin-affine equations. Dravidian crossness (not Iroquois crossness) should therefore be

consistent throughout (see stage 4 for the difference). Common locally in SE Asia and possibly Siberia, with isolated examples reported or claimed to exist in the Americas (Tsimshian, Sirionó, Txicao). Some of these terminologies have vertical equations in lines adjacent to ego's, typically ascending (equating senior kin with wife-giving affines) or descending (equating junior kin with wife-taking affines), in accordance with the status differences between wife-givers and wife-takers typical of such systems of affinal alliance. Some writers have sought to derive Crow-Omaha terminologies from this feature, though this has been contested (see further below). Only matrilateral (MBD/FZS) prescriptive terminologies seem to occur; 'pure' patrilateral (FZD/MBS) terminologies are unknown, despite informants' declared preferences for these kin types as spouses in some societies. The conventional labels 'matrilateral' and 'patrilateral' assume a male perspective; for female egos they should be reversed.

- 3) Expansion of stage 1 into an '*Aranda* four-line scheme (Australia); transition from stage 2 would be awkward, involving restoration of symmetric equations and mass deletions of terms, so choosing to move to either stage 2 or stage 3 probably represents a clear and irreversible choice. In fact, it may be easiest for Stage 3 to emerge directly from an expanded variant of the unattested tetradic society (N.J. Allen personal communication). Stage 3 bans first cross cousins in marriage but prescribes second ones, and the terminology consistently maintains Dravidian crossness, not Iroquois crossness, throughout. It also maintains cognate-affine equations. If there are sociocentric groups linked to the marriage system, they are sorted equally between unmarriageable and marriageable (see Korn's account of the Aranda, 1973: 26-32). Apparently restricted to Australia, where they have nonetheless come under question recently (Douset 2002). However, McConvell (1997) has suggested, on linguistic evidence, that the actual Aranda system developed out of a Kariera-type or stage 1 system no later than 1,500 years ago. Stages 1 to 3 are all undeniably prescriptive, i.e. express various types of cross-cousin marriage.
- 4) *Iroquois*: very common worldwide, including Australia, N. and S. America, and S. Asia (Parkin 1998 on Burushaski), though not Europe; much discussed in Godelier et al. 1998. Non-prescriptive, generally banning first cousins in marriage, but allowing second, third etc. cousins without prescribing them (cf. Vivieros de Castro 1998: 364). Iroquois terminologies lack cognate-affine equations, thus apparently making it difficult to maintain Dravidian (or TLP) crossness consistently beyond first-cousin range in ego's level, PG(E) range in +1 or (E)osGC range in -1, i.e. the minimal genealogical specifications within these ranges (cf. Hornborg 1998: 179). They therefore tend to rely on absolute sex rather than relative sex distinctions in determining crossness beyond these ranges, whereas Dravidian does the reverse, and does it consistently throughout the terminology (see the table in Tjon Sie Fat 1998: 69). Iroquois is therefore to be distinguished from Dravidian, as well as the genuinely prescriptive form of second cross-cousin marriage represented by the Aranda in stage 3 above (cf. Vivieros de Castro 1998: 356). Another difference is that the 'Aranda'

system neatly divides marriageable from unmarried groups (there are equal numbers of each), whereas Iroquois tends to treat all groups as potentially or actually affinal, apart from ego's (cf. *ibid.*: 360). This encourages a move away from sociocentric groups (if any) towards super-categories of consanguines and (potential and/or actual) affines. As we have just seen, first-cousin marriage will generally be banned, but the system may nonetheless allow (but not prescribe; cf. V de C 1998: 364) at least some second, third etc. cousins as spouses. This often seems to be associated with generational delays in the ability to renew alliances between groups or lines, in the fashion of the Munda (Parkin 1992: Ch. 8), though proof is still lacking regarding whether or not any Munda terminologies are Iroquois. In Viveiros de Castro's words (1998: 355-6), this suggests 'longer cycles of repetitions of the exchange' than in Dravidian. However, this last feature has also occasionally been associated with North Indian (Parkin 1998, on the basis of ethnography by Parry, Klingel) and Crow-Omaha (by Lévi-Strauss 1966, Héritier 1981 etc. [the Samo case]; see 5, below), opening up the question of transformational links between these terminological types and Iroquois. The property of allowing marriage to second etc. cousins but excluding first cousins may be a development of an earlier trend for a prescriptive system to disallow genealogical first cross cousins but to go on permitting remoter ones. The change then becomes formalized terminologically, producing Iroquois and no longer Dravidian crossness and breaching affine-cognate equations. Affines may retain the old composite terms, leaving cross cousins to find substitute terms (thus preserving the main sense of difference between consanguines and affines). Apart from coining entirely new terms, which in principle is always possible (perhaps leading to zero-equation terminologies; see 8, below), there are in principle two other ways of coping with this shift, namely by extending other terms to cover cross cousins, either vertically or laterally (see respectively 5, Crow-Omaha, and 6, Hawaiian, below), and by borrowing vocabulary from another language (e.g. Munda loans from Indo-European, Parkin 1992: Ch. 7). The hypothesis of a shift from prescription to Crow-Omaha is less certain and needs testing, though Trautmann and Barnes (1998) have hypothesized a shift from Iroquois to Crow-Omaha in North America. Some terminologies combine  $\pm 1$  Iroquois with Hawaiianization in ego's level, a situation called 'Cheyenne' in N. America, 'Tupian' in S. America, and 'bifurcate generational' by Dole (cf. Viveiros de Castro 1998: 384 n. 63; Dole 1969: 106). Several commentators have remarked on the difficulties in establishing the nature of the crossness of particular systems (Allen 1998: 327-8; Héritier 1981, cited by Viveiros de Castro 1998: 345). Viveiros de Castro's answer (*ibid.*) is that this is so, but that the easiest way of doing this is to examine the terminological equivalences of remoter relatives with primary kin (including first collaterals). The problem seems especially acute regarding Iroquois crossness, but it also affects how to diagram the +1 level with Dravidian first-generation collaterals and lineals (cf. Viveiros de Castro 1998: 337-8; Allen 1998: 317, 331 n. 4; Parkin 1997: 81). For Viveiros de Castro (1998: 369), Dravidian in S. India treats M as parallel kin, FZ as cross, while Australian systems reverse this. This is increasingly rejected for Australia (e.g. Dousset 2002). The problem is marriage, as (assuming patrilineal descent and group

exogamy) mothers marry into ego's group from their natal group, while father's sisters marry out of ego's group into their conjugal group. They therefore straddle the divide between marriageable and non-marriageable groups. No diagram can cope with this conflict, and a choice therefore has to be made how to treat them.

- 5) *Crow-Omaha or C-O*: very common worldwide, possibly including in Europe, at least historically (e.g. the linking of MF, *avus*, with MB, *avunculus*, in Latin). In C-O, cross-cousins are linked with kin in adjacent levels whom it would typically be incestuous to marry (see Parkin 1997: 110-111 for details). This does not explain the 'matrilineal' (Crow) and 'patrilineal' (Omaha) skewing of the two forms, however. C-O equations may appear with all the above types. This reinforces the Needhamite view (1971) that they cannot be seen as a discrete class of terminologies, as well as reminding us of the failure to associate them with any particular sociological feature (cf. McKinley 1971a; also Barnes 1976, 1984, who criticizes McKinley's own 'sociology of knowledge' explanation, McKinley 1971b). Nonetheless as equations they exist, and my own working hypothesis here is that they may be explicable as a way of effecting change in many other, more formal types of terminology (cf. Allen 1975 on Byansi in the Himalayas, 1976 on Sherpa). On that basis, other, non-C-O generational merging might be included here (i.e. without the lateral bias of C-O), e.g. in ego's own line (see Needham on Gurage, 1969). One hypothesis in terms of Crow-Omaha/Iroquois comparisons that needs further testing is the apparent tendency for Crow-Omaha patterns not to separate second etc. cousins from first cousins. For Trautmann and Barnes (1998), as we have seen, C-O features are likely to have emerged from Iroquois-type terminologies, at least in N. America (opposing Kronenfeld's argument [1989] that they derive from Dravidian crossness). Barnes has repeatedly pointed out (1983; also 2012) that Héritier's definition of the Samo terminology of W. Africa as Omaha (1981) ignores important differences between it and the actual Omaha terminology. A new collection has recently come out revisiting the problem of interpreting C-O terminologies (Trautmann and Whiteley 2012), though at the time of writing I have not seen it.
- 6) *Generational or Hawaiian* (Murdock): very common worldwide, except in Europe. In particular, in developmental terms, PosGC become identified with G = PssGC terms, perhaps leaving their old terms to cover affines only where these have previously been applied to PosGC as well. This will normally signal the unmarriageability of PosGC, who are now equated with siblings. This process may eventually extend to -1, then +1 (typically in that order; see below; cf. also my remark on Witowski, above). This stage could in principle emerge directly from any of the above schemes, apart, perhaps, from 5 (C-O). Being non-lineal in RN's terms, this scheme represents a definite breach with prescription and the 'post-prescriptive' types discussed above (1 to 5). It is also now less likely to be treated as a 'type' in its own right than it was by Morgan. For Morgan it was the simplest terminological type (which he called Malay) and evolved into, not from, Iroquois (my stage 5, his Turanian).

- 7) *Bifurcate merging changing into bifurcate collateral*, i.e. breach of lineal–same sex collateral equations, such that, e.g.,  $F=FB=MZH$  becomes  $F\neq FB=MZH$ . Can come at any stage. Not very significant in itself diagnostically, but may lead on to next stage if all collateral kin come to be distinguished through separate terms.
- 8) *Zero equation* (Allen; Murdock's Sudanese). Each primary kin type, as well as first collaterals, has its own term (e.g. F, FB, MZH, MB, FZH), though, as in Hindi and other North Indian terminologies, there may be equivalences between each such kin type and remoter relatives (Parkin 1998). In other words, it can be seen as a development of bifurcate collateral in which the remaining PG(E) equations, a relic of prescription, are broken up and replaced by isolating terms (e.g.  $F\neq FB=MZH$  [7, above] becomes  $F\neq FB\neq MZH$ , and  $MB=FZH$  becomes  $MB\neq FZH$ ). There are therefore likely to be a relatively large number of terms (as in Hindi). Also found in European history (Latin, German, Slavonic), and still today in Scandinavia (but cf. Swedish and Danish) and eastern Europe (Polish, Hungarian). Periphrastic phrases for some terms may occur (e.g. Arabic *bint* [D] *amm* [F, FB] for FBD), rather than primary terms (i.e. periphrastic terms do not constitute a separate type, despite Murdock's Sudanese). Arabic should probably be placed here, associated with FBC marriage. Many African terminologies are also zero equation ones, while others show Hawaiian features, but Ehret's data (2008) also show bifurcate collateral and bifurcate merging features, with a tendency for +1 and -1 to be more conservative than ego's genealogical level (i.e. bifurcate rather than Hawaiian, in line with Witowski's predictions).
- 9) *Cognatic* (Needham; Lowie's lineal). Historically may represent a re-sorting of terms after a terminology has reached stage 8, such that lineal kin are consistently distinguished from collaterals, which may well share a single term. For example, from a hypothetical pattern of five separate terms for F, FB, MZH, MB and FZH, a re-sorting may take place ending up with a more economical two-term pattern of father  $\neq$  uncle. This, of course, would involve the deletion of three terms (assuming the other two were applied to father and uncle respectively). The terminology therefore lacks the descent lines (Needham) of prescriptive and other post-prescriptive systems (stages 1-5). This deletion of terms may be happening in Polish, where *stryj* (FB) may be giving way to *wuj* as a term for 'uncle' rather than just MB (Parkin 1995), and it appears to have been a common transition in Europe that some terminologies have not (yet?) effected. *Pace* Murdock, this terminological pattern is not exactly the same as 'Eskimo', since such terminologies tend to retain classificatory equations in +2 (cf. Dole 1960), though it is not only found in 'complex' and/or modern societies. There is some inter-generational but non-lineal merging, especially of terms for cousins, but also  $GC = CC$  (especially in Latin-derived terminologies like Italian, Spanish and Portuguese). English has separate terms for GC and CC and only 'cousin' as an inter-generational term, so is a pure example of the type. The end point of terminological development? It is certainly hard to conceive of a further logical development away from English etc. that is not already covered in previous stages, though only time will tell whether this assumption is correct.

One practice, sometimes identified separately, namely oblique marriages of the type MB/ZD and BS/FZ that occur locally in the Americas, especially South America – also standard in South India according to Good (1980) – should really go with TLP, since such marriages are basically an alternative to bilateral cross-cousin marriage within the same system, that is, tend not to occur as an autonomous type (cf. Parkin 1997). As for FBC marriage, associated mainly with Islamic societies in the Middle East and Western and Central Asia, I have tentatively placed this under stage 8. Although clearly a marriage preference, this practice cannot be seen as a prescriptive system in the sense of stages 1 to 3. Repeated FBC marriage does not produce a closed formal system, as stages 1 to 3 do (Parkin 1997: 119), nor is there any tendency to redefine marriages to other kin according to a terminological requirement (indeed, since it is only a preference, other kin types can be freely married without being redefined, except as ‘spouse’, by the terminology). Indeed, there is no terminological scheme associated with this marriage preference, which co-exists with the Arabic terminology in the Middle East but with the quite different Urdu or Punjabi terminologies (both Indo-European) in Pakistan (more periphrastic phrases in Arabic, more primary terms in Urdu and Punjabi, though in all of them FBD can only be identified periphrastically, e.g. Arabic *bint ‘amm*). Moreover, the preference relates to genealogical referents, not a category, as usually with cross-cousin marriage and its immediate successors. However it arose, therefore, there is no systemic link between terminology and marriage practice, and it is best placed in stage 8, a non-prescriptive stage, on the basis of the zero-equation tendencies of the terminologies with which it mostly co-exists ethnographically. Ehret suggests that FBD marriage was present in the proto-Semitic speech community as long ago as 6000 years BP (2008).

Stages 1 to 5 are linked by three things which stages 8 and 9 lack (6 and 7 being transitional in this view). First, they are either fully prescriptive, with cognate-affine equations and consistent Dravidian crossness (1, 2 and 3), or else derived from prescriptive systems, still expecting, preferring, allowing or tolerating (but not prescribing) marriage with remoter cousins and/or other ‘relatives’, but without cognate-affine equations or the terminological redefinition of wrong marriages (though Iroquois is uncertain here). The hypothesis of derivation from prescription is more speculative in the case of C-O (5) than Iroquois (4) and needs further testing. For Trautmann and Barnes (1998), as we have seen, it is more likely that C-O derives directly from Iroquois, at least in North America. Kryukov, however, claims that C-O should be derived from asymmetric prescriptive (3 above), not TLP (1998: 312). This revives similar earlier hypotheses by Lane and Lane (1959) and Eyde and

Postal (1961), rejected by Coult (1965) and Barnes (1984). Since inter-generational equations may occur with both symmetric and asymmetric schemes, and since they are one way of effecting change away from prescription (e.g. Byansi and Sherpa; Allen 1975, 1976), the matter is not so clear and needs revisiting. However, as already noted above in part, whereas Iroquois is practically defined by its separate treatment of remoter from first cousins, C-O terminologies do not seem to make such distinctions (another hypothesis that needs further testing).

Secondly, stages 1 to 5 all have a bifurcate merging or bifurcate collateral pattern in at least +1, and possibly other levels too. Though not very useful in identifying different systems, precisely because many of the latter share these two patterns (cf. Kryukov 1998: 311), the threshold between their (partial) presence and complete absence does seem to mark the threshold between prescriptive and prescriptive-related (or post-prescriptive) systems on the one hand and those terminological schemes or types (8, 9) that have no systemic association with marriage rules and preferences on the other. The latter will, of course, distinguish affines from cognates post-marriage, but not also marriageable from unmarriageable kin pre-marriage, as in a prescriptive system (incest rules generally ban unmarriageable kin, but not all societies specify marriageable kin terminologically). In other words, in these later stages affines exist after the marriage, not also before it, as is formally true of prescriptive systems. There is therefore no term for spouse before the marriage, as formally with prescriptive systems (if only the term for the prescribed category), only afterwards. Hawaiian (stage 6) may eliminate all bifurcation too, thus crossing the above 'threshold', or may preserve it in +1 but eliminate it in ego's level and possibly -1 only, thus remaining transitional. As already noted, although Morgan treated Hawaiian as a distinct type of terminology, there is less of a tendency to do so today.

Thirdly, stages 1 to 5 represent terminologies that are sorted into descent lines (in Needham's sense, i.e. including, but certainly not being restricted to, actual vertical terminological equations – for example, the matriline formed by MF, MB, MBS, MBSS), which 6, 8 and 9 are not. In Lévi-Straussian terms, stages 1 to 3 are elementary systems, stage 5 semi-complex, stages 6 to 9 complex (Iroquois not being separately identified in his paradigm).

As already noted more than once, bifurcate patterns often survive longest in +1. Indeed, Kryukov suggests that, while ego's level may be more subject to merger (Hawaiianization), +1 is more susceptible to differentiation (1998: 313), or at least to maintaining differentiation. This may be because, formally speaking at least, this level represents marriages that have

already taken place. More particularly, it expresses past sibling exchanges in marriage, which can be found in some form in both prescriptive systems and those immediately derived from them (at least 4, if not 5). However, where this form of exchange cannot be repeated in the following generations between the same groups or lines, a different terminological pattern is likely to emerge in descending levels, starting with ego's and then proceeding to -1.

Whatever the merits of this particular argument (which I partly derive from Viveiros de Castro 1998), certainly once the +1 level ceases to be bifurcate we can expect that the terminology has lost all systemic association with marriage, which is now likely to be 'complex' in Lévi-Strauss's terms and non-directed by the terminology, i.e. with open choice of spouse apart from incest prohibitions.

Stage 8, with its zero-equation pattern and possible periphrastic phrases for at least some kin types, is probably relatively unstable and tends to drift into stage 9, as has certainly happened repeatedly in Europe. To repeat, stage 9 really does seem to represent a certain endpoint: just as no attested system seems to evolve *into* TLP, so nothing seems ever to have evolved *away from* stage 9. Indeed, it is hard to envisage any further development, not least because most if not all of the logically possible patterns of kin-type equation and distinction are accounted for in earlier stages. Stage 9 cannot evolve directly from any of the genuinely prescriptive systems (1 to 3), nor from Iroquois or C-O, but may do so successively through 7 and 8, or else directly from stage 6 through the introduction of lineal-collateral distinctions (this is a formal possibility, though it may also explain the cognatic features of Murdock's 'Eskimo' type; this hypothesis needs further testing). Generally, the assumption is that all these changes are irreversible (cf. Kryukov 1998: 313 on all these points; note that he uses Lowie's 'lineal' in place of Needham's 'cognatic'; see also Godelier 1998: 406).

As already suggested above, derivation of stage 9, cognatic, from stage 8, the zero-equation, many-term scheme, would involve the deletion of some terms, e.g. of FB in favour of the term for MB becoming a global term for 'uncle' (indicated for Polish; Parkin 1995). The argument that TLP cannot be derived from anything else because it would involve deleting terms, which is less likely than their expansion (Allen 2008: 108), therefore seems not to be applicable to the transition between stages 8 to 9. In fact, the 'Hawaiianization' of terminologies (stage 6), in which intra-cognate distinctions within a genealogical level are removed, also involves a process of deletion. In practice, of course, redundant terms simply fall out of use, rather than being deliberately or consciously deleted.

Some caveats should be made at this point in the article. First, as should by now be apparent, these nine stages are not an inevitable series through all of which all societies either



have gone or will go. In any case, that would be close to impossible. Once some changes have taken effect, other, alternative ones appear to be ruled out. Thus it is hard to conceive of a society changing from stage 1 (TLP) to stage 3 (Aranda) via stage 2 (Kachin), for reasons already given; similarly, Hawaiianization may rule out change into Crow-Omaha and vice versa, because either change means making a definite choice between lateral and lineal extension. Secondly, while with few exceptions (mainly urban, literate south Indians), prescriptive systems involving cross-cousin marriage are found in relatively small tribal societies with simple traditional technologies and minimal or no division of labour, many societies that can be defined in this way do not have such systems, and may indeed share the same terminological principles with a society in the more technologically and economically developed world, as with the famous near-match of Inuit ('Eskimo') terminologies with English. One striking instance of this is hunter-gather societies, which, outside Australia, usually lack such systems, despite both they and the systems being seen as having an evolutionary priority (often falsely as far as specific present-day hunter-gather populations are concerned). This suggests that grand paradigms of change are less likely to be useful than specific histories of change in limited regions of the world. This recalls Allen's observation (2008), already cited above, that reasons for change are likely to be local. It also suggests that work in this area is more likely to give rise to middle-range theories of the sort Jack Goody has adopted into anthropology from Robert Merton's sociology, not grand theories in the manner of Radcliffe-Brown's functionalism, Lévi-Strauss's structuralism or David Schneider's cultural relativism. Nonetheless Allen himself sees grand theories of world-historical change as still a possibility (personal communication).

### III

In the context of terminological change, the connection between prescriptive terminologies and prescriptive marriage systems based on cross-cousin marriage requires we address the problem of what makes a society abandon cross-cousin marriage, with the possible implications of this for the associated terminology. As just noted, Allen has argued (2008) that such explanations are likely to be local rather than global or universal and need investigating separately in each case. Outside elite pressure is certainly one factor, as with missionary influence condemning the marriage of cross-cousins or upper-caste dislike of any cousin marriages in India (especially in the north) influencing the lower castes and tribes, many of the latter especially having cross-cousin marriage in some, often attenuated form.

However, that does not explain how change got going in the first place, i.e. how these elite ideas emerged from a hypothetical situation of universal cross-cousin marriage possibly even in prehistory. As regards Native North American hunter-gatherers, one relatively recent hypothesis is that TLP terminologies and cross-cousin marriage are found in small bands which maintain band endogamy, while Iroquois crossness appears with larger bands that tend towards band exogamy, since exogamy permits, or forces, the search for a wider range of potential spouses than cross cousins, who in this scenario are apparently found within the band (Ives 1998). Trautmann and Barnes (1998: 54-5) make a similar claim, also for North America, including Crow-Omaha systems with Iroquois for this purpose. This may be true of North America, but it can hardly be a universal explanation, as many societies around the world with the TLP complex are agricultural and divided into exogamous descent groups as the main operators of cross-cousin marriage, meaning that one's cross cousins are in other descent groups. The debate between Dole (1969) and Basso (1970) mentioned earlier similarly revolved around the question of whether the Hawaiianization of ego's level among the Xingu reflects a situation in which population decline has led to group endogamy (Dole) or to group exogamy (Basso). Again, whatever the case, it can only be a local explanation. Lastly here, Kronenfeld (1989: 96ff.) has speculated on the possible reasons for a change from Dravidian (or TLP) to Iroquois, claiming that an Iroquois terminology is easier to handle when it comes to tracing kin ties genealogically and that the imperative to trace them would arise if the fit between genealogy and social category he claims to accompany Dravidian terminologies broke down. It is not entirely clear what he means by 'social category' at this point in his article: Dravidian-type terminologies are not necessarily associated with sociocentric groups like marriage sections, though they do typically distinguish lineal/parallel kin from cross kin/affines. In any case, Kronenfeld does not suggest why such changes might take place. His reliance on social morphology in explaining terminological types has been briefly criticized by Trautmann and Barnes (1998: 57, n. 6).

As Needham was at pains to point out in several of his analyses of systems of affinal alliance (e.g. 1967), there is no necessary association ethnographically between marriage practice and the terminology one would expect from it. While there may be limits to such variation, a prescriptive terminology can occur with a 'complex' system of marriage, a preference for cross-cousins as spouses may occur with a non-prescriptive terminology, and a symmetric or two-line prescriptive terminology may occur with an asymmetric pattern of affinal alliance based on MBD/FZS marriage (though not, it seems, the reverse). These combinations of inconsistent features themselves suggest change. As a general hypothesis,

therefore, we might argue that human populations may start as tetradic, then move to TLP, before changing into one of the other stages set out above. This may be to another form of prescription, such as stages 2 and 3: there is evidence of stage 1 to stage 2 transformations in SE Asia, especially eastern Indonesia (i.e. from symmetric to asymmetric or two-line to three-line), and evidence of stage 1 to stage 3 transformations in Australia, where the latter appear to be concentrated (i.e. from two-line to four-line). Reports (e.g. by Turner 1977: 40; Dousset 2002, McConvell 1997) indicate that different Native Australian societies have influenced one another in the adoption of different affinal alliance systems, even since contact with Europeans, i.e. very recently given the 60,000-year history of human settlement in Australia, and that these influences have been indigenously recognised for what they are and discussed as such, i.e. people are conscious of them. Alternatively transition from TLP may be out of prescription altogether, certainly to Iroquois, and possibly to C-O. In the case of Iroquois, one can trace the breakdown of prescription logically in the very differences this type displays from Dravidian or TLP. C-O, by contrast, seems remoter from TLP, and as we have seen, direct transformations between stages 1 or 2 and stage 5 are uncertain and controversial.

In principle, one possible reason for the abandonment of cross-cousin marriage may be connected with an emerging dissatisfaction with the alleged closeness of ego and alter as spouses. While potentially relevant, this hypothesis is in need of serious qualification. It has long been recognized that cross-cousin marriage does not typically apply to first cross-cousins alone but also to second etc. cross cousins and even alters who are not clearly linked to ego at all genealogically, but only as a member of a (marriageable) category. This is the classificatory principle at work, in which a term applies not just to the logically most reductive or closest kin type but to their remoter equivalents too on the principle of same-sex sibling equivalence (and therefore not only involving cross cousins). This concept goes right back to Morgan, was dismissed by Kroeber (1909), and revived in Morgan's defence by White (1958). It is distinct from the prescription of second cousins as spouses (stage 3 above), although the classificatory principle applies to kin in these cases too. In any case, demographically it would be impossible for a system of cross-cousin marriage to work if it applied solely to first cross cousins (Needham 1959: 127, 1960: 105), who may even be banned in particular prescriptive systems, e.g. the Wikmungkan prohibition of the genealogical FZD (Needham 1962, McKnight 1971). The dislike of close kin as marriage partners is definitely an aspect of how many societies look at marriage, but so is a preference for it, and this distinction cannot be simply mapped on to that between prescription and its

absence. Perhaps the most that can be said is that the stability of systems of cross-cousin marriage relies on first cousins being included in the prescription: where they are not, terminology and marriage practice may begin to part company. Examination of genealogies, where available, can reveal the extent to which genealogical cross-cousins or other prescribed spouses are actually married, a good example being Rivière's study of the Trio of Guyana (1969).

Certainly Lévi-Strauss's notion of semi-complex systems, in which close kin are banned in marriage with reference to their membership of a range of lineages, does have ethnographic validity. One way of describing the formal features of systems of cross-cousin marriage is to say that (assuming patrilineal descent) a man repeats the marriage of his father, in that he takes his wife from his mother's kin group, although this is a formal property of the model and not necessarily an ethnographically universal practice. Semi-complex systems, by contrast, may well prohibit one's mother's lineage or other kin group as a source of a spouse for oneself, as well as other lineages (e.g. those of MM, FM). This is often linked to C-O terminologies, but not necessarily, as the case of north India indicates (with zero-equation rather than C-O terminologies), and it usually means that one should not repeat a marriage into a particular kin group within a certain number of generations, often specified as a norm or rule. Thus in north India, alongside the so-called four-*got* rule (a *got* being a patrilineage), which prohibits ego from marrying a woman from any of the *got* of his four grandparents (incidentally ruling out first-cousin marriage), there is also the *sapinda* rule, banning marriages to anyone related to ego within seven generations on the father's side and five on the mother's (often reduced in practice). One additional feature that is often found in such cases, though it is less discussed in the literature, is a preference or tendency to remarry into such kin groups after the prohibited number of generations has passed. In other words, it is good to marry into a previously related kin group, but not too closely (e.g. Parry 1979, Parkin 1992: Ch. 8). Even less remarked is the possibility that alliances between sibling groups within the same generation may be intensified, as among the Munda, where many tribes in this language family of east-central India describe their marriage practices in terms of marrying a sibling's spouse's sibling (equivalent to a cross-cousin with prescription; Parkin *ibid.*); while current marriages may not repeat those of previous generations, they may follow one another quite intensely within the same generation. The delay in immediate alliance renewal between groups also has the effect of dispersing the alliances of one's kin group among a number of other kin groups over time, with possible implications for power, wealth or prestige. This dispersal is not itself unique to this sort of situation and can also be found

with prescriptive cross-cousin marriage of all sorts, in which, sometimes, it is sufficient for one sibling to marry according to the rule. However, in the situation I am currently discussing, exemplified by north India, it does mean that, while one's spouse may conceivably be definable as a remote cousin, marriage practices are not going to be defined by marriage to cousins, including cross cousins. In north India, indeed, cousin marriage of any sort is widely disliked, in contrast to the south, a canonical example of TLP or bilateral cross-cousin marriage. In north India, closeness of relationship is by definition a restrictive factor in choosing a spouse in a way it is not with prescriptive systems, even though particular societies with prescription, such as the Wikmungkan of Australia, already mentioned, may ban a genealogical first cross-cousin (here FZD) despite their bilateral prescriptive system (Needham 1962, McKnight 1971).

The desirability of marrying closely related kin is, of course, a practice of elite groups the world over, culminating in sibling marriages in some rare but frequently cited cases, but also being an aspect of upper middle-class marriages in nineteenth-century Britain (cf. Kuper 2008 on the tangled relations of the Wedgewoods and Darwins). None of this typically involves terminological prescription, much though it may involve cousins. It is therefore difficult to see questions of the genealogical closeness or distance of kin as spouses as an immediate factor in terminological change, nor is it the exclusive property of a certain type of society. As for the emergence of zero-equation and especially cognatic terminologies, this may be linked with a situation in which lineal distinctions or those between patrikin and matrikin cease to be operationally relevant, despite a residual preference, perhaps, for the former as more salient than the latter in people's identity formation (e.g. transmission of surnames). The indication in Polish that *stryj* as FB is giving way to *wuj* (formerly just MB) as uncle is one example of this trend (Parkin 1995).

Marriages to close kin may therefore be a bit of a red herring in this context, though they have occasionally had a peculiar impact on anthropological theory. For instance, it's just possible that the oddity that Morgan married his own cross-cousin prevented him from associating Dravidian terminologies with cross-cousin marriage in order to preserve his own self-image as a civilized gentleman (Trautmann 1984). More fundamentally, Morgan linked both Iroquois and Dravidian terminologies with the very nineteenth-century notion of group marriage, not with cross-cousin marriage, which the Iroquois lack. This involved downplaying the differences Morgan himself recognized between the two types of terminology in favour of their similarities, also made necessary by his desire to use terminological types to prove his theory of the Asiatic origins of Native Americans,

Dravidian being seen as an Asian version of North American Iroquois in this argument (cf. Trautmann *ibid.*; 1981: 71-2). It was left to Rivers to make the link between Dravidian terminologies and cross-cousin marriage (Trautmann *ibid.*: 74). However, neither Morgan nor Rivers was very concerned with changes in the terminologies themselves, which they used mainly as a proxy to suggest change in other parts of the kinship system, especially forms of marriage (cf. Parkin 1997: 162).

There is another way of looking at the impetus for change. By definition, terminological change is a matter of alterations in how kin are defined. Many if not most societies that are prescriptive or immediately post-prescriptive in type (i.e. tetradic society, as well as stages 1-4 and possibly 5) define everyone within them as kin, regardless of the possibility or otherwise of tracing genealogical links between different individuals. However, other societies restrict the circle of kin, very often on the basis of genealogical traceability, defining other members of the society as lying outside that circle, and using other sorts of category (professional, ethnic, friendship etc.) to place them in one's social universe. This is typically the case for modern societies with a well-developed division of labour, so that for many people one is defined by one's profession or other non-kinship role rather than a kinship status, though it is not restricted to them. South India is an example where this situation goes along with terminological prescription and prescribed cross-cousin marriage, but more generally it is probably the case that the emergence of non-kin-based categories of significant others leads to a situation in which one typically marries someone not previously defined as related to oneself. This in turn makes terminological prescription redundant. Contemporary reports (Fuller 2008, Kapadia 1993) of changing attitudes to marriage in south India indicate that this may be happening here too, as cross-cousin marriage is falling out of favour in at least some circles of what nonetheless remains a complex and stratified social system in which class is emerging as an influence alongside caste. This situation invites a longitudinal study to confirm this change, as well as to test the proposition that change will ensue to the terminology as well.

Returning to the start of this transformational paradigm, it is reasonable to ask at what stage of prehistory TLP terminologies emerged and became established and whether the first stage was indeed something like tetradic society. Apart from some eighteenth-century precedents, usable historical ethnographic data hardly go back beyond Morgan's researches in the middle of the nineteenth century, but we can nonetheless assume that such terminologies have a long history before that, going back millennia, though in the absence of direct evidence we can hardly know for certain. However, Hocart long ago sung the praises

of circumstantial evidence as every bit as useful and reliable as direct evidence (1936: Ch. 1). Tetradic theory uses precisely circumstantial evidence in that it extrapolates backwards from attested terminologies and marriage systems to a putative ancestor. Moreover, it also postulates that the four categories designated by the hypothetical four kin terms need not be labelled linguistically but could be marked in some other way, e.g. colour differences (Allen 2008), suggesting a further step backwards in time, before humans had language (60,000 years BP?). Other social anthropologists have noted Allen's initiative and commented critically on it. Barnard (2008), who sees phases in the development of language as more or less paralleling phases in the development of kinship—the latter phases represented respectively by the ideas of Morgan, McLennan and Lévi-Strauss—would seem to regard language as a more likely means of expressing the tetradic form. He also recognizes the potential of tetradic theory but does not see it as the only possible ancestor. Layton (2008) goes a step further to deny that the simplicity of tetradic society gives it a historical priority, but does not develop the point.

At the very least, whether one accepts tetradic society as prior to TLP or not, humans with either must have developed the ability both to classify kin through a terminology, whether linguistic or not, and to formulate rules, or at least recognize norms, regarding marriage practice, as Meyer Fortes pointed out in what was probably his very last work (1983). As Gamble has pointed out (2008: 33), this clearly distinguishes non-human primates from at least later hominins: 'Negotiation to form alliances and cliques is a primate trait...while formal recruitment to pre-existing categories is not,' i.e. is human. Allocation to such categories is also at root mandatory, despite their potential for negotiation and metaphor. Gamble's insight goes along with oft-repeated emphases that primate kinship can only be studied biologically—only among humans is kinship also social, requiring both rules and classification, and only living humans can discuss it with one another and with the anthropologist.

If the study of human biology can put dates to the emergence of these intrinsically human features (and there still seems to be widespread disagreement regarding the dating of this emergence, as well as what form or forms it took; see Allen et al. eds. 2008), it will give us some idea of how far back kinship terminologies as classifications go, even if we can never be entirely certain of the form they took originally. Recent developments in cognitive science and the links being forged between it and social anthropology, evolutionary biology, evolutionary psychology and linguistics (on the latter, see Jones et al. 2010) are increasing the potential for this to happen, as some of the chapters in the recent volumes *Early human*

*kinship* (Allen et al. eds. 2008) and the Per Hage memorial volume (Jones and Mililic 2010) indicate. However, the basic distinctions in this form of evolutionism are those between distinct hominin and by extension primate species, not those between the stages in terminological change I have postulated in this article: they therefore do not constitute a direct model for the latter. The latter all apply to modern humanity, i.e. to *Homo sapiens* in the categories of evolutionary biology; they therefore assume the same cognitive and classificatory faculties on the part of all the different speech communities that produce and use them. Many anthropologists, after all, have openly marvelled at the ability of certain ethnic groups in places like Australia to devise complicated systems of cross-cousin marriage that they (the anthropologists) have great difficulty in grasping. How such systems arose and developed is as legitimate a question as their principles and practice in the here and now. It is nonetheless a question that recognizes difference and diachronic development only within the parameters of a common humanity and humanity's common cognitive, classificatory and rule-generating abilities.

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