*CHAPTER 4*

# The Rhetoric of Kinship: 'Doing kinship' – some Mambila cases

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Ethnomethodology and anthropological post-modernism have provoked us to re-examine the anthropological enterprise in strongly contrasting ways. This chapter argues that the rhetoric culture approach (www.rhetoricculture.org) provides a fruitful alternative to anthropological post-modernism and that an ethnomethodological stance provides means to explore rhetoric culture on the ground. Conversation-analytical approaches can help unpack the way different rhetorical tactics are deployed, sometimes as humour, other times as claims to power and influence. Such approaches therefore provide means to explore the rhetoric culture project on the ground. I provide some cases that demonstrate this with reference to kinship, discussing three specific instances from continuing fieldwork with Mambila people in Cameroon.

In their time both ethnomethodology and anthropological post-modernism have been seen as heretical. Ethnomethodology sought to change the course of sociology, claiming that sociology had confused its object with its subject: explanans for explanation. It claimed to analyse precisely the unexamined premises of conventional sociological explanation. For example, how is it that I am able to exchange greetings in English, let alone in Mambila, granted both the uncertainties of the world (what Michael Carrithers terms 'vicissitudes' (2008)) and the

limitless expansion of context necessary to comprehend a single utterance? The solution offered by conversation analysis (which was developed as a part of ethnomethodology), provides pointers to key moments in social action when features of a social system are easier to understand. This provides anthropologists with a valuable method to achieve their goal of understanding the outlines of social process.[1]

It is a truism within economic anthropology that human actors are not the profit-seeking rational maximisers who populate the idealised world of academic economics (see Davis 1992). To acknowledge this does not render the study of economic activity impossible. Perhaps we should follow the shift of emphasis implicit in the last sentence and talk of the study of 'kinship activity' (or the 'processes of kinship' following Carsten, 1997) rather than 'kinship'. A parallel move from 'Kinship' to 'kinship' is a symbolic gesture to accompany that change in emphasis. What Schneider (1984) finds unacceptable in Kinship studies is little more than the danger of (Whitehead's notion of) 'misplaced concretism' which Gregory Bateson already warned against in *Naven,* in 1936 (1980). Bateson was supremely conscious of the distance between the theoretical apparatus and the phenomena he was using it on to understand them. Such consciousness must be constantly maintained, but should not paralyse us. The analytic move, the grasp for understanding, is a particularly human ambition. Moreover, we must recognise that although this is not an exclusively anthropological goal, anthropological analysis attempts to make the conceptual process explicit, to reveal the culture-boundedness of analysis itself. In that final step we see both the ultimate challenge to, and the appeal of, anthropology.

**From Kinship to kinship-processes**

The processual approach, particularly via rhetorical processes, as we shall see, can re-invigorate the study of kinship, freeing it from the doldrums in which it languished since Needham (1971)

and Schneider (1984).[2] Reification or misplaced concretism is one of the bogeys of anthropology and it is largely responsible for the problems here.

*Against Schneider and the tide of fashion*

Schneider argued that kinship as an etic category has no analytic virtue because there are societies with no emic correlates to such a category (1984: 153-5, especially 154). He often cites the set of economics, politics and kinship as false categories precisely because they lack equivalents in many societies. Yet one of the first things that anthropology undergraduates are taught is that such terms have analytic value, though they must be used with caution. Analytical categories are used to separate intertwined aspects of social life. Someone interested in the economics of western societies may well seek cross-cultural comparisons, for example with the economic aspects of Nuer society. That there are no purely economic institutions anywhere does not prevent the study of economic aspects of peoples' life. It is not iconoclastic to suggest that Wall Street, the Bourse or 'the City (of London)' are more than purely economic institutions. Returning to kinship, Schneider argues that an obsession with descent (erroneously bolstered by the discovery of genetics) has been foisted onto an unsuspecting world, much of which has different concerns. His argument, following Bateson's use of Whitehead's term, misplaces concretism. It treats kinship as an object, or at least as an either-or quality whereas I would see it as an aspect of human life which admits of degrees, and which may be defined by indicating some paradigm cases. This is, of course, a Wittgensteinian approach, and Schneider would have disputed my selection of paradigms as being ethnocentric. But I claim them as paradigms only for analysts (I am being analytocentric not ethnocentric).[3] Kinship as an etic category is to be used by anthropologists to approach society, and ultimately to understand the ways in which the members of that society understand and represent themselves to themselves.

Naively,[4] it remains un-deconstructed that every society must eat (economics), manage interpersonal relationships (politics) and reproduce (kinship). People may not be explicitly concerned with where babies come from or how babies are distributed among available breasts and care-givers, but willy-nilly these tasks must be done. Anthropologists tend to be interested in how these work out in practice and what local explanations are given (or withheld) for such actions. One of the ultimate goals of social anthropology is to discover how people understand their world. This goal is just as important if a group professes no interest in an aspect of their life as it is when an elaborate explanation is readily offered. Ironically, it is by focusing on reference terminologies that anthropologists become vulnerable to Schneider's charges of ethnocentrism. If we move from the symbolic 'cultural' level on which he concentrates to social structure and look at the wider picture, his arguments lose their force (see Feinberg 1979, 2001 and Kuper 1999 for variations on this argument).

Having sketched a set of theoretical concerns, I next consider three examples of talk which implicates kinship. These examples do more than that: they provide small examples of how rhetorical praxis is manifested in mundane interaction.

**Introduction to Mambila**

The Mambila lie on either side of the Nigeria/Cameroon border, mostly living on the Mambila Plateau in Nigeria. A smaller number (c. 12,000) are to be found in Cameroon, especially at the foot of the Mambila Plateau escarpment, on the Tikar Plain. My fieldwork has been largely restricted to these latter groups, and in particular to the village of Somié. Somié had a population of approximately one thousand (based on the official 1986 tax census) at the time of original fieldwork. Self-sufficient in food, the villagers have grown coffee as a cash crop since the early 1960s.

Cameroonian Mambila on the Tikar Plain have adopted the Tikar institution of the chiefship, yet their social structure otherwise closely resembles that described for the Nigerian village of Warwar by Rehfisch (1972) based on fieldwork in 1953. At that period Nigerian Mambila did not have the same type of institutionalized chiefship as is found in Cameroon. In Nigeria villages were organised on gerontocratic principles, and largely lacked political offices. The system of exchange marriage described by Rehfisch (1960) has now vanished, and with it the two sorts of named group which recruited through different combinations of descent, marriage type (exchange or bridewealth) and residence. Marriage is viripatrilocal, and is increasingly on the basis of courtship, although bridewealth is still a major factor. However, since bridewealth may be paid in instalments over a number of years it is not cited as a reason for the failure of young men to marry. Zeitlyn 1993[5] provides information about the kinship terminology; Zeitlyn 1994 includes a more general ethnographic introduction.

I am familiar to everyone in the village where I have been a regular visitor since 1985. My strange accent, my obsessive questions and my sound recorders are well known to all but recent arrivals in Somié. In the course of previous research, I have undertaken extensive participant observation focusing initially on traditional religion, and then used census data, genealogies and archival research to examine the demographic history of the village (Zeitlyn and Bagg 2000) as background to a study of family talk (Zeitlyn 2005).

*a) Joking about proper usage: 'He's just like his father'. Names and likeness in Mambila talk.*

This section considers a short extract from a conversation that took place in Somié village in 1993. I hope I can convey some of my pleasure at finding such a wonderful example. It is no small challenge to an ethnographer first to see the jokes at all, and secondly to be able to share them. Among the perennial problems of working with naturally occurring speech is that one is often most interested in some of the relatively rare occurrences of everyday speech. This is, of

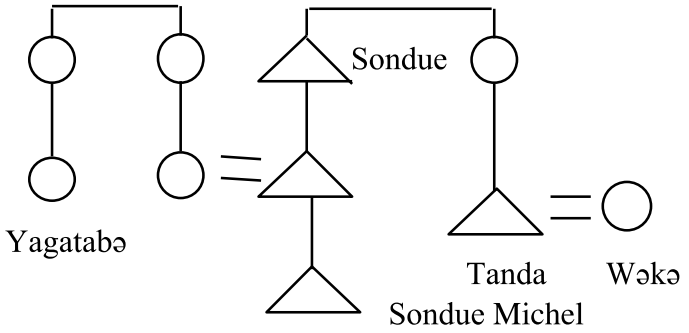
course, a motive for using elicitation frames and other more or less controlled research techniques. Without reliance on systematic sampling, research such as my own which eschews the artificiality of asking questions about the small things that fascinate, for the 'purity' of naturally occurring conversation (in this case in the absence of the ethnographer) can only hope to collect enough data to include some unusual examples. We cast our nets and hope that eventually a different species of fish will be caught.

Example One: Tape 9304b recorded on 26/9/1993

| | |
|---|---|
| Y; Allah, wo [ŋene ko gi | Y; Allah, you [see it all |
| W; Useni ko Monica | W; Please there Monica |
| Y; Hən né ŋuna yəə, ne [nguna Tam | Y; this is your child, it's [Tam's child |
| W; Ne nguna Tam | W; It's Tam's child |
| mə ŋung yɔr man takpuk kpung | with its black shiny skin like so! |
| MS; Emile () wò fela mi wa? {Laughter} | MS: Emile () are you identical to me? {Laughter} |
| W; À we ne ka wò. | W; He's just like you. |
| Koke yəə, à wa, à wa də fada wa. | Look you, he's here, he's here like what? |
| De sa wo na ka də yə ŋuna Sondue ne ki cén. | For this one, you can say the children of Sondue are all one. |
| MS: Hey Sondue ne yili makuú sen. Wò jə ne () Ne jam yə wa? | MS; Hey, Sondue is the name of her *makuú*. You say () Is that good? |
| W; Né bɔ̀ makuú de sa mì lo ko rə ne bu hi wa? | W; Those *makuú* that I know of, where are they? |
| Mə mi se nde a bu yili gogo den. | So I can honour their name. |
| | |
| *Key*: | *Speakers*: |
| [ marks the beginning of overlap. | W =Wəkə Donica |
| Comments are included in {} | MS = Sondue Michel |
| () is a small pause. (.) a slightly longer one. | Y = Yagatabə |

The recording took place at the house of Wəkə Donica, a wife of Nyangi. Previously she had been married to Tanda, a FFZs of Sondue Michel, who was named for Sondue, his paternal grandfather, Wəkə's first husband's MB, or, in other words her *makuú*.[6] So Sondue joked that she should avoid his name just as she would avoid those of her senior affines. He aspired to his grandfather's seniority or status, so Wəkə should not utter his name. Sondue and Wəkə are of approximately the same age, and her marriage to Tanda was long over, so Sondue's claim was, if taken literally, misplaced. The conclusion, which recalls Sperber's account of symbolism (1974), is that this is humour. To share the joke we must understand the network of both genealogy and social relationships, and the rhetorical play which made the interchange funny at the time for those speakers.[7]

Diagram of the relationships involved:




Mambila people say that children may take after either parent or neither. In abstract discussion, boys are said to tend to resemble their fathers, and girls their mothers. Mambila interlocutors do not connect this with either the matrilateral inheritance of witchcraft (*lɔp*) or with accounts of procreation. To say that a child resembles a parent, more than a passing similarity is required: the likeness must be immediately apparent, as is the case with Sondue Michel and his children. There is more in the transcript: Yagatabə (his MMZD) and (following her) Wəkə first refers to

Sondue as Tam. This is his name[8] as used by his maternal kin such as Yagatabə (see the genealogy above), his 'mother's-side' name (to translate the Mambila idiom).

*b) Social norms and comments upon them*

Not all norms are realised in action, but many are. Grice's maxims for efficient communication[9] have long been recognised as examples of norms honoured in the (implicationally rich) breach rather than being explicitly followed. There is a long-standing, unsatisfactory, debate about the status of norms (or social facts) and how they should be recognised or analysed. I wish to avoid direct involvement in the debate on the status of norms although there are clear connections between my data, my analysis of it, and the terms of the debate.

Let us start with the use of names and kin terms. It is a norm of English (both American and British) that parents use personal names to address their children but expect to receive kin terms. In another paper Andrew Wilson and I have shown that in some natural language data from America this is demonstrably the case. Yet the same norms serve to mark as liberal other families in which children use personal names for their parents deliberately and markedly (see Wilson and Zeitlyn 1995; Zeitlyn 2004, 2005). To take another case (although one in which I have no data to analyse), mother-in-law avoidance is often manifested by strict avoidance of the mother-in-law's name (as is the case in the years following marriage in Mambila). This, I imagine, would be reflected in sound recordings of naturally occurring speech made, for example, in Mambila households. There will simply be no (or very few) occurrences in which a junior affine addresses a senior affine by name whereas mothers-in-law will actually address their daughters-in-law by name. I take that simple asymmetry to be, if not a demonstrable 'fact', then at least a contested symptom of a particular configuration of social relations, usually summarised as 'mother-in-law avoidance'. Hence the comments in the transcript, indeed the

possibility of joking about this, attests to Mambila speakers having concepts of stable norms and it is these with which I am concerned.

Example Two: the presentation of genealogies. Sample data -Talking about the arrival of missionaries: T9306A #335 recorded on 15 Oct 1993. For text and translation, see online appendix at www.mambila.info/rhetoric_appendix.html).

| | |
|---|---|
| DM; Né ŋuna mò fə to to. Bô Kolaka. | DM; He was my very first child. With Kolaka. |
| Aa sá bí kɔ́, bí na hi wa? | But then you were nowhere, where were you? |
| Aa sá bí kɔ́, bí na hi wa? | But then you were nowhere, where were you? |
| Weeheee, sâ né fə huan mò to. | Weehee, that was my first child. |

In the conversation from which these extracts are taken Sondue and Diko were talking about the past, because Diko was one of the senior people in the village, respected widely for her knowledge. They were doing this with an eye on the tape recorder - which in this case was a literal metonym for the researcher. As the first wife of the Chief Konaka (who was chief until his death late in 1949) Diko was at the centre of ritual organisation and village politics for at least the last 60 years until a little before her death late in 2004. The recording was made after I had asked Sondue to talk to Diko to follow up some conversations I had had with her, without the constraints that my linguistic inabilities put on the conversation. The short extracts given here demonstrate the way in which the genealogy is given. The first extract starts with a throwaway comment ‘my son Ndi’, which Sondue picks up - either because of his ignorance or as useful lead for the historical researcher. As Diko thought about it, she realised Sondue was too young to have known Ndi as an adult (Ndi died in a road accident in 1953). Hence, ‘Where were you?’

(the implied answer is ‘nowhere’). So she explained that Ndi was her first child and that he was followed by another child called Menandi after an earlier chief who had died shortly before his birth. When that son died, her next was given the same name (it was the second Menandi who was then living in the city of Banyo).

There are two further points to note: first, the genealogy she gave does not mention any of her daughters (one of whom also lives in Banyo!) Second, the interactivity of the exchange must not be overlooked. Sondue makes his own contribution - identifying Kuŋ (one of Diko’s sons, already long dead at the time of recording) via his surviving widow, and volunteering that he, Sondue, had seen Kuŋ when he was a child. Few anthropologists working outside of their own community are in a position to make this sort of contribution, let alone while pursuing the ‘genealogical method’.

Elsewhere in the same transcript there is a long account of the sequence of gifts and work that ideally accrued during the years of betrothal in the early twentieth century, and the different quantities of oil, beer, and chickens that were given to the bride’s parents. If the examples given above are too linguistic for some readers’ taste, then in the arrangement of marriage we find action - the prospective husband farmed the fields of his parents-in-law; working in the fields of both his fiancée’s parents. He and his family laboured to provide the palm-oil and beer given in large quantities to their affines. So, too, parents work to raise their children, and benefit from their children’s work. These may be truisms, but they bear repetition in the face of the facile statement that kinship does not exist (Schneider 1984 and Needham 1971). It is not just that an individual may benefit from someone else’s labour, as Diko’s mother’s sister Lè did from Diko’s. It is that it was seen as fit and proper that Diko’s mother should send Diko to live with Lè (just as in contemporary Somié one girl has lived for the last decade with her mother’s full sister who has only one son). As Esther Goody (1973 and 1982) has so clearly demonstrated, fostering provides a clear demonstration that the complex set of ideas which constitute kinship

are an essential part of human ideology and sociality, precisely by removing some of the 'normal' correlates of kinship.

This manner of phrasing allows for 'family resemblances' of different local understandings of kinship, rather than identifying any putatively necessary or essential defining features. I do not accept that it is therefore a less fitting analytic concept. Similarly, genetic engineering changes some variables revealing our unstated assumptions about 'kinship at the core' (q.v. Strathern 1981 and 1992, Edwards 1999).

*c) Clarity and confusion when talking about Mambila genealogy*

In this section I describe a short piece of conversation about local history in which a genealogy became an issue in the talk of a group of Mambila men. The implications of the rhetoric deployed, of the style of argument, are discussed.

**An incident during fieldwork. Background**

A Saturday afternoon, market day in May 1998. People were wandering about, moving from beer drink to beer drink. Some were at meetings of rotating credit societies, others in small shelters in the market place. A group gathered in the palace square at the centre of the village. Unexceptionally, some elders were conversing: a few passers stopped to listen. That the anthropologist was among the passers-by was also a commonplace.

*Fieldnotes 981/26*

> Mama Simon was standing in middle of a circle of men - on the edge of the palace square. I was talking to Baba Joel some distance away - I realised - half-overhearing that Mama was lecturing them about something. I asked BJ who said it was about the past so I

got out my minidisk recorder and we approached the circle.

Because of our arrival it got a bit more genealogical than it had been but it was certainly about kin to begin with.

But BJ started throwing his weight about and Wg wouldn't have it. NB all but espy Wg were drunk. Speakers: Mama Simon, Baba J, Wg and Nɔ Mark at the end. Silent were Nyakati, Samuel (Ŋgwa's son) and 3 others - a tight little circle.

Elaboration written on the following Monday:

6.30 pm Saturday 2 May 1998. End of a long hot Saturday. Market day in Somié. It is early in the rainy season; the rain often threatens but does not fall. It is the time of maximum work in the fields so market day is eagerly looked forward to, and people take every opportunity to relax with the locally brewed maize beer that is distributed either for sale or at meetings of rotating credit societies. By the end of the afternoon on most market days many people in the village are feeling the effects of alcohol to a greater or lesser extent. I emerge for a stroll, trying to slip into the bar for my daily bottle of beer without exciting too much comment. On the edge of the Palace square I bump into BJ who had failed to come and see me as promised that afternoon. He apologises loudly, effusively, a little too much. He asks if we should go and talk now, I say 'no tomorrow will be fine' (thinking that he will be sober in the morning). I notice a group of men standing not far off clustered around Mama Simon who is lecturing them about something. I ask, what is he talking about and BJ says something in the past so I get out my recorder and go and join them, BJ follows. Mama Simon appears to have been outlining someone's family history. He carries on despite our arrival but BJ (who is knowledgeable about village genealogies and has conducted several village censuses for me) was moved to intervene. This leads to a heated discussion about the sequence of children and the way that marriage was organised. Wg who is perhaps the most drunk of

all those present argued the most loudly, and before I turn off the recorder retreats with someone else to voice his disagreement. At this point most of the audience who had been silently standing around chose to leave, so I leave with them.

The transcript and translation are included as an online appendix at www.mambila.info/rhetoric_appendix.html).

**Analytical Remarks**

The first and perhaps the most important thing to note is the importance of genealogical connection in this conversation. Relationships are described in terms of who bore whom. It should also be noted that in this sense men bear their children. Does that statement presume the importance of genealogical connection? I think not. The main sense of the word *ŋar* is to give birth. A secondary sense is its application to men. Genealogies are described as sequences of 'bearings' e.g.:

| | | | | | | |
|---|---|---|---|---|---|---|
| | À | ŋar | () Tomokeh, | à | ŋar | Ɔgeaá |
| | She | bore | () Tomokeh, | she | bore | Ɔgeaá |

This conversation reveals several features of Mambila kinship. Perhaps the most basic is the relative unimportance of genealogies in Mambila social structure. It is not important to maintain them and hence they are not issues of great political dispute as is the case in unilineal societies such as the Lugbara (Middleton 1960). Without pressure to maintain a genealogy, it becomes a matter of casual dispute, as in the conversation here considered. No one has bothered; ultimately no one really is concerned about who married whom or who gave birth to whom two generations ago. Nothing follows from either possibility. Patterns of descent have no major social implications. So genealogies are maintained in a haphazard fashion, and individuals who meet

an unfortunate but memorable end, such as falling from a palm tree, are more easily placed. However, as the conversation demonstrates, people tend to remember that someone fell from a palm tree, rather than exactly whose son they were.

However, the discussion turned into a clash of wills between several of the speakers (I suspect encouraged by my presence). Issues of authority and influence are created, demonstrated, renewed and developed in small, individually trivial instances such as these. Especially in societies without formal hierarchies of offices, rhetorical achievement through argument and interaction is all there is; there are no titles or positions to compete for (see Zeitlyn 1994 and 1992). There are no clear winners and there need be none. The participants leave with the sense of one person having prevailed over another as a rhetorical accomplishment. This contributes to their next interactions with those parties, and hence slowly, incrementally, reputations are won and lost. It is through such incremental interactions that social status is emergent (cf. Girke and Meyer 2011: 2).

**A further confusion: accounts and the fixity of the past**

Accounts of actions (and meanings) presented another problem. A misleading impression is given of the fixity of the past.[10] Indigenous accounts, our own among them, give a misleading impression of certainty and concreteness. Past actions are described as if they were unambiguous, and planned with a single specifiable goal in mind. Such retrospective definition or post-hoc rationalisation typifies the human condition. But because of this, accounts of actions must not be taken at face value. Retrospective accounts are as much a part of the problem of human action as the actions they refer to, and sociological accounts are no different. Suchman argues (1987) that sociology mistakenly aspires to produce 'scientific' versions of indigenous accounts. Yet the very terms of these accounts form some of the most interesting and most

challenging aspects of human sociality and should therefore be examined rather than taken for granted and used in explanations.[11]

Just as the past is made and comes to be fixed, for the time being, so meaning is *emergent* from interaction; it is produced as a joint (social) accomplishment by the ensemble of the speakers and others present. It is a rhetorical accomplishment (Strecker and Tyler 2009: 3) and emphatically *not* a solitary intention, located in an individual's head, however loudly the western philosophical tradition asserts (or assumes) that it is (see Clark 1996 for a systematic rebuttal of the traditional view). Ethnomethodology can provide great assistance for those studying how humans make meaning. For example, the notion of 'repairs' is central to the ethnomethodological research programme: in normal practice meaning is assumed, presumed, taken for granted. We work on that basis until we see reason to doubt. A breakdown in conversation can lead to our addressing each other's understanding. It is at such points that meaning is explicitly negotiated between co-conversants. From the conversational steps taken to effect repairs following a breakdown in the smooth flow of conversation, those involved acquire further grounds for their assumptions, which might not be questioned at the next conversational round. Those grounds are, in practice, adequate for the task at hand.[12] Meaning is emergent from the morass of social activity, and may be examined most effectively when problems (of whatever sort) occur. So anthropologists really are, or should be, looking for trouble (see Moerman 1988: 51-3 and Strecker and Tyler 2009: 5). Not just at the extreme end of 'What do you mean?' but where disputes arise about the correct course of a ritual, or about who should inherit a field or cow. Trouble spots are points at which meaning is at issue. The resolution of the problem results in some sort of (more or less temporary) consensus: even if it is nothing more than an agreement to disagree.

This general perspective is one in which meaning is an emergent property[13] of social interaction. On this view it is located not in our heads, but in socially constructed space: in the

interactions between social actors. This is a triumph of sociology (in the widest sense of the term) over philosophy. It is a triumph of the Rhetoric Culture project. More importantly, it serves to orient our research practice towards troublesome but mundane interaction. This is more or less where it has always been, but now we can see why it has been so productive. The key term here, along with 'process' is 'emergent' (see Clark 1996: 22-3 for connections with un-intended consequences, and Sawyer 2001, 2002 for parallels with the philosophy of mind). Emergent properties are not consciously strived for and they are often not available to the conscious reflection of the actors concerned yet they are important features of their lives.

Consider Gilbert Lewis's (1986) transcript of the construction of the Gnau (in Papua New Guinea) *Panu'et* ritual: there is dissent; conflicting voices give substance to our understanding. The dispute sets limits for the possible local interpretations of the case in point (as illustrated over the *longue durée* by Bloch's examination of Merina circumcision ritual in his *From Blessing to Violence* (1986)). Moreover, it provides a public demonstration of the resolution that was achieved. Life goes on and the talk that is part of it goes on too. There is no time to stop the bus for straw philosophers to catch up.

**Conclusions: studying process**

If kinship is an emergent aspect of human life, then it is part of the process of social existence. A processual anthropology has been announced more times than it has been achieved. A justly celebrated example is the work of Janet Carsten for whom 'kinship is a process of becoming, not a fixed state' (1997: 12). She continues to say that:

> the approach I have adopted has also involved rethinking what anthropologists mean by kinship. I have tried to capture the texture of relations (22/23) between those who live together, and to show how kinship, far from being an analytic abstraction, is part of the fabric of people's lives. Kinship is not a lifeless and pre-

> given force which in some mysterious way determines the form of people's relations with each other. On the contrary, it consists of the many small actions, exchanges, friendships and enmities that peoples themselves create in their everyday lives. For most people it is perhaps the heart of their creativity. But the content of these relations is not only continuously created anew; it is also shaped by long-term political processes. And this has also involved rethinking what kinship is - from a different angle.
>
> The kinship I describe here cannot be separated off from politics or economics or history but is embedded in them. The meanings of kinship cannot be separately analysed. This of course is another truism - this time of anthropology - but it is more often avowed than actually demonstrated. (1997: 22/3)

Some of the methods I have described above provide means by which we can make a long, cool, systematic examination of the processes of kinship. A crucial change is in the primary data to be analysed. Rather than rely exclusively on my own experiences mediated through fieldnotes, bolstered by conversations with informants and the results of questionnaires (all of which I have used more or less exhaustively) the core data analysed are transcripts of recordings of everyday conversations. Neither the presentation of the data in a manner meaningful to non-local readers nor the production and subsequent analysis of the transcripts is straightforward. Here I have attempted both. The study of kinship as it *emerges* from people's practice (which includes their conversation) is as important to twenty-first century anthropology as Rivers' genealogical method made it central at the beginning of the twentieth century.

The theoretical and analytical problem now posed is one of how to use conversational data as we proceed with the analysis. In principle we can infer intentions in manner similar to that of co-conversationalists. We, just as participants, can detect snubs, irony, politeness or its

absence. The problem is to systematise and to make explicit the basis upon which we do this (the debate between Schegloff (1997, 1999a, b, c) and Billig (1999a, b) has clarified some of these issues).

Anthropological theory seeks to produce concepts which help us to understand of societies around the world, particularly when viewed in a comparative frame. Kinship is one of the analytic terms of art that has been helpful and yet frustrating. The approach I have employed encourages the appreciation of meaning as an emergent phenomenon, and critically for this volume, one *emerging from mundane rhetorical practice*.[14] The meaning of words and of actions (whether ritual or not) results from the use of the words and the social context within which this speech event occurs. This poses problems for the analyst which do not arise for the actors. Only analysts are concerned to seek decontextualised, invariant semiotics - and so much the worse for them. Even self-analysis is impossible if that is the goal. Rather than aim for such impossible abstractions, I have set myself a different task which is, if not impossible then problematic for different reasons. As an analyst I present an account by which others can understand (however imperfectly) how Mambila actors understand the world. Moreover, I seek to convey this in a way which will facilitate comparison. A tall order, which is of course vulnerable to diverse post-modern challenges: what right have I to say this is how it is? My English words do not carry the same resonances as the original Mambila words let alone the quiditity of Mambila being, and so on. These are absolutist challenges that only engage if I claim to be saying the last word about Mambila life and experience. I am not that naive. However, I do not believe that it is too naive to claim that I have some understanding (flawed, partial, incomplete as it may be) of Mambila life as I have seen it lived over the last thirty years, and since other anthropologists know less about Mambila there is something to convey in my rhetoric about their rhetoric.

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Early drafts of this paper have been read by Mike Fischer and Bill Watson who have, in different ways, helped me to clarify what I was trying to argue, as has more recent discussion with Peter Parkes. I am very grateful to all for their trouble. Anna Rayne's editing has contributed to the clarity of presentation and she has suffered far more than just my prose style, for which she is owed my perpetual thanks. In revising this chapter for publication, I have sought to leave it in the spirit of the original conference contribution while situating it more clearly within the overall Rhetoric Culture project.

**Notes**

1. I am hardly the first to take this approach. As well as the contributors to *The Ethnography of Communication* (Gumperz and Hymes (eds) 1986) other anthropologists have used ideas taken from conversation analysis. We must acknowledge, among others, Goldman's analysis of Huli disputes (1983), Sherzer's work on the Kuna (1983), the work of the Tedlocks (B. 1982 and D. 1983), Moerman's work on Thai conversation and social structure (1988), Bilmes (1993) also on Thai, Hanks (1990) on Maya deixis and Duranti on Samoan village politics (1994).
2. For other approaches to the central importance of kinship, see e.g. Strathern 1992, Bouquet 1993 and Carsten 1997.
3. Training as an anthropologist alienates one from one's fellows if they lack a similar experience of academe. *Pace* Fabian, it is we who are 'other.' Anthropologists, whether indigenous or not, use a meta-language to write about 'their' society. That is

the point of the subject. Even if we use a local language as meta-language it will be foreign and peculiar to the 'subjects'. The best parallel is with linguistics, not imperialism. To write about experience (I am thinking particularly of the organisation of turn-taking in conversation) is to do strange things to that experience. If we think about it we cannot do it. We should recognize this. Similar kinds of understanding (but unformulated) lie beneath our actions.

4. I risk being accused of reductionism. Without some such basic understanding we are unable to begin to understand how other people conceive of the world in which we all live (see Gellner 1975 on radical translation and Zeitlyn and Just 2014).
5. A short transcript from that paper and a digitised audio recording are available from the Virtual Institute of Mambila Studies via the following URL: http://www.mambila.info/ 1993paper/mambila.html.
6. Women use *makuú* for a husband's elder siblings and his parents. See Zeitlyn 2005 for Mambila kinship terminology.
7. 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.
8. The complexity of Mambila naming is further discussed in Zeitlyn 2005.
9. These are described by Levinson (1983: 101) as being 'guidelines for the efficient and effective use of language in conversation to further co-operative ends'. For further discussion and introduction, see Haviland 1988.
10. I am grateful for Lucy Suchman (1987: 47, 50-1) for clarifying much of this; see also Becker 1995. Symmetries between past and future are explored in Zeitlyn 2015.
11. See Latour 1996 and review by Zeitlyn 1997 for a technological example of this.
12. That this is far from philosophical adequacy remains a problem for philosophers alone.

13. Some would go so far as to say an epiphenomena, but I would not agree.
14. As a referee to this paper points out an implication of this approach is that all such kinship practices are rhetoric (or rhetorical in some sense). Indeed I assume they are all meaningful and amenable to a rhetorical analysis, as I have undertaken with my examples in this chapter, since they are amenable to repairs.

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