

 <p>WORDS AND PROCESSES IN MAMBILA KINSHIP</p> <p><i>The Theoretical Importance of the Complexity of Everyday Life</i></p> <p>DAVID ZEITLYN</p>	<p>Words and Processes in Mambila Kinship The Theoretical Importance of the Complexity of Everyday Life</p> <p>David Zeitlyn</p> <p>Lexington Books ISBN 978-0-7391-0801-7</p>
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Chapter One

Introduction

This book seeks to link the empirical study of people as they go about their everyday life with some of the larger theoretical concerns of pragmatics and social anthropology, in particular kinship theory. The starting point is the way in which people use kin terms, pronouns, and proper names when talking to and about each other. Such speech contains more complexity than conventional theory can explain. At a theoretical level it requires the rejection of the conventional distinction between address and reference because this distinction is inadequate to deal with the complexities of actual talk. This is demonstrated by the conversations discussed below, in which speakers display great sensitivity about exactly how people are referred to, whether or not those people are present, and about who is listening. The patterns of usage of pronouns, names, kin terms, and other referring expressions are revealing of wider social processes. They are intimately linked to the fine grain of power, social divisions, and negotiation, and are thus of great interest to contemporary anthropology. This suggests that the study of kin terms is not a quaint, old-fashioned hobby like stamp-collecting, but a means of addressing pressing and important theoretical concerns. This is especially so since high theory so grievously lacks connections with the patterns and the flux of everyday life.

Field recordings which I made in Cameroon, and subsequently transcribed and translated according to the precepts of conversation analysis, form the basis of a detailed ethnographic account of language use. Mambila language use is here studied in a variety of different ways which are used to give a methodologically heterodox but systematic account of kinship as process.

This work is a lateral reconsideration of one of the bastions of kinship theory: kinship terminology. The study of kinship terminology has been unfashionable for so long that it has not been much affected by those varieties of post-modernism which claim that there is nothing to study, that the whole edifice is

an artefact of those engaged in the study of kinship. I believe this is profoundly wrong, and we can do no better than return to Louis Henry Morgan's original observations (1871) to see why: granted the large number of languages in the world (6,000 is the current guesstimate) it is astonishing how terminological subsets of widely separated languages display precisely the same patterns, and even more astonishing that they are used for (more or less) the same purpose: to talk about those with whom the speakers have important relationships concerning sex and the socialisation of babies. However, I do not want to get caught up in arguments about the universal definition of kinship, nor the dubious priority of descent over marriage or vice versa, nor even the relationship between kinship behaviour and terminology. These are old arguments, and are best approached indirectly. So my starting point is not kinship terminology, although I believe that this can be given a language-specific definition using grammatical features of individual languages which distinguish kin terms from names and pronouns. Such definitions are neutral between different anthropological arguments (see below). To maintain my separation from such arguments, I start by considering the many different ways in which people can talk about and to each other. After this I can begin to investigate ways in which kin (however that term is understood) use language as part of their everyday lives. By and large our lives are predictable and unsurprising. If they were not, then the events which strike us as exceptional and distressing would not stand out as such. For example, consider the misbehaviour of a child or the meanness of a neighbour (both of which may be expressed non-verbally). Even a badly behaved child behaves acceptably most of the time; but then it misbehaves, maybe because it is tired, or does not like a particular food. We get clues to a child's frame of mind from the tantrum which precedes bedtime, or the food thrown onto the floor, let alone any verbal clues such as 'I don't like rice pudding'. These are events in social life which we attend to, and which we interpret. We test our interpretations by basing actions upon them. Everyday life is well understood by its participants, who manage their way through life's vicissitudes with astonishing finesse. The implications of such truisms have not, I believe, been taken sufficiently to heart by anthropological theorists, and that is one of the motivations for writing this book.

This book uses pragmatics to provide a fresh perspective on some analytical problems within social anthropology. Pragmatics offers anthropology not only new sources of evidence in the detailed structure of conversation, but also empirically sound foundations for the results which obtain. Conversation as a type of action is undeniably a social phenomenon, and much of the abstraction which precedes orthodox linguistic theory-making is therefore misplaced. Pragmatics stresses the situational, immanent, and negotiated qualities of language use. Instead of focusing on speakers, I focus on speech events. Similar positions are found in the literature of the 1960s and, for example, in the first edition of *Directions in Sociolinguistics* (Gumperz and Hymes eds. 1972). Yet the field of kinship studies remains dominated by the sway of the genealogical method and the *abstracted* analysis of kinship terminologies. Although there have been moves

away from this they are few and far between.¹ In their papers in *Directions in Sociolinguistics* both Ervin-Tripp (1986) and Tyler (1986) use the rule-based generative models, propounded in greater detail by Geoghegan (1973). These are ego-based and inflexible. Socio-cultural factors are connected directly to verbal output without intervening steps of cognitive modelling. Levinson's (regrettably unpublished) Ph.D. contains a comprehensive critique of Geoghegan and the rule-based approach.² Pragmatics may not be able to offer anything quite as neat looking, but it has an openness to ongoing process, to situational fluidity, and to the strategic usage by co-conversants which is not found in rule-based systems. This is not to say that semantics has no place in what follows. People bring to any interaction reasonably stable expectations of other participants and of the world in which they live. In the course of the interaction some of these expectations may be modified. Meaning is not created anew in each conversation, but interpretations and shades of meaning may be altered. To say that pragmatics encourages us to examine the negotiation of meaning in the course of conversation is not to pretend that there are no stable meanings, but rather that stability is an emergent phenomenon. Stable meanings are the cumulative product of many conversations in the course of which reasonably stable expectations of the world, and its languages, are formed.

This is to summarize an important and continuing debate. The material discussed here is relevant to that debate but my approach is tangential to its central concerns: however the meanings of words are best understood, an anthropological understanding should include the way in which words are used by speakers in actual circumstances and not concentrate on purely abstract relationships between words. I suggest that the fringe of associated meanings, the connotations and signification (in philosophical terminology) are of as much interest to anthropologists as the stable core meanings. We acquire language on insubstantial and inadequate grounds (Macnamara 1982) and continue to talk knowledgeably (Shanker 2001). Knowledge and confidence in what we say accrues through bootstrapping.³ Meaning is constructed through conversational interaction rather than by the internalisation of paradigms (Heritage 1984: 75-102 esp. 99). In a sense the definitional cores, the very paradigm cases which have been shown to be widely shared (Rosch *et al.* 1976; Rosch 1977a) may be immanent, and implicit, but not acknowledged. The procedures of conventional analysis tend to obscure this.

Of course, traditional kinship analysts can rest secure since their avowed intentions are different from mine. They wish to analyse the core foundational concepts and relationships between these basic materials; sociological correlates are neither necessary nor expected. Instead I start with language-in-use, which makes it impossible to exclude society and concentrate on abstract formal analysis. Moreover, since kinship terms are part of a set of social deictic⁴ terms, the use of this set may be of sociological importance. The move from kin term to name to pronoun may be more revealing than the move from one kin term to another. Even groups with universal kinship terminologies (where every person

can be assigned to a kin category with an appropriate kin term) have pronouns, names, or nicknames.⁵

This puts traditional analysis in a strange position: it is oddly irrelevant. My questions are different from theirs and hence the tenor of the answers is different. By starting with language-in-use we are inevitably involved in sociological analysis. This is good for anthropology.

Other work⁶ focuses on the abstract structures of kinship terminologies. Read (1974, 1984) formalized these as algebras. If kinship terminologies form isolatable sets (and I believe they do) then this is a legitimate exercise. Moreover, such kinship algebras may provide solutions to Morgan's problem:⁷ why are there so few different types of kinship terminology in the world? However, and this is where I part company with Read and other formalists, an algebra needs rules of interpretation to be used as a model, and any one algebra may have many such rules of interpretation (which explain at a stroke why many different languages have similar structures: they all incorporate different interpretations of the same algebra). To be fair, Read is admirably clear on this point.⁸ However, my interests are less in the formal structures, and more in the rules of interpretation.⁹

My long-term goal has been to devise ways of studying kinship. Here I present some early steps on the path towards a more systematic account. As I have said, the conceptual starting point is not restricted to kinship and kin terms, but includes the broad field of social deixis, by which I mean all the linguistic and non-linguistic ways of referring to people. At one extreme this includes descriptive expressions, such as 'the person standing by the door', which achieve reference.¹⁰ Why and when speakers use other means to refer to people is one of the general questions which prompted this study. In particular, I wish to examine the distribution of social deictic terms to see what social correlates there may be. I am especially interested in the *choices* which speakers make between kin terms, proper names, pronouns, and titles. I call all of these terms 'person referring expressions' (abbreviated to PRE). To understand which person gets what PRE in which social context is no small challenge for an anthropologist.

Choices are made in other contexts too. In chapter Four I examine the lists of names which were elicited using kin terms as prompts. Informants were asked to identify their siblings and in-laws, which they usually did by name. The resulting lists of individuals were analysed in the light of the relevant genealogies (which had been collected separately, in some cases years before). There are some curious omissions and inclusions in the lists elicited. These raise the possibility that the choices made by these informants involve factors other than the genealogical meaning of the kin terms used as prompts. My goal has been to understand the pattern of such choices. For if, in addition to genealogical meaning, other factors are at play when an anthropologist asks questions, how much more likely is this to be the case in natural conversation?

Naturally occurring speech is an interesting place to look for evidence of shared conceptual schemes. It is important that we recognise why this is so. A

speaker who is 'correctly understood' can therefore infer a community of understanding or shared meaning. Linguistic usages which are demonstrably accepted in naturally occurring contexts are different from the (possibly unreliable) stated opinions of a single maverick informant. This is a solution to the problem of informant dependence which troubles some anthropologists. Moreover, 'small' words may reveal as much about conceptual structure as the 'big' words which figure as the focus of noun or verb parts. Indeed, the very fact of their being small and unregarded implies that their use may reveal basic unexamined assumptions. A group may debate the meaning or the significance of 'religion' or even 'personhood', but the way in which the participants use pronouns implies a consensus about certain categories of person which underlies the explicit or conscious debate. Moreover, speakers do not think about the use of 'small' and 'big' words at the same time. When speakers are debating, arguing, or discussing they are not minding their p's and q's. Not only is there insufficient time: there are also more important things at stake (from the participants' point of view). Hence pronouns and other social deictic terms may reveal shared conceptual structures which are relevant to social theorists. I have in mind, of course, social anthropological theories of kinship and kinship terminologies. This position is further explored below.

The theoretical and analytical problem now posed is how to use conversational data, and how to proceed with the analysis. In principle we, the analysts, can take our cues from the conversationalists: we can infer intentions in a manner similar to that of co-conversationalists. We can detect snubs, irony, politeness, or its absence, perhaps not as well or as the actors but *in the same way*. The problem is to systematize and to make explicit the basis upon which we do this.¹¹

My working assumption is that a speaker's choice of social deictic term is sociologically motivated and therefore that its analysis may be sociologically revealing. From a linguistic point of view, a speaker wishing to refer to another person could use any of their name, title, kin term, or pronoun (once problems of ambiguity have been allowed for). Schegloff and Sacks (1979) in seeking to uncover the conversational preferences which structured conversational exchanges identified a preference for minimisation in the identification of referents (to which Schegloff returned in 1996).¹² Moerman has since demonstrated (1988: 31ff) that the same principles structure Thai speakers' references to people. A further example lies in Brown and Levinson's analysis of politeness (1978). This is predicated upon Grice's maxims for conversation¹³ which are systematically violated due to the social factors of interest to anthropologists. In a like manner, in my examination of the use of social deictic terms below I found that speakers departed from the usual preference for minimal identification for emphasis and as a result of social factors. For example, in English it is often thought rude to refer to a senior kinsperson by a 'mere' pronoun (especially in their presence) although this would be sufficient to identify the person concerned. Hence the idiomatic rebuff: 'Who's "she", the cat's mother?'¹⁴ The use of a pronoun, especially *in the presence of the person referred to* makes that pronoun marked.

Stereotypically, in such cases the use of a kin term implies recognition of a parent-child relationship, which is put into doubt by pronominal usage. Granted a context of usage—particularly the presence of the person being referred to—it is perceived to be rude to remove this recognition and so the principle or maxim of providing only the minimal necessary identification of referents (as often accomplished with pronouns) must be violated to preserve social structure in the form of stable parent-child relationships.

As one speaker replaces another and is in turn replaced, the topic being discussed may or may not change. The use of anaphora, or so-called ‘textual deixis’ (the use of a pronoun or preposition to refer to a preceding phrase) is an index of this: ‘Enough of that: now to talk about X’ is a bald and clear example of a topic boundary. ‘Enough of that’ would suffice. Fox discusses examples where an anaphora is used by one speaker to keep a topic going which a co-conversant thought had closed (1988). Such theories of anaphora are based upon notions of discourse segmentation: the natural divisions of a conversation by speaker and topic. In part, the use of anaphora marks a discourse topic as still being open, and keeps it open. In an example presented below (see chapter 3), in the course of a dispute about alleged adultery an anaphora is used across a topic boundary in order to mask or control disagreement. In that disagreement, as in most others, Grice’s principle of co-operation (1969) was systematically violated. Pronouns were used as a way of preventing the argument from becoming bogged down over a single (side-) issue. Pronouns may act as variables whose reference is not agreed upon. The use of an ambiguous pronoun allows the discussion to move on, leaving the precise identification of the referents to one side. The ambiguity may prove unimportant, and may never be resolved: ordinary conversation typically leaves large numbers of unresolved issues and loose ends.

Vatuk’s research (1982: 70 and 92-3) provides detailed examples of usage which show how cultural values (such as modesty or shame in the presence of affines) lead to systematic departures from the use of kin terms as genealogically understood. For example, she records the use of M for MM, and MZ for genealogical M. (Using standard genealogical designations: M is mother; MM is mother’s mother; Z is sister, so MZ is mother’s sister, and so on). A further example from a different cultural tradition is found in the work of Ronald Casson (1975; Casson and Özertuğ 1976) in which the use of both kin terms and proper names in address is explicitly considered. Casson and Özertuğ consider the hierarchy of respect among the adult male population of a small Turkish village, based on trustworthiness, wealth, and age. They demonstrate that this hierarchy is consistently maintained among the population and then show that correlates exist between this hierarchy and address term usage. This could be used, for example, in the analysis of dispute resolution to show the shifts in allegiances and status assessments which comprise the fine grain of social life. The point is to find places such as this where discourse structures have some sociological pay-offs. To take another example where data is available for a very wide range of languages: comparison of sections of the Bible in different languages may reveal

differences in how people are identified and how reference is then maintained.¹⁵ This needs to be complemented by analysing how the different strategies for making and maintaining reference are used within and between social groups and in different genres of speech. The asymmetry of the use of names and kin terms between parents and children in many societies is the type of case I have in mind. Parents use names to (and about) their children, who answer them with kin terms. In Europe and America divorce and remarriage have now become so common that the choice of appropriate terms for step-kin is a real problem, and the solutions to this are sociologically interesting. English has a notorious ambiguity in the area of parent's sibling/parent's sibling's spouse. This is particularly unclear when we come to parent's sibling's third spouse, or parent's third spouse's sibling. The patterns of choices made are likely to be revealing of patterns in family structure and relationships.

If kinship terminology cannot be properly analysed in isolation from social context then what sort of analysis am I advocating? In other words, how can I move from the programmatic or polemical stance of the argument set out above to the examination of actual data? The best examples I know derive from India. Levinson (1977) documents a systematic hierarchy of differences in the many T/V¹⁶ pronouns used between different social groups (castes in his case). This provides an independent test of Marriott's theory of caste structure. T/V pronouns provide a simple case in which the systematics of usage have sociological correlates, especially in a situation where social groups are as well defined as those in southern India. Looking at the usage of pronouns within the castes Levinson also found variation both in the overall usage patterns between castes and in the way pronouns were used of different kin categories. (He also documented affinal kin terms in a Dravidian kinship system.) A subsequent study in the same ethnographic area by Carter (1984) examines the acquisition of kin terms by small children in the light of Levinson's work. Sadly it, like Levinson's Ph.D., has been little read by anthropologists. Bean (1978) examined kin terms, pronouns, and names in one monograph. However, her analysis does not cover the range of possibilities found in actual usage (her data come from interviews rather than naturally occurring conversation). The rule-based analysis that she presents takes genealogical connection as prior and basic. Since her analysis does not use data from conversation, she does not confront the issue of the choices that speakers make between the uses of the different address terms she analyses.

Gow (1995) talks of people being implicated in a landscape, and of kinship itself as implicated in the landscape with the people. He sees in a landscape traces of the history and kinship of an Amazonian people. I take a similar approach to the way in which language is used: the patterns of language used implicate kinship. In chapter 2 I first consider the address/reference distinction as it is usually understood in anthropology. Against this I suggest that there is reference in all address terms and, just as importantly, there is address in all reference

terms. If the latter proposition is less clear-cut than the former, reflect on how different reference terms may be used depending on who is being addressed. To say that reference terms are 'neutral' is to make a strong empirical claim that such terms are socially acceptable in all contexts, or that it is always possible to use a reference term. However, such claims fly in the face not only of the implications of the English catch-phrase 'who's "she" the cat's mother?' but also of other empirical evidence to the contrary. The world abounds with evidence that people using reference terms are sensitive to their immediate audience. This is usually dismissed with a sweep of the hand by those wishing to maintain the reference/address distinction who without justification assert that those counter-examples are not important. For anthropologists, however, they should be! For example, while an English teenager speaks of 'Mum' to a sibling, they might say 'my mother' to a stranger. Mambila use of proper names is similarly sensitive to the addressee: for example, when referring to a person it is common practice to use one name when speaking to their maternal kin, and another name when speaking to their paternal kin. This is particularly telling when we consider the philosophical debate in which Kripke (1980) argues that proper names are the ultimate referring terms.

One proponent of the use of conversation analysis in anthropology concludes a general discussion with the claim that:

[the] major advantages are: (1) Ethnographic interpretations are grounded in data that can be placed before the reader. The reader is therefore in an improved position to evaluate the author's interpretation. (2) The ethnographer can ground his statements about social structure and organization, or about cultural competence, in actual interaction of his subjects. (3) Although the inevitable need for contextualization can be seen as a disadvantage from the perspective of conversation analysis, for the ethnographer it is an advantage. A stretch of talk may point 'outward' to events or cultural knowledge that would otherwise have remained hidden from or unnoticed by the ethnographer' (Bilmes 1996: 186).

I hope that the following exemplifies those claims.

- 1 Some exceptions are Carter (1984); Fischer (1964); Schebeck (1973); Casson, (1975); Casson and Özertuğ (1976). Others are cited in chapter 2.
2. (1977: 111-34, esp. 124-34). The same criticism applies to 'schemas or scripts': they are too precise and therefore inflexible.
3. This term is generally used for operations whereby one uses existing knowledge or information to develop more powerful routines, which are in turn used in similar fashion so that the system 'lifts itself by its own bootstraps': *Penguin Dictionary of Psychology*, Arthur S. Reber (1995); see general discussion of 'context' in Duranti and Goodwin (eds.) (1992).
4. Social deixis is defined below, p. 6.
5. Although I note that in Australia, where such universal kin terminologies are widely used, names are often un-utterable in any usual social context because of affinal, clan, and mortuary avoidances.
6. For example, Romney (1989); Read and Behrens (1990); White and Jorion (1992).
7. See above.
8. See especially the articles which present an overview of his approach (Read, 2001a, 2001b).
9. And not even on those alone, as I argue below p. 90.
10. Thus they link to the discussion of definite descriptions and proper names: see Russell 1956 and Kripke 1980.
11. The debate between Schegloff (1997; 1999a; 1999b) and Billig (1999a; 1999b) has clarified some of these issues.
12. Exemplified by telephone callers who announce themselves with the phrase 'It's me', relying on their voice being recognised rather than naming themselves.
13. 1975. These are described by Levinson as being 'guidelines for the efficient and effective use of language in conversation to further co-operative ends' (1983: 101). For further discussion and introduction see Haviland (1988).
14. Partridge gives the following definition: 'A catch phrase addressed to a child who refers to his mother, or any other grown-up woman he should respect, merely as 'she': since mid C19th. Sometimes 'And who is "she"? She is...' (1984:1046).
15. For a case study of reference to persons in Mambila folk stories see Perrin (1978).
16. T/V pronouns are the second person singular and plural pronouns (in French 'tu' and 'vous', hence the initials). In many languages throughout the world the plural ('vous' in French) is used, politely, of a singular subject ('tu' in French).

