

Kinship Terminology As A Deictic System.
An Evaluation Of Ethnolinguistic Methodology.

A Thesis Submitted
For The Degree Of
Doctor of Philosophy

by

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A B S T R A C T

This work presents a critical examination of the methodology of the analysis of kinship terms over the last 100 years, gives a global evaluation of the 'state of the art', and proposes a new, deictic interpretation of kinship terminology.

The methodological survey is divided into three sections devoted to: the historical approach (section 2), which explains kinship terminology in terms of historical causes; the structural/functional approach (section 3), which relates it to present structures or functions; and the formal approach (section 4), which concentrates on the internal structure of sets of kinship terms. For the first two of these sections a number of representative authors are chosen and their views on kinship terminology are examined. The formal approach and the discussions connected with it are presented in the form of a survey of the study of English kinship terminology.

The global evaluation (section 5) assesses the major issues arising from each of these approaches. In conclusion a new proposal is formulated for approaching kinship terminology as a deictic system.

In a final section (section 6) this proposal is elaborated theoretically, and applied concretely to Dutch kinship terms.

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1. Introduction.

1.1 My interest in kinship terminology came about by accident. In teaching textual interpretation of the Bible to future missionaries I was forced to reflect more particularly upon those aspects of the text which were culture-specific, with a view to discussing their translatability. In searching for ways of dealing with these problems I discovered the theories of linguistic relativity, particularly in the work of E. Sapir and B.L. Whorf, and German thinking on the concept of "Weltbild" with its roots in W. von Humboldt's ideas.

 Though the claims made by these theories were fascinating, the actual discussion of particular cases did not yet seem very convincing. One would expect the facts to be more difficult with regard to grammatical aspects of language but easier in the lexical domain. Concentrating then on the lexicon I found little enlightenment in the fact that Eskimos have many more words for snow than the English, that Bedouin Arabs have an abundance of words for camels, etc. My attention was drawn to kinship terminology as an ideal example, a particularly instructive case of correlation between language and culture (cf. e.g. Porzig, 1950: 118). This seemed to be a well documented case, studied intensively for a whole century. Yet, the more I studied the available accounts, and the deeper I went into the different methodological claims, the more confusing it became.

I did not doubt that it was an ideal test case, but I was puzzled by the inconclusive outcome of so much effort. As a result I began to wonder whether there was perhaps no satisfactory answer possible, or whether the wrong question was being asked.

I then undertook to examine the question of the social relevance of kinship terminology systematically. I decided that the best way of doing this would be to examine first the different methodological approaches and establish exactly what had been done and against which background to place this. I would then evaluate the major lines of research in the hope of bringing out clearly both the achievements and the points which would seem to block further progress. And I hoped that looking at the results of that research I might be able to propose a way out of the problem, in line with recent developments in linguistic thinking.

1.2 It seems appropriate to explain in greater detail how I set about this task. The study of kinship and kinship terminology has always been central to the interests of anthropologists. This makes it inevitable that we should pay attention to a fair amount of anthropological literature relating to our subject. Moreover, it would hardly seem realistic to study the possible relations between language and culture without trying to acquire at least a basic knowledge of the social and cultural aspects of one's subject. I take the view that we need an interdisciplinary orientation in this kind of study. I do not deny that each of the disciplines involved does exist in

its own right: however, unless we keep in touch with other disciplines concerned in the same subject it will be very difficult to integrate the different points of view at some later stage.

I shall first discuss extensively some of the major contributions to the study of kinship terminology. I believe that it is essential to do this at considerable length for the simple reason that we have to know the relevant facts in order to be able to formulate a good diagnosis. I do not know of any study which has examined this century of studies on kinship terminology from a primarily linguistic point of view: the first aim I have set myself is to provide such an examination. I have not been content to look simply at what scholars did when analysing kin terms - I have also tried to understand why they approached their subject the way they did. A proper evaluation cannot ignore such background information. At times this may seem to make the discussion a bit long, and not every aspect of the different approaches will have a bearing on our final evaluation and diagnosis. Both respect for the scholars concerned and my desire for presenting a verifiable account rather than an overall impression made it necessary, however, in my opinion.

This decision brought certain difficulties with it. On the one hand I wanted to present all the facts before giving a global evaluation. On the other hand I foresaw that many details of the work of different scholars or schools of thought would not come up specifically in such a global evaluation which aimed at diagnosing the 'state of

the art' - so that a wrong impression of uncritical acceptance could be given by ignoring some of these points altogether. I have tried to minimize as much as possible this disadvantage of my approach by placing a few critical remarks or question marks at the end of each section, not by way of a fully fledged criticism of a particular theory or method, but as preliminary remarks before the more global evaluation. I accept this difficulty as a minor evil, justified by the positive, indeed essential value of an extensive research into, and presentation of the facts.

The next point I had to consider was what to include in my survey of the study of kinship terms; for the amount of case-studies and theoretical publications is such that full coverage is humanly impossible. A way out of this dilemma would possibly have been a thematic approach, a discussion of some major issues and the way they had been dealt with by different, representative authors. I did not choose this way because there is a double element of uncertainty built in, viz. how to determine which issues are major ones, and who are representative authors for each one of them. Instead I decided to concentrate upon a selected number of scholars and look at their work without prejudice as to what are major issues in their study of kin terms. Some repetition is unavoidable when one considers the work of several scholars in succession. I decided, however, that this was not too high a price to pay for as fair and as verifiable an account as possible.

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The difficulty of making a good, representative selection of authors still remained, of course. I solved this by accepting the view that one can distinguish three phases in the history of the study of kinship terms, a view which is presented, for example, by J. Goody (1971). One distinguishes a historical approach, followed by a period in which structural or functional approaches were dominant, and finally a formal type of analysis which studies kin terms as a well-defined set in its internal coherence and organizing principles.

For the historical approach I selected L.H. Morgan, the founding father of the subject; J.F. McLennan, who was a contemporary critic of Morgan's; F. Boas, because of his influence on a whole generation of ethnolinguists and anthropologists; E. Sapir, because of his views on linguistic relativity as well as kinship terminology; A.L. Kroeber, because he was the first to point to psychological rather than sociological factors in the explanation of kin term systems, and even more because he laid the foundation for what was to become feature analysis of kin terms; W.H.R. Rivers, because of his influence on anthropologists on this side of the Atlantic, and because of his genealogical method; and finally I included a section on historical comparative linguistics and the study of kinship terms since this still remains an acceptable and respected branch of the field.

For the structural/functional approaches I selected B.K. Malinowski as a representative of a typically functional approach, with a predominantly psychological interest; A.R. Radcliffe-Brown for his strictly structural

and social approach; K. Davis and W. Lloyd Warner, because of their clear discussion of the genealogical basis of kinship, and because they are the fore-runners of modern formal analysis; G.P. Murdock, because of his cross-cultural interest and his method of establishing statistical correlations between features of terminological and of social organizational systems; C. Lévi-Strauss, because of his revolutionary challenge to many traditional points of view; and finally L. Weisgerber, as a representative of Humboldt's heritage.

The formal approach presented a special problem. It is not different from the previous phase, in that a formal analysis works on the assumption of the structured nature of the set of kinship terms. It is more a matter of emphasis: less attention is devoted to external factors in explaining the structure of a set of kin terms; instead one tends to concentrate on the internal structure of a set. A complicating factor is the greatly increased number of practitioners of kin term analysis, not least among linguists who become increasingly more interested in semantic questions. The style of study changes too: we no longer find so much the comprehensive type of study which was possible in earlier days. Types of analysis proliferate, and particular analyses increase in number; issues of methodology are under continuous scrutiny in all this work, not so much in global expositions, but rather in discussions of specific points. In order to bring some unity to these latest developments I have chosen to present a near complete survey of the study of English kinship terminology. Apart from the more anthropologically

orientated work of Talcott Parsons and D.M. Schneider, we find here a full picture of all types of formal analysis and the main issues discussed in connection with these developments. This part will be particularly concerned to bring together all these analyses and discussions: I feel that this is of value in itself and justifies its length. Inevitably there is some repetition here and there. I hope, however, to have been successful in keeping it to a minimum.

After completing the survey of the methodologies used in studying kinship terminology I give a global assessment of the major issues which have arisen in each of the three types of approach. In the light of what appear to be the remaining problems concerning the social relevance of kin terms, we shall try to reconsider the "status quaestionis". This will lead us to the formulation of a more modest proposal on the relation between language and culture in the field of kinship terminology, a proposal which will place the issue among the phenomena of deixis within linguistic theory.

In a final chapter I elaborate on the latter theoretical viewpoint, and show by analysing kin term usage in my native Dutch how this new insight might result concretely in a more flexible view of the social relevance of a particular part of vocabulary.

2. The Historical Approach

2.1 Lewis H. Morgan.

2.1.1 Lewis H. Morgan is a typical representative of the 19th century: his orientation in his scientific work is clearly historical and evolutionary. In his summary of the results of his major work, Systems of Consanguinity and Affinity of the Human Family (1871), we find such statements as the following:

"It remains to notice the order of origination of these customs and institutions as a great progressive series founded upon the growth of man's experience. ... The establishment of this series as a means of recovering the thread of man's history through the primitive ages is the principal result of this solution of the origin of the classificatory system". (1871: 487).

What started off as an interesting discovery - the peculiar and seemingly unique kinship system of the Iroquois - very soon appeared to be a very common system, not just in the other Algonkin speaking tribes of North America, but right across the Indian population of that area. Once he had established that in all probability this type of kinship system was universally distributed throughout the Indian Family, he began to extend his enquiries to the rest of the world. In all this he was not free from the prejudices of the 19th century, assuming without proof or justification a monogenetic origin of mankind, and considering non-Western forms of culture as inferior and primitive:

"Mankind, if one in origin, must have become subdivided at a very early period

into independent nations. Unequal progress has been made by their descendants from that day to the present; some of them still remaining in a condition not far removed from the primitive, and now revealing many of the intervening stages of progress" (1871: 479).

His ideas are clearly evolutionary, assuming a scale of growing perfection leading up to our Western kinship system, and he tried to bring some order into this development by drawing up a classification of types of relationship systems. In this way he wanted to continue the work of classification which philology had begun (1871: V). Once Morgan saw how common the Iroquois type of relationship system was among the Indians of North America, he began to work along the lines of the following hypothesis:

- This system is prevalent among all the American aborigines.
- If so, the system must be coeval, in point of time, with the commencement of their dispersion over the American continent, and - as a system transmitted with the blood - it might establish their unity of origin.
- It could then be assumed that they brought the system with them. Assuming furthermore that they came from Asia, finding the system in existence there would possibly furnish some evidence towards the Asiatic origin of the American Indians (cf. 1871: 4).

After completing as thorough an inventory as possible of relationship systems from all over the world, he concluded that there were only two radically distinct systems: he named these the descriptive and classificatory systems (cf. 1871: 468 ff.).

"In the descriptive system consanguinei are, in the main, described by a combination of the primary terms of relationship, the collateral lines are maintained distinct and divergent from the lineal, and the few special terms employed are restricted to particular persons, and to those nearest in degree".

This is the form found in the "Aryan, Semitic, and Uralian families"; it means (to take the example of English) that uncle and aunt are terminologically distinguished from father and mother, cousin from brother or sister, and nephew and niece from son and daughter.

"Under the classificatory system consanguinei are not described by a combination of primary terms, but each and all, however remote in degree, fall under some one of the recognized relationships. By comprehensive, as well as apparently arbitrary, generalizations they are reduced to great classes or categories, the members of each of which, irrespective of nearness or remoteness in degree, are placed upon the same level, and admitted into the same relationship".

What this means concretely is shown by Morgan's Seneca data, here quoted in the grouping made by Lounsbury (1964 a: 195 f.):

<u>Hakso:t</u> , "my grandfather"	FF, MF; FFB, FMB, MFB, MMB FFFBs, etc.; <u>also</u> FFF, MMF, etc.
<u>akso:t</u> , "my grandmother"	FM, MM; FFS, FMS, MFS, MMS; FFFBd, etc.; <u>also</u> FFM, MMM, etc.
<u>ha?nih</u> , "my father"	F; FB; FMSs, FFBs, FMBs, FFSs; FFFBss, etc.
<u>no?yẽh</u> , "my mother"	M; MS; MMSd, MFBd, MMBd, MFSd, MMMSdd, etc.
<u>hakhno?sẽh</u> , "my uncle"	MB; MMSs, MFBs, MMBs, MFSs; MMMSds, etc.
<u>ake:hak</u> , "my aunt"	FS; FMSd, FFBd, FMBd, FFSd; FFFBsd, etc.

hahtsi?, "my elder brother" B; MSs, FBs; MMSds, FFBss,
 MFBds, FMSss, MMBds, FFSss,
 MFSds, FMBss; MMMSdds, etc.,
when older than Ego.

he?kē:?, "my younger brother" Same, when younger than Ego.

ahtsi?, "my elder sister" S; MSd, FBd; MMSdd, FFBsd,
 MFBdd, FMSsd, MMBdd, FFSsd,
 MFSdd, FMBsd; MMMSddd, etc.,
when older than Ego.

khe?kē:?, "my younger sister" Same, when younger than Ego.

akyä:se:?, "my cousin" MBs, FSs; MMSss, FFBds, MFBss,
 FMSds, MMBss, FFSds, MFSss,
 FMBds; MMMSdss, etc.; and MBd,
 FSd; MMSsd, FFBdd, MFBsd,
 FMSdd, MMBsd, FFSdd, MFSsd,
 FMBdd; MMMSdsd, etc.

he:awak, "my son" s; Bs; MSss, FBss, MBss, FSss;
 MMSdss, etc., of a man; but:
 s; Ss; MSds, FBds, MBds, FSds;
 MMSdds, etc., of a woman.

khe:awak, "my daughter" d; Bd; MSsd, FBsd, MBsd, FSsd;
 MMSdsd, etc., of a man; but:
 d; Sd; MSdd, FBdd, MBdd, FSdd;
 MMSddd, etc., of a woman.

heyē:wō:tē?, "my nephew" Ss; MSds, FBds, MBds, FSds;
 MMSdds, etc., of a man.

hehsō?neh, "my nephew" Bs; MSss, FBss, MBss, FSss;
 MMSdss, etc., of a woman.

kheyē:wō:tē?, "my niece" Sd; MSdd, FBdd, MBdd, FSdd;
 MMSddd, etc., of a man.

khehsō?neh, "my niece" Bd; MSsd, FBsd, MBsd, FSsd;
MMSdsd, etc., of a woman.
heya:te?, "my grandson" ss, ds; Bss, Bds, Sss, Sds;
FBsss, etc.; Also sss, dds, etc.
kheya:te?, "my granddaughter" sd, dd; Bsd, Bdd, Ssd, Sdd;
FBssd, etc.; also ssd, ddd,
etc. (1)

Morgan's own tabulation is based on descent lines, the crucial point in his definition of a classificatory system. He distinguishes six groups: the lineal line, four collateral lines, and marriage relatives. The Seneca word labelled "grandfather" (hoc'-sote) in Morgan's transcription) thus appears four times in his list (1871: 167-169):

- in the lineal line as: FaFaFaFa, FaFaFa, FaFa.
- in the third collateral line as: FaFaBr, MoMoBr.
- in the fourth collateral line as: FaFaFaBr, FaFaFaBrSo, MoMoMoBr, MoMoMoBrSo.
- among the marriage relatives as: WiFaFa.

Having established what exactly constitutes a classificatory system, and having established equally that this system is found all over North America, Morgan goes on to prove his second point: the unity of origin. Four explanations are

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1. Since Morgan it is customary to reckon kinship relations from an Ego since "each person is the centre around whom a group of consanguineal is arranged" (op.cit.: 15). The abbreviations used to indicate relatives vary among authors, but basically they are the first letters of the English kinship terms. The difficult point is the distinction between "sister" and "son"; ways of solving this are:

S vs. s (so in Lounsbury)
Z vs. S
Si vs. So.

I adopt the last convention but when quoting from authors I shall do so in the convention they use.

possible for the similarity of system (op.cit.: 495 f., and 500 ff.):

- by borrowing from each other: but domestic institutions are not propagated by borrowing.
- by accidental invention in disconnected areas: but as there are only two major types - descriptive and classificatory - chance cannot possibly be a sufficient explanation.
- by spontaneous growth in disconnected areas under the influence of suggestions springing from similar wants in similar conditions of society. Here Morgan begs the question, he rejects this possibility as difficult to reconcile with the evolutionary sequence which is part of his final conclusion.
- by transmission with the blood from a common original source. Assuming that there are no other possibilities but these four, and accepting that the other three are unacceptable as explanations (all of which Morgan does assume), this fourth solution is a foregone conclusion.

Kinship terminology plays a crucial part in Morgan's reasoning. He assumes that at some time, when a system of relationship is initially developed, a terminology is adopted which fits in with the social system. Moreover he assumes that "once [a system] had come into practical use, with its nomenclature adopted, and its method of description or classification settled, it would, from the nature of the case, be very slow to change" (op.cit.: 15): in other words, the form of a set of terms may change

with time, but its meaning i.e. its descriptive or classificatory structure, will remain pretty well stable. Of course, Morgan was not interested primarily in language, he wanted to explain the different systems of relationship embodied in sets of kin terms, he was interested in reconstructing history, the path of evolution, and he thought he could do so from the kinship terminologies and at the same time explain the variations in kinship terminology that we now find (cf. Tax, 1955: 459). This is what Tax calls Morgan's circular argument (ibid., p.457): that kinship terms can be used to reconstruct history because they remain constant, that they must remain constant because they are structurally the same among people who once were one and who have otherwise changed, and that these people once must have been one because they have the same kinship system.

2.1.2 I would say that any argument of a hypothetical nature like the one Morgan develops at great length, can easily look circular. The test as to its validity would depend on two things: whether it is carried through to its proper conclusion by verification against established facts, and whether the implicit assumptions, present in most arguments, are not positively wrong.

The verification of Morgan's thesis has to be done by anthropologists as his main thesis is anthropological. While there is much in his work of lasting value, his basic evolutionist theory does not stand up to the confrontation with the facts: "On the whole, the ethnographic evidence fails signally to support the evolutionist contention that the matrilineate is primitive, the

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patrilineate intermediate, and bilateral descent associated with higher civilization" (Murdock, 1949: 186-187).¹

As to Morgan's implicit assumptions, I would like to concentrate on the ones about language in as far as these can be deduced from the scant references to this subject in his book. Briefly this is what his thoughts boil down to. A system of relationships is invented to regulate relations between a group of human beings; basically two such systems have evolved. Each system adopts a nomenclature to express the relationships which obtain within it. Morgan cannot conceive how the classificatory system as a social system could change into the descriptive system; and he sees the terminological systems as equally stable: "A change of any of these relationships, or a subversion of any of the terms invented to express them, would be extremely difficult if not impossible; and it would be scarcely less difficult to enlarge or contract the established use of the terms themselves" (op.cit., p.15). Every person who speaks the common language is as it were the trustee of the system. It would seem that if changes in the terminology do take place, it will be, in the case of the descriptive system,

1. This is a different way of stating what Morgan (op.cit.: 480) presents as the evolution from Promiscuous intercourse via the Communal Family, the Tribal Organization, the Barbarian Family, and the Patriarchal Family, into the Civilized Family in the case of the classificatory system, with the nuclear family of the descriptive system quite separate from this.

that descriptive terms worn out by long usage, are being replaced by compound terms, and, in the case of the classificatory system, that changes probably only lead to greater complexity in line with the existing system. An evolutionary sequence of the various stages starting from Promiscuous Intercourse is postulated for the classificatory type: while this kind of change occurs only very slowly and at long intervals, language which is indicative of the system, is subject to a continuous process of very gradual change. But even when the form of language thus changes, the ideas and conceptions they represent are independent of the mutations of language. Where philology has been unable to solve the problem of the linguistic unity of mankind by conclusive linguistic evidence, the constancy of the systems of relationship might help provide the answer. (cf. Morgan, 1871: esp. 14,15,506).

2.1.3 As we progress in this historical survey we shall see how his theory has been challenged on various points: his circular reasoning, his major distinction between descriptive and classificatory systems itself or its rigidity and oversimplification, his use of kin terms as an argument. For the moment I would simply like to make a few general observations from the linguistic point of view.

While Morgan works on the meaning of kin terms disregarding their form, he draws conclusions as to genetic links between language communities. This goes right against the concept and method of philology.

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A genetic classification must be based on criteria of sound-meaning resemblances of linguistic forms: only then are the results unequivocally conclusive as to any genetic link. When one works equally on the basis of criteria of sound without meaning or of meaning without sound (as Morgan does), the result will be a typological classification. Such a classification can be trivial or of great value but is always arbitrary in the sense that any criterion or combination of criteria may be used provided they are clearly defined. In fact it would seem that Morgan uses mainly one criterion, the classification of collateral kin as embodied in the meaning of kinship terminological systems: from this typological argument he draws genetic conclusions.

Morgan seems to suggest that a system of relationships develops and that subsequently a terminology for it is adopted. In my view language is what makes the human animal human; consequently I would not accept that anything human could develop separate from language, ideas are not operative unless they are, in some way, verbalised. Moreover I would hold that relationships within groups of human beings are, in the same way as language is, co-extensive in time with mankind itself: any theory about the origin of kinship terminologies would for all practical purposes coincide with, or at least be part of, a theory about the origin of language. Any such theory is, in the present state of our knowledge, meaningless.

It is implied that adequate nomenclatures are adopted for each type of kin classification. But as the

formal aspect of the terms is left out of consideration, there is no way in which the value of recognised linguistic mechanisms can be assessed, such as metaphor, the use of sound-symbolism, processes of association, the use of compounding. As for the process of language change, the possibility of change in vocabulary through borrowing is dismissed far too lightly, and inversely the fact that sometimes lexical items just cease to be used and disappear from the lexicon of a language is not even considered. In connection with this it is important to state that proof 'e silentio linguarum', from the absence of linguistic categories or linguistic forms, must be handled with extreme caution, and never be considered absolute by itself. Even if one is careful and avoids sweeping statements and global solutions where the facts do not warrant anything of the kind, all one may be able to conclude to might be that at some time certain common concepts did exist. But one does not know anything more precise yet as to the exact nature of such concepts, e.g. the content of a certain relationship. The temptation is always there to simply identify it with the present-day meaning. Somehow it seems much more relevant, even in a historical perspective, to ask, not at what precise moment language and culture did match on this or any other point, but what the nature of their relationship was.

2.2 John Ferguson McLennan.

2.2.1 Although John F. McLennan published his work

Primitive Marriage in 1865, six years ahead of Morgan's Systems, I have preferred to discuss his contribution after Morgan's for reasons which will soon become clear. At first McLennan did not discuss kinship at all when he studied social organization: his main interest lay in the evolution of marriage and types of descent. In this respect he is one of the most influential forerunners together with J.J. Bachofen who published his Das Mutterrecht in 1861. While Bachofen took a more theoretical line, McLennan broke new ground by basing himself on ethnographic material, accounts of strange customs brought back by an ever increasing number of travellers. The view he so arrived at was that marriage started from promiscuity; gradually this led to polyandry. This situation came to be experienced in due course as a lack of women, which led to women being captured from neighbouring groups. It was this custom of marriage by capture which in McLennan's view constituted the beginning of marriage proper.

McLennan did not discuss kinship in his work; but when he read Morgan's Systems where a different account was given of the evolution of marriage, he was forced to consider Morgan's arguments based on kinship terminology, and thereby to state his interpretation of Morgan's ethnographic facts. He did this in Studies in Ancient History, published in a new edition in 1886, in which Primitive Marriage was reprinted. Morgan's work is discussed in a study, entitled "The Classificatory System of Relationships" (p.247-315). We shall only report here on his reflections which would seem to have

some bearing on the problem of kinship terminology.

He interprets Morgan's views on classificatory terms as implying a claim that several persons are referred to by only one term (e.g. in the Hawaiian system all the women relatives in the generation above Ego are called "mother") because the languages in question lack terms to denominate their exact relation to Ego. McLennan refuses to accept this view: "This is an explanation on the ground of poverty of language (of which no proof is adduced - nay more, which there are facts to disprove), not an explanation from the nature of descents" (1886: 259). McLennan does not deny the facts, but he would seem to have hit implicitly on such notions as 'structure' and 'balance' when he said about the Hawaiian system: "While the language is thus poor in terms to denote persons of the highest importance in a system of blood-ties, it is rich in terms required in a nomenclature of courtesies" (ibid., 279, footnote 3).

McLennan's main contention was that Morgan was wrong "to have so lightly assumed the [classificatory] system to be a system of blood-ties" (ibid. 269). Even if one - as McLennan himself does - accepts an original phase of promiscuity, one cannot help feeling that McLennan is right in saying that there could be no difficulty in proving conclusively a blood-tie at least with the mother: if therefore the classificatory term 'mother' is given to other women as well, it will not be on the basis of some blood-tie. And anyhow, there are in the ways in which various peoples trace descent, sufficient means to indicate

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blood-ties so as not to have to rely on classificatory terms to fulfill this function. This leads him to this 'nomenclature of courtesies': "What duties or rights are affected by the 'relationships' comprised in the classificatory system? Absolutely none. They are barren of consequences, except indeed as comprising a code of courtesies and ceremonial addresses in social intercourse" (ibid. 273). This is not just a way of getting round an awkward problem, he sees positive reasons for his interpretation. "In their intercourse they must have terms by which to address one another; and personal/individual names not being in use - an invincible prejudice against the use of them being general among backward races - the terms employed must be general terms, applicable 'to the members of the family in classes' " (ibid. 283). This reluctance to use proper names was observed by Morgan too among the American Indians (op.cit.: 132). McLennan thought that what was required for use in address were terms which were fairly general and which at the same time took into account the age and sex of the person speaking in relation to the age and sex of the person spoken to. And the answer to this need he saw in the classificatory system. To illustrate his view he refers to the so-called Hawaiian type of classification in which type all cousins are equated with brother and sister which makes for generality, while all the men of the first ascending generation are grouped together as are the women of that same generation. This generalisation can go far: in the case of the Fanti of

Ghana even sex is not always specified in Ego's generation, as is shown in the following table:

	Female Ego	Male Ego
G +2	nana 'grand relative'	
G +1	egya Fa	
	na Mo	
	wofa MoBr	
G 0	nua 'sibling'	nua 'sibling'
		akyaraba 'sister'
G -1	ba 'child'	ba 'child'
		awofasi 'sister's child'
G -2	nana 'grand relative'	
	(Kronenfeld, 1973: 1579)	

Elsewhere an age distinction is made within Ego's generation, but only a partial sex differentiation (for elder siblings) while no distinction is made between brothers and sisters on the one hand, and cousins on the other, e.g. the Njamal of North Western Australia:

<u>Kurda</u>	EBr, FaBrESo, MoSiESo, etc.
<u>Turda</u>	ESi, FaBrEDa, MoSiEDa, etc.
<u>Marage</u>	YSi, YBr, FaBrYSo, FaBrYDa, MoSiYSo, MoSiYDa, etc.

(Burling, 1970 b: 22)

The first ascending generation is sometimes purely generational, in other cases it is of the type called 'bifurcate merging' grouping together Fa and FaBr, and Mo and MoSi, while singling out MoBr and FaSi.

E.g.: Samoa (cf. Panoff, 1965: 63)

Here the generational terms cover both the 1st and 2nd generations.

Tamā FaFa, FaFaBr, FaMoBr, MoFa, MoFaBr, MoMoBr;
Fa, FaBr; HuFa, WiFa; eBr (when the
difference in age with Ego is 15 years or
more).

Tinā FaMo, FaMoSi, FaFaSi; MoMo, MoMoSi, MoFaSi;
Mo, MoSi; HuMo, WiMo; eSi (when the
difference in age with Ego is 15 years or
more).

Futuna (cf. Panoff, 1965: 72)

This Polynesian system illustrates the bifurcate
merging type.

Matua Fa, Mo, and all relatives of the first
ascending generation

Tamana Fa, FaBr; MoSiHu

Tinana Mo, MoSi; FaBrWi

Tuatinana MoBr

Masaki FaSi

The range of meaning in the bifurcate merging type is not
the same everywhere, as the following data from the Mara
in Australia show:

(cf. Korn, 1973, 117, after Warner, 1933)

lur-lu F, FB

kai-djŭr-ri M, MZ

bar-nan-a FZ

gar-dī-gar-dī MB, MBSS, MBSD

While Morgan continued to maintain that kin terms were never used except between people who knew they were each other's relations, and never used by such people except to denote that relationship, McLennan could quote many ethnographic accounts stating explicitly the contrary, beginning with a quotation from Lafitau:

"En se parlant les uns aux autres, ils se donnent tous des noms de parenté, de frère, de soeur, d'oncle, de neveu, etc., observant exactement les degrés de subordination et toutes les proportions de l'âge à moins qu'il n'y ait une parenté réelle par le sang ou par l'adoption. ... Ils pratiquent la même civilité à l'égard des étrangers, à qui ils donnent, en leur parlant, des noms de consanguinité, comme s'il y avait une vraie liaison du sang, plus proche ou plus éloignée à proportion de l'honneur qu'ils veulent leur faire" (cf. McLennan, 1886: 306).

2.2.2 A few short observations and comments would seem appropriate with regard to what I consider to be an extremely interesting contribution to the subject. First of all it is worth noticing that a new concept is brought in: address. Later on it will be generally accepted to make a distinction in kinship terminology between terms of reference and terms of address. For many languages this distinction is not of great importance: it is often the case that there are no specific terms of address, only a way of using some kin terms (or special forms of kin terms) as terms of address, though there may be terms which are on the whole used more often in address than in reference. This would seem to be a matter deserving of more attention, whether the distinction between reference and address is necessary or at least useful, or whether the

differences can be dealt with as a matter of usage. The question of use vs. reference is an important one which arises in any study of semantics which pays attention to contextual and situational factors. And once one starts taking into account non-linguistic factors, it may well prove necessary to consider the distinction between semantics and pragmatics in this context and to try and see if the distinction is a necessary and useful one or if it needs re-definition to be acceptable. As in systems of address kin terms alternate or combine with proper names, the status of proper names and their position vis-à-vis common nouns may have to be considered. Social factors of role and status, and psychological factors of intimacy and respect, distance, may come up in this connection. The issues raised by McLennan's views deserve serious consideration, for if he is right in claiming that classificatory relationship systems have no consequences whatsoever for the social system, the whole hypothesis of linguistic relativity would become more difficult to define and formulate since the case of kinship terminology and its social relevance is normally regarded as one of its best examples in the lexical domain.

2.3 Franz Boas.

2.3.1 The contribution made by F. Boas to our field of interest is twofold: as an anthropologist he examined critically and, to a large extent, refuted, the methods

and assumptions of his predecessors; and as a student of American Indian languages he developed interesting views on the relation between language and culture, on ethnolinguistics.

The 19th century was the century of history, and Boas who began his scientific career in that century, subscribed to the importance of the historical point of view.

"We may perhaps best define our objective as the attempt to understand the steps by which man has come to be what he is, biologically, psychologically, and culturally. Thus it appears at once that our material must necessarily be historical material, historical in the widest sense of the term" (1932: 244).

"To understand a phenomenon we have to know not only what it is, but also how it came into being. Our problem is historical" (1936: 305).

But this does not mean that he made this historical bias his without any reservation or change. For while he accepted the historical point of view, he rejected the evolutionary variety of it.

"The evolutionary point of view pre-supposes that the course of historical changes in the cultural life of mankind follows definite laws which are applicable everywhere, and which bring it about that cultural development is, in its main lines, the same among all races and all peoples" (1920b:281).

He sees it as a misleading principle to assume that wherever similarities of culture are found, there must have been connections: it is a hypothesis which simply cannot be proved and therefore is without foundation and usefulness. Others had pointed out this weakness before him; but in order to account for the similarities of culture found all

over the world, some scientists had introduced a different historical explanation, viz. that such similarities must always be due to migration and diffusion. As Boas points out, this explanation too rests on unproven and unprovable assumptions and hence must be considered as of no value. He enumerates some of its weaknesses:

"On this basis historical contact is demanded for enormously large areas. The theory demands a high degree of stability of cultural traits such as is apparently observed in many primitive tribes, and it is furthermore based on the supposed coexistence of a number of diverse and mutually independent cultural traits which reappear in the same combinations in distant parts of the world" (ibid.: 282).

His own approach to history is much more careful. He too wants to trace the history of growth of human culture, but before feeling able to deal with such a vast subject he first wants to establish histories of growth of diverse tribes, based on fact, not on hypothesis or speculation. This means: to take account of whole cultures, not just the one aspect under observation in isolation, and to take account of the geographical situation of a tribe among other tribes as a possible factor of explanation. It is then that the various elements which may have helped to shape a history of growth, will become clear: the environmental conditions with their modifying influence, psychological factors at work in the development of a culture, and historical connections. In this manner anthropology will be able to discover the processes which in definite cases led to the development of certain customs. This

alone will provide a sound enough basis for further comparison and generalization. (cf. Boas, 1896: 270-280).

2.3.2 Boas' thoughts about the relation between language and culture are stated very clearly and succinctly in his Introduction to the Handbook of American Indian Languages (1911). He first discusses the various attempts to establish classifications of mankind (op.cit.: 2 ff.), and he points out that classifications have been made on the basis of anatomical characteristics, of culture, and of language, with very different results. One would obviously want to correlate these various classifications but this proves impossible as the three aspects develop and change at a different rate. We see at present cases where the physical type has not changed noticeably while language and culture have changed completely according to geographical position: the clearest example is that of the Negro population of the various parts of the American continent. Alternatively the language can maintain its special position while the physical type changes and adapts to the environment: the standard example are the Magyar speaking people. Even if both language and physical type change at the same time, such changes do not necessarily run parallel to one another as is proved in the case of the Arab conquest of North Africa where the Arabs kept their language but through inter-marriage were immersed into the local population and changed their type, while the original population preserved their type but changed their language. Finally we see many examples where physical type and language remain

permanent while culture changes through diffusion: Boas gives here as one of his examples California where we find great diversity of language and a certain degree of differentiation of physical type, with a considerable uniformity of culture.

While this is the present day picture, it might still be possible to assume an original close association of type, language, and culture: but there is nothing to prove such an assumption of close correlation, it is not even plausible given the fact that we do see now that language changes more rapidly than physical type, and this is even more true of culture. Not only is it impossible then to establish any correlation, there may never even have been one.

Boas' extreme caution in formulating explanatory theories for social phenomena and in assessing the relationship between language and culture did not prevent him from trying to define his view of the matter; and in doing so, he also mentioned the study of kinship terms. Towards the end of his Introduction (1911: 52 ff.) he speaks of the special importance which should be attached to the study of language in the work of an ethnologist, not only for practical reasons but also from a theoretical point of view. First there is the problem of language and thought, i.e. the question whether all languages are capable of dealing with abstract thought, or whether restrictions stemming from the language could hold a culture down at a level of lesser cultural sophistication. Boas concludes quite rightly that any such direct relation between language and culture is very unlikely. At the most

one might say that a language does not produce linguistic forms which are not required in the general state of a culture. One could almost call this a beginning of functionalism, where he says: "It seems much more likely that the lack of these forms is due to the lack of their need" (loc.cit.: 53-54).

While ruling out a direct relation of a relativist nature, Boas, with all his usual caution and reservation, comes up with an explanation of how the understanding of linguistic categories and elements can be useful in understanding ethnological phenomena (loc. cit.: 56 ff.). The essential difference between linguistic and ethnological phenomena is, that the linguistic classifications never rise into consciousness, while in other ethnological phenomena, although the same unconscious origin prevails, these often rise into consciousness, and thus give rise to secondary reasoning and to re-interpretations. Both the linguistic and the ethnological phenomena have in common the feature of grouping together a considerable number of activities under the form of a single idea, without the necessity of this idea itself entering into consciousness.¹ But while in ethnology secondary explanations often tend to obscure the real history of the development of ideas, the categories in language always remain unconscious, and so

1. It is not difficult to see some similarity here with the theories of E. Durkheim for whom society is in essence a complex of ideas: collective life is made up of representations, collective representations, i.e. ways of acting, thinking, sensing, which are external to the individual, and these exist in the collective conscience.

the processes which led to their formation can be followed without the interference of secondary explanation. For this reason it is worthwhile to analyse linguistic concepts and to see how each language has its peculiar way of grouping ideas; this applies not only to the morphological-grammatical characteristics, but also to the vocabulary. In connection with this last aspect Boas says:

"Inferences based on peculiar forms of classification of ideas, and due to the fact that a whole group of distant ideas are expressed by a single term, occur commonly in the terms of relationship of various languages; as, for instance, in our term uncle, which means the two distinct classes of father's brother and mother's brother. Here also, it is commonly assumed that the linguistic expression is a secondary reflex of the customs of the people; but the question is quite open in how far the one phenomenon is the primary one and the other the secondary one, and whether the customs of the people have not rather developed from the unconsciously developed terminology" (1911: 60-61).

To show how customs might have developed from language, Boas refers to cases in which the use of descriptive terms for certain concepts, or the metaphorical use of terms, has led to peculiar views or customs:

"it seems plausible to my mind, for instance, that the terms of relationship by which some of the eastern Indian tribes designate one another were originally nothing but a metaphorical use of these terms, and that the further elaboration of the social relations of the tribes may have been largely determined by transferring the ideas accompanying these terms into practice" (1911: 61).

When we look at Boas' actual treatment of social organisation including kinship terminology, we are faced with a picture which is not wholly consistent or clear.

In one place he says: "The fundamental difference between the organisation of the Kwakiutl and the northern tribes appears also in the terminology of relationship". "The terms correspond to a loose organisation in which relationship is counted equally on both sides" (1920a:368). These quotations seem a fairly neutral way of stating that there is some link between language and culture on this point without specifying the direction of dependence, if any. In another article on the Tribes of the North Pacific Coast (1924) he is less careful, though perhaps unintentionally. At one point he says: "These differences are expressed in the systems of the terms of relationship" (loc.cit.: 371), implying, I would have thought, that the differences in social structure precede the terminologies which express these, while a bit later he speaks of "the terminology of relationship which underlies the social system of the Bella Bella and of the more northern Kwakiutl tribes" (ibid. 373). Unless I read too much into the word 'underlies', Boas here seems to have inverted the order of precedence. The least one can say is that he did not really apply his full critical attention to the problem of linguistic relativity, notwithstanding his earlier theory - quoted above - of transferring ideas contained in terms into social practice. Anyhow, even this idea of metaphorical extension as the starting point for a new social practice, is not adhered to very consistently. When discussing the origin of totemism he clearly holds that a social fact - the desire to be able to identify an incest group - is at the root of a set of kin terms

being extended in meaning (cf. 1916: 321). When he discusses the Relationship system of the Vandau of Mozambique (1922), he speaks again of the system of relationship terms being "founded on the following principles", and goes on to enumerate social factors, mainly questions of status and role. But to be fair I think it might well be that, as a rule, he does not intend to discuss the question of linguistic relativity and in particular the matter of priority between language and culture. He clearly accepts that there is some sort of correspondence, and as he appears to be a social scientist first and foremost, his reasoning usually starts from the social side. But even so he remains cautious: kinship terms are for him only one ethnological factor, one cultural feature among others. And while he accepts some correspondence, he never does so in an absolute way, drawing general conclusions from what is only one aspect; he always is very factual and intent on relativising his conclusions by taking into account all known facts. As he said for instance at the end of his study of the Vandau relationship system: "I am far from maintaining that the present explanation corresponds to the historical development of the system" (loc.cit.: 395). What he did prove was that one of the generalizations of the evolutionists, viz. that avunculate¹ always goes with maternal succession, is not true in its absoluteness.

1. Avunculate: the rights and duties the maternal uncle has towards his sister's sons, and his power over them.

2.3.3 In conclusion we can say that Boas brought the issue of language and culture, including that of kinship terminology, down from the level of speculative history and unwarranted hypothesis, to that of fact and verification. While admitting that evolutionism and diffusionism were the wrong kind of theory, one would not want to rule out theory and hypothesis altogether. But it is quite clear that we must know what the theory is supposed to explain. We need, in other words, a clear picture of our problem, based on fact. Boas, by his painstaking analytical work, has made contributions of importance here. The notions of metaphor and even more so of extension will gradually become crucial issues in the study of kinship terminology. And in his analysis of the Vandau system he mentions such things as forms of address, social rank and respect owed, all of them notions which play a role in the on-going debate on the nature of kin terms and the correct way of analysing them.

2.4 Edward Sapir.

2.4.1 The name of Edward Sapir is one which must figure in any study concerned with problems of language and culture: some of his ideas are at the basis of what has come to be known as the Sapir-Whorf hypothesis. In fact, as we have seen, Boas too had formulated similar thoughts in his Introduction (1911: esp.61), but it was left to Whorf to try and work out a concrete model of the dependence of thought and culture on language, working

on the basis of a comparison between the grammatical forms and categories of Hopi and SAE. It is too easily forgotten that Whorf's hypothesis had nothing final about it: as Stuart Chase said in his Introduction to Whorf's Selected Writings (Whorf, 1956: p.X), Whorf definitely felt that more research was needed and "theoretically this might mean the end of linguistic relativity". Sapir's thinking has nothing final about it either. On the contrary, one often feels that he is still trying to find a balance, so much so that some of the things he said seem almost contradictory.

There is no doubt that some of his writings point to linguistic relativity:

"Language is not merely a more or less systematic inventory of the various items of experience which seem relevant to the individual, as is so often naively assumed, but is also a self-contained, creative symbolic organization, which not only refers to experience largely acquired without its help but actually defines experience for us by reason of its formal completeness and because of our unconscious projection of its implicit expectations into the field of experience" (Sapir, 1931: 128).

"It is quite an illusion to imagine that one adjusts to reality essentially without the use of language and that language is merely an incidental means of solving specific problems of communication or reflection. The fact of the matter is that the 'real world' is to a large extent unconsciously built up on the language habits of the group. ... The worlds in which different societies live are distinct worlds, not merely the same world with different labels attached" (1929: 162).

Whether Sapir changed his mind on the subject or whether he simply approached the same problem from totally opposite angles and never really got round to reconciling the two,

I do not know. What is certain is that, when he published Language in 1921, he stressed mainly the negative point of view: "It is easy to show that language and culture are not intrinsically associated" (1921: 213). In that tenth chapter of Language he develops an argument similar to the one of Boas on the distinction between race, language, and culture. Language, he argues, is not related to "national temperament" or race (op.cit.: 216 ff.), nor is language in any way causally related with culture (op.cit.: 218). But the fact that they are not necessarily correlated, does not mean that they never are. "There is some tendency, as a matter of fact, for racial and cultural lines of cleavage to correspond to linguistic ones" (op.cit.: 215). However, Sapir makes it quite clear what he sees as the importance of such coincidences of racial, cultural, and linguistic divisions: "[their] significance is not one of inherent psychological relation ... The coincidences of cleavage point merely to a readily intelligible historical association" (op.cit.: 216). So we are back again with the historical interest, but with a word of caution, for history of culture and history of language are non-comparable processes:

"The drift of culture, another way of saying history, is a complex series of changes in society's selected inventory - additions, losses, changes of emphasis and relation. The drift of language is not properly concerned with changes of content at all, merely with changes in formal expression" (op.cit.: 218).

Two words in this last quotation are worth noting, viz. 'content' and 'formal expression'. The correlations Sapir is talking about, are between cultural

traits and linguistic features of grammar and morphology: this is clear both in his 'pro-relativity' article from 1931 and in Language (p.219). But in Language he then continues:

"It goes without saying that the mere content (emphasis mine) of language is intimately related to culture. ... In the sense that the vocabulary of a language more or less faithfully reflects the culture whose purposes it serves it is perfectly true that the history of language and the history of culture move along parallel lines."

However, Sapir calls this kind of parallelism "superficial and extraneous and of no real interest to the linguist". It would be a mistake for a linguist, he says, to identify a language with its dictionary. Still, this statement too is mellowed somewhat, again in a perspective of history:

"Many cultural objects and ideas have been diffused in connection with their terminology: so that a study of the distribution of culturally significant terms often throws unexpected light on the history of inventions and ideas" (1929: 162-163),

provided one does not pretend to trace history to its very origins:

"It may be that originally the primal cries or other types of symbols developed by man had some connection with certain emotions or attitudes or notions. But a connection is no longer directly traceable between words, or combinations of words, and what they refer to" (ibid.: 164).

2.4.2 Undoubtedly, Sapir's general ideas are of great interest for our study. Fortunately we also have his thoughts on the precise subject of kinship terminology in his article "Terms of Relationship and the Levirate" (1916). On the whole he subscribes to the main argument developed by Rivers "that many groupings of kinship terms

are best understood as expressive of particular types of marriage" (loc.cit.: 327). In a footnote he briefly explains his personal view of the matter:

"Personally I believe that the factors governing kinship nomenclature are very complex and only in part capable of explanation on purely sociological grounds. In any event, I do not seriously believe that thoroughly satisfactory results can be secured without linguistic analysis of kinship terms. Moreover, for the proper historical perspective we must have some feeling for the lack of strict accord between linguistic and cultural change. This means that an existing nomenclature may be retained, at least for a time, in the face of sociological developments requiring its modification. Direct sociological interpretation of descriptive kinship data may be as unhistorical as any other mode of direct interpretation of descriptive cultural facts".

More in particular he then goes on to deal with one specific form of marriage, the levirate¹, to see how it finds its expression in kinship nomenclature of two given groups of American Indians, the Upper Chinook in Southern Washington, and the Yahi (or Southern Yana) in northern California. One would expect the levirate to affect the terminology for step-relationships and filial and fraternal relationships, as potentially real relationships. In fact this potential identification is present in the terminology, for instance:

- in Upper Chinook (loc.cit.: 329)

i-mut	FaBr	<u>and</u>	StFa
a-gutx	MoSi	<u>and</u>	StMo

1. Levirate: a custom whereby a widow preferably marries a brother of her deceased husband.

i-wulx BrSo (m.s.), SiSo (w.s.) and StSo
 a-wulx BrDa (m.s.), SiDa (w.s.) and StDa
 - in Yahi (loc.cit.: 330)

galsi Fa and FaBr
 mucdi FaSi and BrSo or BrDa (w.s.)
 ganna Mo and MoSi
 u'dji'yauna MoBr and SiSo or SiDa (w.s.)
 'i'sip!a So and BrSo (m.s.) + SiSo (w.s.)
 mari'mip!a Da and BrDa (m.s.) + SiDa (w.s.)

Sapir's data bear out a similar correspondence for terms for fraternal relationship while the influence of the levirate also appears in Yahi terms of affinity.

The fact that the Southern Yana have this bifurcate merging system while Northern and Central Yana have distinct terms for each of the four types of uncle and aunt, confirms for Sapir that one has to look for some such influence as the levirate marriage to explain the special development in the case of the Yahi. How the Yahi came to attach such an importance to this form of marriage is a different problem; it probably is a matter of influence from outside, but lack of factual information makes it impossible to verify this.

Sapir makes it very clear that, what is an acceptable explanation of a certain type of terminology in one case (as the levirate for the Upper Chinook or the Yahi), is not necessarily an explanation which therefore must hold generally for all cases with a similar terminology. Each case has to be examined on its own merits and the most appropriate explanation be selected (loc.cit.: 335). But he feels that it does show the importance of paying

attention to the terminology of step-relationship.

2.4.3 This is not the moment to give a full-scale evaluation of the linguistic relativity hypothesis, but a few concluding remarks and observations seem indicated. The question of the 'original meaning' keeps cropping up with the tension between what can be positively established as fact and what can be tentatively stated as working hypothesis. Sapir does not oppose the two points of view in this way but I think he gives them all the same in different places. In his 1929 article (p.164) he said that a connection between words and what they refer to, is no longer directly traceable; in the 1931 one he wrote: "Such categories ... are, of course, derivative of experience at last analysis". He said this admittedly of grammatical categories, but I see no reason why not to understand it about language generally, especially as the linguistic relativity of grammatical categories is commonly accepted to be the more difficult to establish. Reading Sapir against this contemporary background of the distinction between hypothesis and verified fact may well turn his sometimes seemingly self-contradictory statements into a useful starting point and source of inspiration for further research.

In the passage I just quoted from the 1931 article, Sapir said that categories were "derivative of experience", and he continues:

"but, once abstracted from experience, they are systematically elaborated in language and are not so much discovered in experience as imposed upon it because of the tyrannical hold that linguistic form has upon our orientation of the world".

Sapir here clearly points to language's own structure in its systematization of fundamental concepts from the grammatical point of view. This notion of structure has since been applied to vocabulary as well, e.g. in linguistic field theory. I think this insight must affect the validity of Sapir's opinion on the relevance of vocabulary in establishing a parallelism between language and culture. In Language (1921: 219) he deems vocabulary as "of no real interest". While one might agree that isolated lexical items are of little or no interest in this respect, lexical structure must be given much more serious attention.

A last remark: Sapir, in his study on relationship terms, makes much of step-relationships. Whenever one sees this labelling of terms of very different systems in terms of their nearest English equivalents, one feels hesitant because of the ever-present danger of interpreting the other system in function of one's own system instead of evaluating it as a system in its own right. And I see a link here with another problem, viz. the question: which terms are kin terms? Do we define them with reference to blood-ties basically and consider non-blood-tie relations as extensions, or do we define them socially? And, if the latter, how do we avoid circularity in explaining a system by reference to social factors if we define the set of our terms socially in the first place? Whatever the answer to these questions is going to be, I would like to give Sapir credit for stimulating reflection on these matters.

2.5 Alfred Louis Kroeber.

2.5.1 The importance of A.L. Kroeber's work in connection with the study of kinship terminology lies in the fact that he continued to stress the value of the historical view-point throughout a time when structuralists and functionalists made their impact with new and enlightening theories (cf. below Ch.3), while he drew attention at the same time to what were to become some of the basic orientations and concepts of modern formalism.

Kroeber's claims for the value of the historical approach were less exclusive than those made in the 19th century. In a discussion with Radcliffe-Brown on the subject he is quite ready to admit that a different approach can be justified though he had obviously given a different impression before.

"What I should have expressed was my conviction that the factors at work in the phenomena in question are numerous and variable enough to make it seem highly questionable whether determinations of constants other than of narrow range or vague nature can be made, or at any rate have yet been made, while historical considerations are omitted" (Kroeber, 1936: 338).

One year later he made a similar claim for the necessity of a historical dimension to any really valuable study of kinship systems, this time from the angle of linguistic history: "the analysis and comparison of such systems without reference to their linguistic history, so far as this may be available, is an arbitrary limitation on understanding" (1937: 607). All this fits in with his

view of the aim of the investigation of human society or culture: to determine the constants, the abstractions extricated from phenomena as they occur in space, time, and variety of character (cf. 1936: 341). As far as linguistic history is concerned his interest was in turn predominantly typological, areal, or genetic.

2.5.2 What has been said here about his historical orientation in his study of culture in general applies equally to the study of kinship: "the patterns have had each a history of its own as a pattern, just as the languages in which they occur have had each a history of its own" (1952: 200). I think that this thought that each system has a pattern of its own, is crucial for a correct appreciation of Kroeber's important article, "Classificatory Systems of Relationship" (1909). In it he wants to show that Morgan's basic distinction between classificatory and descriptive systems is wrong:

"According to the prevalent belief the systems of certain nations or languages group together distinct relationships and call them by one name, and are therefore classifying. Other systems of consanguinity are said to indicate secondary differences of relationship by descriptive epithets added to their primary terms, and to be therefore descriptive. Nothing can be more fallacious than this common view" (1909: 77).

It is true that, understood in this sense, such English kinship terms as brother, uncle, cousin, etc., are classificatory because they do not distinguish elder and younger brother, paternal and maternal uncle, all possible kinds of cousins, etc.. There is a complication, however, in that Morgan did not define classificatory that way: for

him it refers to the merging, terminologically, of lineal and collateral kin. Leslie A. White (1958) has discussed this point in defence of Morgan as many people had come to accept Kroeber's criticism on this point. Of course, part of the blame for this confusion must rest with Morgan himself who could have distinguished more clearly between the semantic question of reference (how many referents does a term have for Ego) and the morphological question of descriptive devices (how to specify relationships by using kin terms plus a specifying adjective or some such device). While White is right in insisting that Morgan should be read properly, one gets the impression that he also wants to defend Morgan's evolutionary point of view, be it in an updated version. And I feel that Kroeber's criticism of Morgan, though unfortunately ill-founded in its point of attack, remains valid in a wider context. It would seem to me that what Kroeber in fact is attacking is implicit racism, a view whereby race, inferior type of culture and social organisation, and the linguistic terminology used to express that culture, are linked and are substantially different from one part of mankind (classificatory) to the other part (the descriptive elite). This basic criticism is wholly in the tradition of Boas, Sapir, and others, and by itself would not warrant separate discussion; but Kroeber also put forward constructive new ideas.

Morgan classified relationship systems on the basis of one criterion only, viz. lineal vs. collateral, ending up with a very weak and disputable typology. If one wants to avoid the trap of ethnocentrism or racial

prejudice, more criteria are required. Kroeber saw that every term had a certain range of referents, different from language to language. To account for this fact he put forward his theory about the principles or categories of relationship underlying these terms. He touches here upon a basic problem in semantics: the range of referents of words; and he solves it in the way many people would approach the problem at present, by pointing at a distinctive feature model. He distinguishes eight components of meaning: generation, lineal/collateral, relative age within one generation, sex of relative, sex of speaker, sex of linking relative, blood relationship/affinity, and the condition of life of the linking relative. Each language uses its own combination of features, some use many, others fewer. If Morgan's distinction is to have any validity, Kroeber feels it will have to be on the basis of the categories described. It appears that Morgan's descriptive systems express a small number of categories of relationship in every (or almost every) term of the system, while the so-called classificatory systems express a larger number of categories but with less regularity. This feature-based approach to relationship systems makes it possible to single out the main characteristic of a system, to compare systems, and to classify them typologically in a sound way.

When we look at a particular analysis we see both Kroeber's interests combined. For instance in his "Kinship in the Philippines" (1919) he aims at a reconstruction of the ancient kinship system while using, in the analysis of his data, the sort of category we have been

discussing, and characterising the reconstructed system in terms of these categories. In the light of these findings it then appears that inter-tribal divergencies are considerable but that departures from the general logical scheme are much slighter: in fact, given the differences of level or type of culture they are proportionately small.

When Kroeber criticised Morgan's concept of a classificatory system, he saw as the root of this misconception the fact that terms of relationship had been regarded principally as material from which conclusions could be drawn about the organisation of society and conditions of marriage, rather than as elements determined primarily by linguistic factors. This is not to say that he ruled out all social relevance of language. In line with Boas and Sapir he argued the case for the distinction between race, language, and culture. Race stands clearly apart in a separate position; but, "Speech and culture do tend to form something of a unit as opposed to race. (...) The cultural differences tend to crystallize around language differences, and then in turn are reinforced by language, so that the two factors interact completely" (1948: 226). Kroeber is very cautious not to conclude too hastily or too easily to historical connections. Organisation or structure in both language and society takes place according to unconscious or covert or implicit patterns.

"The number of such linguistic and social patterns or forms being limited, there is some tendency for them to repeat in different cultures with a degree of similarity, though without historical connection or without attaining actual identity" (1948: 249).

On the whole, it would seem, Kroeber would be inclined to look for congruences in the matter of language and culture correlations, not for determinism.

This generally cautious point of view can be observed equally when he discusses the "term-usage correlation" of kinship terms (1936: 339). He gives the analogy of the relation of dress to the human body: there are various degrees of fit, differences of style.

"Similarly with the fit of kin terminology to social usage: it may be close or wide. Expectably there will always be some fit, and there may be a great deal, but it may also be remarkably partial".

"A normally large amount of play or give in fit is evident. Portions of a naming system can be indifferent from the point of view of social structure, or vice versa" (ibid: 340).

In this paper he views the matter slightly more positively than in his earlier work where he wrote, for instance: "To connect the institutions and the terms causally can rarely be anything but hazardous" (1909: 83; cf. also 1919: 84).

Up to this point his position may have been a bit stricter and more cautious than that of others: but it was not this that attracted most attention and drew extensive comment and reaction. What caused most controversy was his insistence that factors other than the social one, played a role, indeed, were more important: "Terms of relationship reflect psychology, not sociology. They are determined primarily by language and can be utilized for sociological inferences only with extreme caution" (1909: 84). His insistence on the linguistic factor was most welcome; in his paper on the Philippines he paid attention to the changes within kinship vocabulary,

distinguishing such different possibilities as adoption of other words as kin terms, extension of meaning of existing kin terms, and formation of totally new words, while he recognised that etymological and semantic development, and dialectal divergence, may go their own way and diminish the possibility of predicting any specific term-usage correlation (cf. 1919: 82-84).

But his reference to psychology led to confusion, yet is perhaps his most interesting contribution together with his feature analysis. The confusion is clear from the reaction of someone like Rivers:

"In social, as in all other kinds of human activity, psychological factors must have an essential part. (...) These psychological elements are, however, only concomitants of social processes with which it is possible to deal apart from their psychological aspect" (1913: 93).

So psychology is seen by Rivers, and by many others in his wake, as the study of the ideas, beliefs, emotions, and sentiments which act as the immediate motives of people's actions. I believe that Kroeber must be understood differently. In Kroeber (1909) three factors are enumerated: sociological, linguistic, psychological. In Kroeber (1919) and in Kroeber (1936) we find again a three-way division: the social and linguistic factors are there; the third factor, the 'psychological' one, is now, however, differently and more explicitly defined as "a system of classificatory thought", "styles of logic in a limited field of universal occurrence", "a logical scheme". Defined in this way Kroeber's claim for the overriding importance of the psychological element becomes easier to understand, whether one accepts his view or not.

"Institutions and terminologies unquestionably parallel or reflect each other at least to the degree that a marked discrepancy of plan is rare. Institutions probably shape terminologies causally, but in the main by influencing or permitting a logical scheme. In a sense this logical scheme underlies both institution and terminology, so that the correlation between them, although actual, can be conceived as indirect" (1919: 84).

2.5.3 In conclusion we can only wonder at the strange way in which things turn out. Kroeber's influence in anthropology has been enormous, and rightly so. Yet on this particular point of the methodology of the study of kinship terms he pointed the way to two key issues of the sixties and seventies, and he was never taken up seriously on either of them. These two issues are his 'categories of relationship', basically what is now known as 'formal analysis', and his 'logical scheme' which resurfaces to my mind in present day 'cognitive anthropology':

"Cognitive anthropology is based on the assumption that its data are mental phenomena which can be analyzed by formal methods similar to those of mathematics and logic. Each particular culture consists of a set of logical principles which order relevant material phenomena. To the cognitive anthropologist these logical principles rather than the material phenomena are the object of investigation" (Tyler, ed., 1969: 14).

It is not difficult to see here a parallelism with the universalism and mentalism of much of recent linguistic theory. But in those early days the time clearly was not ripe yet to take up and develop fully all the possibilities implicitly contained in Kroeber's work.

2.6 W.H.R. Rivers.

2.6.1 W.H.R. Rivers held as a cardinal assumption that systems of kinship terminology give a valuable instrument for studying the history of social institutions, especially forms of marriage:

"I had reached the belief that in the systems of relationship we have, like fossils, the hidden indications of ancient social institutions and that their study is essential for advance in our knowledge of prehistoric sociology" (1914: I,3).

From this point of view there is little new in his position. Like Morgan he attached great importance to terminological systems:

"My chief object in making the survey was to obtain systems of relationship together with such other facts concerning marriage, descent and other social institutions as would assist the interpretation of the system" (ibid. 5).

But his field-work, more specifically his study of the Hawaiian system, made him abandon Morgan's evolutionary orientation. He supported Morgan, however, against McLennan, by showing that kinship nomenclature was more than just a system of address: "the designations of the [classificatory] system carry with them all kinds of duties, privileges and restrictions and are evidently of the utmost social importance and significance" (1914: I,6; cf. 1913: 44-48). And while he accepted Kroeber's criticism of Morgan's terms 'classificatory' and 'descriptive', he refused to accept the place accorded by Kroeber (cf. above 2.5.2) to psychology:

"In social, as in all other kinds of human activity, psychological factors must have an essential part. (...) These psychological elements are, however, only concomitants of social processes with which it is possible to deal apart from their psychological aspect" (Rivers, 1913: 94; cf. pp. 48-54 and 75 ff.).

2.6.2 Rivers' influence has been enormous, he can be said to have established the study of social organization as a recognised field of study. Even if some of his enthusiasm was slightly naive, some of his claims over-optimistic and exaggerated, his method of approach and lines of research continued to form the basic pattern of most research work for a long time.

His method in the field was what he called the genealogical method (Rivers, 1910). It consists in establishing for a given individual his pedigree, and then enquire by what terms Ego would refer to and address each of the persons forming part of the pedigree. These terms then form the object of a twofold analysis: a morphological one, considering the system of relationship as the expression of principles of classification to find the structure of the system, and a linguistic one which looks at the system of relationships as a collection of linguistic items (cf. 1914: II,9). The morphological analysis leads to a double conclusion:

"The chief general conclusion to be drawn from the survey of this chapter is that the forms of Oceanic systems of relationship are directly dependent upon features of: social organisation, and especially upon forms of marriage". "Another generalisation reached in this chapter is that there is a connection between distinctions in nomenclature and the presence of functions associated with relationship" (ibid.: 43 and 45).

The link between relationship systems and forms of

marriage has been Rivers' central point of argument, and one can only admire his grasp of complex systems and his ability to translate them into requirements of some anomalous form of marriage. His second conclusion about functions associated with relationship served a double purpose: it intended to prove McLennan wrong by concrete examples and, as all the functions enumerated centered upon marriage, it reinforced his first and basic conclusion.

The linguistic argument consists of a comparative survey of relationship terms in Melanesia, and goes on to claim that the area at first had a multiplicity of languages till foreign domination after invasion gradually imposed uniformity. It is in this light that the relationship terms are compared and assigned to different strata: the terms common to all or many languages are supposed to date from a later time while local variations are assumed to be survivals of an earlier diversity. This in turn makes it possible to discover the development of the social organisation, especially the forms of marriage (1914: II, ch.23). He returns to this point when he devotes a chapter to language (ibid. ch.36) to argue that language and social structure must be studied together: he sees no point in treating language as if it were independent and self-sufficient, to be studied merely for its own sake (1914: II, 495/6).

2.6.3 Rivers established a new style in ethnographic description which made a world-wide impact. He also established the study of social organisation on a sound basis: he describes his method as the

"formulation of a working hypothetical scheme to form a framework into which the facts are fitted, and the scheme is regarded as satisfactory only if the facts can thus be fitted so as to form a coherent whole, all parts of which are consistent with one another" (1914: II, 586).

He even sees such a hypothesis as more than a way of accounting for known facts for it must be "capable of accounting for all the known facts and for new facts discovered after the scheme has been fully formulated" (ibid.: 587). As a formulation of method there is not much here one would disagree with, yet looking at his work the impression prevails that he is looking for confirmation of his thesis rather than for verification of a hypothesis. It would seem that the idea of the connection with and dependence upon social organisation of the system of relationships pre-empts the final interpretation by being, unconsciously no doubt, selective in collecting the evidence. When one asks for kin terms on the pattern of a genealogical chart, one is unlikely to get much information about the usage of kin terms in different universes of discourse. And why this limitation should be imposed, is a question which never even seems to cross Rivers' mind.

Another weak point is that he accepts a close "interdependence of language and social function": the two have to be studied in combination. Yet he never even considers, to the best of my knowledge, the logical possibility that the social structure might depend on language rather than the other way round, nor the possibility of an influence working both ways.

While one might feel in sympathy with Rivers when he urges that language should not be studied in total separation from the culture it is part of, one would want to claim enough independence for linguistic studies to have at least equal rights.

2.7 The historical comparative approach.

2.7.1 A survey of the historical approach to the study of kinship terminology would not be complete without an account of the achievements of historical comparative linguistics.

While originally the scholars of the 19th century only wanted to examine and establish relations between certain languages, the growing influence of natural science brought a change of perspective. The example of the classifications arrived at especially in botany gave the impetus to considering language as an organism, a living organism, subject to change and evolution. In the light of this concept classification meant something different from establishing relations between certain languages: it meant tracing back their history to their common point of departure, the "Ursprache". At one time this 'Ursprache', deduced from the languages under comparison, was seen as representing a real language, so much so that in 1868 A. Schleicher wrote a fairy-tale in Indo-European, "The sheep and the horse". Scholars were increasingly concerned with the strict scientific quality of their method; this led to a mechanistic view of language where determinism reigned rather than creativity. It is not difficult to see that

absolute claims and theories could flourish in this climate.

From 'Ursprache' it was but a step to a common culture.

"Wissen wir, dass Völker sprachverwandt sind, so glauben wir zunächst, dass sie stammverwandt seien, dass heisst, dass es eine Zeit gab, wo sie zusammen ein Volk bildeten" (von der Gabelentz, 1901: 293).

Von der Gabelentz sketches the reasoning behind this idea: What various peoples call by the same name, they must have known in the same way, a common vocabulary points to a common stock of ideas, including the original form of civilization. To a large extent these discussions were connected with the question of trying to locate the place of origin of the Indo-Europeans; but other matters were considered as well, notably social organization. It is here that the work of B. Delbrück must be mentioned, "Die indogermanischen Verwandtschaftsnamen, ein Beitrag zur vergleichenden Alterthumskunde" (1890).

In the study of lexical items and their cultural implications two approaches can be distinguished. There was an anthropological orientation, either positivist or philosophical, intent on reconstructing the path of history by taking so-called primitive societies as earlier stages, or by measuring our present forms of social life against ideal forms and concepts. More important for us is the other approach, the philological one, which proceeded by reconstructing the original roots and by interpreting the meaning of those roots. An example of such a root in the field of kinship would be pā = 'to protect', or mā = 'to

create, bring forth', roots which are at the origin of father and mother terms in the various Indo-European languages.

2.7.2 Delbrück's claims in his work on IE kinship terms are in fact very modest and restrained: this is in line with the generally prudent method of the group of Junggrammatiker of which he was a prominent member. He does not indulge in giving a rosy picture of a happy and almost perfect community at the origin, nor does he assume such initial stages as free sexual intercourse developing into matriarchy. He simply wanted to reconstruct the 'Urform', the original form, without wanting to explain such forms. He no longer tried to trace etymologies to roots but he followed meanings in their historical development in tradition. This does not mean that not much can be discovered any more; the restrictions imposed upon reduction to roots do not affect the study of the etymology of derived words, and often it is quite valuable to know whether or not a word occurred at all in IE times. And anyhow, comparative philology is only one aspect for Delbrück, one which has to be complemented by "comparative archaeology".

Delbrück concludes that for the following relationships terms must have existed in IE: husband and wife, the woman as child-bearer, widow (not widower), father and mother (also terms derived from infants' babble), son and daughter; probably too child, brother, and sister; nothing for father's father, but possibly for mother's father and his wife; next grandson and granddaughter, and father's brother (= second father); mother's brother

is less certain, as is aunt; nephews, nieces, and cousins are not represented; a whole set of terms covering relationships between a daughter-in-law and her husband's family: a similar set may have existed for a son-in-law but cannot be traced.

When complementing his philological work with archaeological evidence Delbrück relies totally on what he could ascertain about social organisation in ancient India. The combination of the two leads to his picture of the IE family as a patriarchal monogamous union where the man possessed all authority, married sons kept in close relationship - even locally - with their father's family, and the women left their own families to join the men's families. Originally no relationship between two families resulted from marriage.

The position taken by Delbrück, his method, and his conclusions particularly in the field of kin terms, are representative of their kind with differences on points of detail only, and with some elaboration on points of detail as gradually more information became available. A good example of the method applied to many aspects of culture, including social organisation, is O. Schrader's fascinating book Sprachvergleichung und Urgeschichte in the 3rd revised edition of 1907. Both the general ideas (op.cit.: II, 123-132) and his treatment of kinship, "Die Familie" (ibid.: 303-369), do not differ substantially from Delbrück's; and the same approach and arguments are used by Galton (1957) in his criticism of Isačenko's theories about an original "happy little sex community" as Delbrück used in his criticism of, for

instance, J.J. Bachofen.

2.7.3 An account of the study of kinship systems in the IE field would not be complete without discussing the work of E. Benveniste in his book Le vocabulaire des institutions indo-européennes (1969). Benveniste stresses in his opening remarks (op.cit.: I,7 ff.) that the concept of IE is based on the language factor, and on the language factor alone. While he sees the usefulness of considering common vocabulary as an illustration of a common culture, his own preoccupation is different. Instead of concentrating on the reconstruction of the broad lines of the common vocabulary he directs his attention to specific languages to see how they form and organize their vocabulary for social institutions, and to try and re-establish the context in which what is language-specific, originated. This will show hidden structures, the principle of unity behind divergency, but it will also show how languages go about reorganising their system of distinctive oppositions and renewing the mechanics of their semantics. In an earlier article Benveniste (1954) had insisted already that the context of words had to be used to show how different forms can be meaningfully related: but this is not enough, one has also to explain the difference, point out the elements which are at the origin of a new kind of meaning. He shows this very well, for instance, with the example of caput : testa > a.fr. chef : teste > mod. fr. tête : crâne. While etymological dictionaries usually explain the change from caput to tête as originating in joking usage, Benveniste proposes a different - one might say: an intra-linguistic - explanation on the basis of a very

careful consideration of the contexts of use of caput. In this, as in all his examples, he makes it clear that what is at stake, is a semantic structure consisting of relations. We have here a combination of the historical and the structural in a Saussurean sense. It is in this sense that we must understand: "Nous éclairons par là la signification; d'autres se chargeront de la désignation" (1969: I, 10). The 'signification' refers to the internal structure of the sign, the 'désignation' to the fact that a sign refers to an object, a process, a quality, etc., of the extralinguistic reality as structured by the culture and experience of a given group of men. This means that he does not want to consider the historical and sociological aspects of the process of change in meaning and in the structure of vocabulary, just the linguistic structure itself. Yet it is possible to discern in those changing and developing vocabularies for institutions a reflection of a profound evolution of those institutions, the emergence of new activities or ideas.

It will be clear now that Benveniste does not intend to repeat simply the work of his predecessors on IE kinship terms (cf. 1969: I, 205 ff.). The facts of the common stock in this domain are sufficiently well-known since Delbrück, but there has been little progress since then in terms of a better understanding of those facts. Delbrück had reached two conclusions, first that the kinship structure reflected in the vocabulary is that of a patriarchal society based on descent in the male line and resulting in the so-called extended family, with an ancestor around whom all male descendants are grouped;

and secondly: that kin terms relate to the man; terms which concern women are not numerous, of uncertain meaning, and often morphologically unstable. But the general understanding of kinship systems has improved greatly, and Benveniste wants to reconsider the IE kinship system with these new insights in mind, especially with the view of trying to throw some light on certain anomalies which persisted, in other words looking at what is language specific rather than at the common denominator, though always with reference to the underlying unity of the IE linguistic stock.

His close examination of all the terminological material, available in various sources, leads him to a number of interesting conclusions (ibid.: 274-276). IE society certainly was patriarchal but with remnants of a system where mother's brother dominated: there is e.g. the fact that in Latin mother's brother is called 'avunculus' i.e. 'little avus' while avus strictly speaking means father's father (ibid.: 223-231). The common phase of IE was characterised by classificatory terms which tended to be replaced in different degrees in different languages by descriptive terms. In classical Greek the two co-existed, *φράτερ* from the common stock and *ἀδελφός* newly formed. *Φράτερ* does not refer to a brother by blood, descendant of the same father, but to those who are linked by a mystical parentage, while *ἀδελφός* points to individual descent; *φράτερ* is practically always in the plural, for *ἀδελφός* the singular is quite common (ibid.: 213-214).

But it becomes clear when we look at modern IE languages that a change in terminology does not necessarily imply a change in relationship, nor does the continued existence of a term prove the continuity of a given relationship: sometimes terms have changed to effect a clarification by morphological circumlocution but without change in the reality, e.g. French 'bru' 'belle-fille', while matrilinear 'avunculus' is continued etymologically by bilateral 'oncle' in French, 'uncle' in English.

Benveniste's work complements in a very fortunate manner previous work on IE kinship terminology in two ways. First it shows how the structural approach does not replace the historical approach, but rather offers something of its own, and something which in turn helps a better understanding of the findings of historical research. Secondly it has given a very useful reminder, if ever one was needed, that there are no clear-cut types and systems because so many factors play a role, and what we are looking at usually is a delicate balance, maintained under a variety of influences and pressures. And whatever link there may be between vocabulary and institutions, sometimes they just go their own way.

2.7.4 It is not just by chance or neglect that I have spoken up till now under this heading of historical comparative linguistics, only of studies in the field of IE languages: this reflects the privileged position of this group due to the fact that it has written records going back more than three millennia. Characteristic of Benveniste's work was his use of context in the study of

meaning, the kind of context provided by and presupposing written texts. Language families without such a heritage are far more difficult to reconstruct though it is not impossible to achieve results. One recent example of how this can be done, and done more particularly with reference to kinship terminology, is given by I. Dyen and D.F. Aberle in their study Lexical Reconstruction. The Case of the Proto-Athapaskan Kinship System (1974), which sums up and takes further all the work done on PA previously. It would be impossible to review this book here in its entirety, but I would like to record, and briefly comment upon, a few issues of methodology.

The authors define lexical reconstruction as follows: (op.cit.: 7):

"Given that a proto-language had a particular meaning, which one of its reconstructible morpheme sequences most probably had this meaning? The determination of the proto-morpheme sequence which probably had a particular meaning is lexical reconstruction".

This is of particular interest in the area of kinship terminology. There are in a relationship system really two systems: a set of patterns of behaviour, and a terminological system. These two are correlated, behaviour is patterned in relation to the terminological system. Now anthropologists are interested in reconstructing the kinship system as it might have existed at the time of a proto-language, but it is impossible to infer the patterns of behaviour directly: the terminology, however, can be reconstructed at least in large part, hence this particular interest.

Methodologically, therefore, the principal interest of such a lexical reconstruction lies in the possibility that other features of culture can be predicted from a knowledge of kinship terminology (op.cit.: 71 ff.). The authors justify this possibility by saying that, if other features can in fact be predicted in cultures we have ethnographic records of, it seems reasonable to consider reconstructed systems as living systems and consequently make a similar kind of prediction possible for these proto-systems.

Inferences can be made from kin terminology about kinship organization either through intuition and experience, or through predictions based on statistical data concerning the co-occurrence of certain terminological and social features. The latter is preferred as being more secure in its outcome, the former fails mainly because the results are never put to the test of the falsification principle. It is not as though any inference from statistics would be an acceptable prediction. First of all it must be correct in more than 50% of the cases, secondly the predictions must represent an improvement over the base rate of the phenomenon being predicted, thirdly the difference between the base rate and the rate predicted must be a significant one, fourthly the prediction must be one that aligns all positive associations between terminology and the other attribute in question. It is only when all these criteria are met and secure predictions emerge that it can be argued that terminological systems do serve as useful predictors of other cultural attributes.

It would seem, however, that we do not know enough

to justify us speaking about systems of terminology and of relationships; this would in fact mean that changes of any given kin type would imply changes of all other kin types, and we know too little to be able to say that. It would seem more likely that there are several systems within so-called kinship systems (op.cit.: 119). Consequently, inferences will be about various features of social life from terminological features (op.cit.: 120f.). These inferences will be on the basis of statistical demonstration, showing an association of a particular pattern of kinship classification with certain social features in a large enough number of cases, or being able to attach probability to a single event. Only when statistical exploration has not been completed for certain terminological patterns does one rely on inference based on experience and intuition. In this way one can arrive at secure conclusions about the social organization of the PA speech community (op.cit.: 133 f.).

2.7.5 I have not gone into the wealth of data and the extensive elaboration of argument of the book so that, obviously, any real criticism would be unjustified and meaningless. But I would like to conclude with some reflections which came to mind while reading in the book, and some of which are also conclusions for the whole of this section.

It would seem that the term 'reconstruction' is unfortunate though it would be useless to try and change it. The word always suggests something real if no longer in existence; but, strictly speaking, 'reconstruction' only says: two (or more) languages are related and their

actual differences can be explained in some regular way. It does not say anything positive about the precise reality the proto-language must have been, though it will show up certain features. Bloomfield pointed out already (1933: 318 f.) that a reconstruction is made on the assumption of absolutely uniform speech-communities and sudden, sharp cleavages. Neither of these conditions is ever fully realised.

If the linguistic reconstruction does not represent reality, cultural inferences must be interpreted with similar reservation. Delbrück and Schrader were already aware of the problem up to a point and insisted that conclusions drawn from linguistic premises need to be confirmed by archaeological evidence, archaeological taken in its widest possible sense. All we can do is to construct a hypothetical world in which all known facts make sense, and this hypothesis must be tested by every possible means. But the available facts and means are so restricted that we can never hope for a complete picture, just some sketchy lines.

A very awkward problem is that of 'meaning'. Dyen and Aberle say in their definition of reconstruction: "Given that a proto-language had a particular meaning ..." (op.cit.: 7). While it is quite clear that the domain of kinship terms is very stable, universal as a domain, and somehow related with biological facts, it does seem to me that even in this domain one must be very careful not to transfer present meanings of daughter languages onto the proto-language: this is always very dangerous.

It prejudices moreover the outcome of the debate if, or to what extent, kinship meanings are socially based and language specific, or biologically based and more universal.

The use of statistical techniques in Dyen and Aberle is inspired by the work of G.P. Murdock. I hope to come back on this aspect when discussing Murdock's contribution. Similarly their claim on ethnographic records showing that other features of culture can be predicted from a knowledge of kinship terminology (op.cit.: 71) has to be properly understood and carefully weighed. But this is a matter I intend to deal with in the overall evaluation.

3. The Structural and Functional Approaches.

3.1 Bronislaw Kaspar Malinowski.

3.1.1 It would seem that no other figure is more suited to head a survey of structural-functional method than B. Malinowski. The reason for this is not that his theoretical work would be of everlasting value, for it is not. But his influence on the study of anthropology in Britain, including kinship, has been enormous even if not always in a direct, positive manner. Moreover, if a distinction is to be made between functionalism and structuralism there can be no doubt as to who should represent functionalism but the man who called himself "the Arch-Functionalist". Not everybody liked the label, clearly not e.g. someone like Radcliffe-Brown who did not want to be associated with the Functional School of Social Anthropology:

"This Functional School does not really exist; it is a myth invented by Professor Malinowski. He has explained how, to quote his own words, 'the magnificent title of the Functional School of Anthropology has been bestowed by myself, in a way on myself, and to a large extent out of my own sense of irresponsibility' " (Radcliffe-Brown, 1940: 188).

Radcliffe-Brown clearly was not amused by Malinowski's flippancy, though Malinowski did go on to define his idea in a more serious manner:

"This type of theory aims at the explanation of anthropological facts at all levels of development by their function, by the part they play within the integral system of culture, by the manner in which they are related to each other within the system, and by the manner in which this system is related to the physical

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surroundings. It aims at the understanding of the nature of culture, rather than at conjectural reconstructions of its evolution or of past historical events" (Malinowski, 1932: XXIX f.).

This theory hinges completely on field-work: to observe what exists, how it works, and what it means to the native. The culture a field-worker studies must be viewed as a self-contained reality: evolutionary and diffusionist theories are bound to distort reality.

If the quality of a definition depends on its brevity and precision, Malinowski's definition of functionalism cannot be considered as totally satisfactory. Greenberg has studied the notion of function as used in the social sciences and distinguished various uses (1957: 75 ff.). There is the function seen as the contribution to the maintenance of the functioning of a structure as a whole, or organic function. There is the common everyday understanding of the word function whereby it is identified with activity, or activity function; as Malinowski said: "What functions must have a function" (1932: XXXVIII). And there is the internal function where the functional effects are being considered not in their bearing on the functioning of the total structure, but rather on that of some other part which participates in the same structure. Of course, there is nothing necessary about these distinctions; in fact they might have gained by singling out first the rather loose usage of the 'activity function' before distinguishing organic and internal functions. In Malinowski no systematic distinctions are made at all, in fact we find the notion of function

used in all its possible senses, and as a result with a fair degree of vagueness.

I think Malinowski was well aware of this vagueness, and up to a point such vagueness is inevitable in the first stages of a new theory. Perhaps this lack of precision might even be due to the fact that Malinowski's stance was to a certain extent a reaction against evolutionary and diffusionist theories as much as, and perhaps even before, being a new theory. While Malinowski was groping his way towards formulating what he considered to be a satisfactory theory different intellectual streams influenced his thinking. Like the people of the 19th century he was fascinated by the achievements of biology and he continued to use the concept of "organism" in social science, albeit no longer in a historical evolutionary sense. This resulted in his functional theory being "holistic", i.e., considering functions as referring to the whole organism, and in the basic needs - to which functions relate in his view - being biological needs. Malinowski also was a rationalist in the 19th century style who rejected the idea that people of technologically less advanced cultures would be illogical or prelogical: though he refers to them all the time as "savages", he claims for them a rationality underlying their behaviour which at times seems exaggerated to the point of being superhuman. But while Malinowski was firmly established in the biological, materialist, and rationalist ways of thinking into which he was born and educated, he shared with others a feeling of uneasiness about some of the consequences and effects of it all,

especially the mechanistic (and collectivist) way in which the individual human mind was supposed to work.

Leach (1957: 121 ff.) has pointed out similarities between the work of the American psychologist-philosopher William James and that of Malinowski. James popularized the philosophy of C.S. Peirce, known as pragmatism. In James' version pragmatism holds roughly that 'truth' is 'what works', a very inadequate interpretation of Peirce's criterion for testing the truth of statements or beliefs by the 'effects that might conceivably have practical bearings'. W.B. Gallie has summed up James' fundamental point as follows (1952: 25):

"from the plausible thesis that certain biological interests underlie, or provide some of the necessary conditions of, all our thinking, he passed to the more exciting (and more ambiguous) thesis that the sole function of thought is to satisfy certain interests of the organism, and that truth consists in such thinking as satisfies these interests".

Leach suggests that by replacing 'thinking' by 'behaving' and 'thought' by 'behaviour' one obtains a perfect summary of Malinowski's functionalism. And I suppose - though Leach does not say so - that 'truth' could be replaced by 'correct or adequate behaviour'. As a philosopher James remained the psychologist he was by training and as such was interested in thoughts as elements or phases in the life-history of this or that particular individual; he had difficulty in transcending the particular. Malinowski showed the same interest and preference in his social studies, strong on description, weak on theory and generalization.

3.1.2 Against this general background it becomes much easier to understand Malinowski's view of kinship. There is no single comprehensive work by him on the subject: the book he kept promising, "The Psychology of Kinship", was never written. But the subject is present throughout his work, and in his article in *Man* (1930), entitled "Kinship" we have a clear if perhaps slightly extreme, summary of his views. In it he takes to task not only his predecessors with their gratuitous historical explanations but also his contemporaries for what he considers to be almost an obsession with, an unwarranted indulging in, long and complicated terminological systems of kinship. For, while everyone continued to feel that the matter of terminological systems was all-important, Malinowski wanted to get away from this formalizing and hypothesizing. "After all" he said, "kinship is a matter of flesh and blood, the result of sexual passion and maternal affection, of long intimate daily life, and of a host of personal intimate interests" (1930: 19).

I have said before that Malinowski was like James in his interest in the life-history of this or that particular individual. In kinship this resulted in what he called the "biographical method" (*ibid.*, 23 footnote). In this respect he is truly pragmatic:

"The modern or functional anthropologist proposes, therefore, to understand what kinship really means to the native: he wishes to grasp how terminologies of kinship are used and what they express" (*ibid.*, 22).

The word 'really' is significant in this quotation, it illustrates a recurrent theme in Malinowski's writings,

the opposition between 'real' and 'ideal'. Structures, laws, rules are all 'ideal'; what people do, say, experience is 'real'. Malinowski's approach is psychological rather than sociological; consequently, questions of right and duty are secondary to emotion and sentiment.

This very same question determines his view on kinship terminology, where he asks: Can we come nearer to the real core of kinship by the mere use of mock-algebra? And his answer is: "There is a vast gulf between the pseudo-mathematical treatment of the too-learned anthropologist and the real facts of savage life" (ibid., 20). Where anthropology went wrong to his mind was in basing kinship on the clan rather than on the family, and in its interpretation of the significance of classificatory systems, especially in connection with anomalous forms of marriage. As far as kin terms are concerned Malinowski holds that their linguistic nature has been misunderstood. They are not the petrified remains of a previous social state, they are in fact "the most active and effective expressions of human relationship, expressions which start in early childhood, which accompany human intercourse throughout life, which embody all the most personal, passionate, and intimate sentiments of a man or woman" (ibid., 22).

Before specifying Malinowski's ideas on kinship terminology, particularly on classificatory terminology, it is necessary to explain first his view on the other moot point, the place of clan and family in anthropological discussion. Malinowski refuses to see the clan and family as domestic institutions which would occur at distinct stages

of development: for him they appear invariably together. And while the family can exist alone, the clan cannot and is therefore an additional institution. The family is the domestic institution par excellence, bonds of clan-ship develop only much later in life, out of the primary kinship of the family. Seen in this way, kinship as observed in an adult tribesman is the result of a long process of extensions and transformations, starting in early life with the physiological event of procreation, but profoundly modified in human society by cultural influences. This calls for kinship to be studied in all its aspects - including terminology - as a process in development and not merely as a fixed product. This process starts off with the two parents and their offspring, as much a cultural as a physiological reality: Malinowski calls this the initial situation. In his analysis of the facts he finds that the communal interpretation of this initial situation is definitely erroneous: "every human being starts his sociological career within the small family group, and (...) whatever kinship might become later in life, it is always individual kinship at first" (ibid., 25). But parenthood is interesting not only in itself, but more so in that it is the starting point of most other relationships between men. This consequently is what the study of kinship is about: to examine the processes of the extension of kinship from its extremely simple beginnings in plain parenthood, to its manifold ramifications and complexities in adult membership of tribe, clan, and local group. There are really two correlated processes. The first process is one of

consolidation and extension of family ties. What is meant by this is that the individual relation of offspring to their parents always remains one of the dominant sentiments in human life, manifesting itself in moral rules, in legal obligations, in religious ritual; this relation undergoes shocks and diminutions at times, at other times becomes reaffirmed again, from the initial physiological dependence of the infant through the age of education, adolescence, marriage, right up to old age. The second process is one of disruption, in which the group or communal character of human relations is emphasized at the expense of the individual character: for Malinowski this is not really a negation of kinship, rather a one-sided distortion of the original parental relationship when clan identity becomes more prominent in certain phases of tribal life. But as the clan is essentially a non-reproductive, non-sexual and non-parental group, it never is the primary source and basis of kinship: its functions are mostly legal and ceremonial, at times also magical and economic. Kinship always rests on and begins with the family, the parents and their offspring: the function of this institution is the procreation, the early cares, and the elementary training of the offspring. I have given this summary of Malinowski's thoughts on kinship because it is essential to understand his claim that "any problem starting from the classificatory nature of kinship terminologies, must be spurious because the plain fact is that classificatory terminologies do not exist and never could have existed" (ibid., 21-22). Though he does not elaborate on this point, I think the following quotation

from his exposé on kinship clarifies his thinking perfectly:

"Words grow out of life, and kinship words are nothing else but counters or labels for social relations. Even as, sociologically, kinship is a compound and complex network of ties, so every native nomenclature consists of several layers or systems of kinship designations. One system is used only to the parents and members of the household. Another stratum of kinship appellations is extended to the next nearest circle of relatives, the mother's sister and brother, the father's brother and sister, their offspring and the grandparents. Yet another type of kinship words applies to the wider relatives of the immediate neighbourhood. Finally there are kinship words used in a truly classificatory sense, based partly but never completely on the distinctions of clan-ship. The sounds used in these different senses are the same, but the uses, that is the meanings, are distinct. Each use, moreover, the individual, the extended, the local and the classificatory, are differentiated by phonetic distinctions, however slight, by fixed circumlocutions, and by contextual indices. It is only through the extraordinary incompetence of the linguistic treatment in kinship terminologies that the compound character of primitive terminologies has, so far, been completely overlooked. 'Classificatory terminologies' really do not exist" (ibid., 28-29).

This is not just a tentative statement by Malinowski, it is a recurrent theme:

"To 'correlate' kinship terms with kinship facts is based on the mistaken assumption that when there is one term for two people these two people must somehow be lumped together or telescoped or united in the mind of the native, or even that they must be one and the same person" (1935: II, 65, footnote 2).

And his treatment of Trobriand kinship terminology is wholly done along these lines. In the Table of the Genealogical Chart (1932: 435) he writes in capital letters the terms for the nearest family relationships: Fa, Mo, e and y Br, Si, Ch, GrCh. With these relationships go precise patterns of behaviour, especially concerning sexual taboos.

Gradually other persons enter into this circle of kinsmen, and kin terms are extended to refer to them.

"But there can be no doubt that the new use of the word remains always what it is, an extension and a metaphor" (1932: 443). With this gradual extension in meaning goes a corresponding change in emotional content. And as one passes from the "secondary" relations to more distant relatives, the intimacy of the bond and the stringency of the taboo falls off rapidly. And this extension goes as far as the whole clan, or even beyond as in the case of tabugu which not only means GrPa, GrCh, FaSi, FaSiDa, but eventually even "lawful woman", so extending over three clans.

3.1.3 Malinowski gave a lot of thought to linguistic matters, and he came up with some quite surprising ideas, though perhaps not as surprising as all that in the light of his general intellectual views. As his interest was more psychological than sociological, one could expect him to see the interest of getting at thought, at a mentality, through language:

"The study of the linguistic aspect is indispensable, especially if we want to grasp the social psychology of a tribe, i.e. their manner of thinking, in so far as it is conditioned by the peculiarities of their culture" (1920: 33).

It is not so much that language would express ideas or embody concepts and categories: but it stands in some definite relation to the life of the people who speak it and to their mental habits and attitudes (cf. 1935: II,6). Malinowski remains vague here: in both publications quoted he makes clear that this correlation is not too well under-

stood. He is notably unclear on the idea of language mirroring reality: in Coral Gardens (1935: II,65) he says unequivocally "Language does not simply mirror reality", while in the Supplement to The Meaning of Meaning (1923: 327) he had written "Language in its structure mirrors the real categories derived from practical attitudes of the child and of primitive or natural man to the surrounding world". Perhaps there is less of a contradiction than some people have thought: the second quotation, I think, refers not to the external reality, but to "real categories", or as he put it elsewhere in Coral Gardens : language reflects or duplicates "the mental reality" of man (1935: II,7).

While this psychological interest in thought and mentality remained, there was an ever increasing interest in behaviour. Malinowski became very insistent that language is not about communicating ideas because that was one step removed from the immediate reality: in line with his empiricism, his insistence on direct observation and fieldwork, his pragmatism, he sees language as a "mode of action and not an instrument of reflection". Language serves a practical purpose, has a function: and this function is its meaning: "It is the function, the active and effective influence of a word within a given context which constitutes its meaning" (1935: II,52). All his concerns with language eventually lead back to meaning, even his grammatical considerations, and meaning in the sense of 'translation-meaning', i.e. meaning for the ethnographer-outsider who is trying to understand another culture. There is a basic ethnocentrism about his approach:

in this sense we must see his rejection of the Saussurean type of structuralism where "langue" counts, not "parole". For Malinowski it is "parole" which matters (cf. 1937: 63), and this explains his reference to an actual context. The study of language cannot be autonomous but must be done in the actual context of situation. Utterances have meaning through a context of culture or of reference, i.e. they refer to a definite subject-matter, and they have meaning through a context of situation in which they achieve an immediate practical effect (cf. 1935: II, 51 f.). This meaning in context which could be called the pragmatic meaning, is considered to be the primary meaning. It is in relation to this notion of primary meaning that we have to see Malinowski's treatment of the multiplicity of meanings, of homonyms, among other things in the field of classificatory kinship terms. He distinguishes cognate homonyms which are used in different but allied meanings, from accidental homonyms which have nothing to do with each other semantically but have the same sound by sheer accident. Kinship terms belong to the latter group where one should not try to find a common, vague, confused, meaning by lumping homonyms together: they should be considered as a series of distinguishable linguistic units (cf. 1935: II, 20,28). While he calls them accidental homonyms, he does not say that they are not in origin the same words, the same sound-complexes: it is here that his notion of extension comes in. What happens, according to Malinowski, is that a new meaning is created by use in a different context, but this is achieved by extending the meaning of an existing sound-

complex. This has the advantage of creating an atmosphere of familiarity within which the new element can be placed in a satisfactory way, conducive to the necessary pragmatic response (cf. 1935: II, 68-72).

It would be tempting to call Malinowski's cognate homonyms examples of polysemy, and his accidental homonyms instances of homonymy. In that case, however, kinship terms would be homonymous in the sense linguists generally use this word; that would hardly be acceptable to anybody. I am not sure that this is what Malinowski wanted to say. He was probably talking about extensionism in terms of a theory of social learning, not of linguistic semantics (cf. Buchler and Selby, 1968: 4 ff.). One might possibly reproach him, though, that he thought that a study of the way children learn extended meanings was a proper linguistic analysis of those meanings.

Malinowski's attention for the individual and the psychological as opposed to the collective and sociological, was influential though it remains to be seen whether it is a matter of temperament and preference, of emphasis, or something more far-reaching.

The matter of extensionism is equally of lasting importance though the nature of this extension process will be defined in a different way (cf. below 4.4.3). The matter of how to determine what should be considered as the primary meaning has not yet been solved conclusively.

3.2 A.R. Radcliffe-Brown.

3.2.1 A.R. Radcliffe-Brown was completely at one with Malinowski in opposing what he called "conjectural history", but - as I have indicated - he did not like to be called a functionalist (cf. Radcliffe-Brown, 1940: 188). He differed from Malinowski on two points: he was marked more deeply by ideas of the natural, exact sciences, and he preferred a sociological to a psychological approach.

He defined Social Anthropology as the theoretical natural science of human society, i.e., the investigation of social phenomena by methods essentially similar to those used in the physical and biological sciences; and as he understands by natural science the systematic investigation of the structure of the universe as revealed to us through our senses, so anthropology must study the social structure which gives unity to the social phenomena we observe in human society (cf. 1940: 189 ff.). This involves a careful collecting of data through field-work, and these data must then be interpreted. Here it was thought, both by e.g. Rivers and Kroeber in the Historical School, and by Malinowski in the Functional School, that the psychologist would be the proper person to undertake this systematic interpretation, an interpretation in terms of processes of individual mental activity; Radcliffe-Brown disagrees, he sees it as the task of sociology, a sociology which - in the line of the French School of Durkheim - interprets data of culture by relating them to universal laws. And as these laws can only be discovered by the comparative method, by the study and comparison of many diverse types

of culture, he likes to call his approach 'Comparative Sociology' (cf. 1931: 9,14,17-18).

While Radcliffe-Brown disliked being called a functionalist, the notion of 'function' played a role in his method next to the concept of structure:

"The social life of the community is here defined as the functioning of the social structure. The function of any recurrent activity, such as the punishment of a crime, or a funeral ceremony, is the part it plays in the social life as a whole and therefore the contribution it makes to the maintenance of the structural continuity. The concept of function as here defined thus involves the notion of a structure consisting of a set of relations amongst unit entities, the continuity of the structure being maintained by a life-process made up of the activities of the constituent units" (1935: 180).

This concept is clearly based on the hypothesis of the functional unity of a social system, i.e. of the total social structure of a society, together with the totality of social usages in which that structure appears and on which it depends for its continued existence.

3.2.2 It is these general ideas we find reflected in Radcliffe-Brown's views on kinship and kinship terminology.

"To understand any kinship system it is necessary to carry out an analysis in terms of social structure and social function. The components of social structures are human beings, and a structure is an arrangement of persons in relationships institutionally defined and regulated. The social function of any feature of a system is its relation to the structure and its continuance and stability, not its relation to the biological needs of individuals" (1950: 82).

The last remark about the 'biological needs' is clearly directed against Malinowski though he does not mention

him by name; he only mentions Kroeber's name as a matter of fact when attacking the psychological approach to kinship terminology (cf. 1930/31: 427). Where they agree is in rejecting Morgan's hypothetical historical approach (ibid. 426), as well as Rivers' version of it. Radcliffe-Brown points out that the claim that certain forms of social life, of marriage, are at the origin of a certain type of terminology cannot be proved, in fact it might just as well be assumed that it was the terminology that called forms of marriage into being: this would be just as valid a hypothesis and just as unprovable (cf. 1941: 57 f.). What is needed is an explanatory structural principle, not a hypothetical causal link (ibid.: 75, 81).

While for Morgan kinship terms were the crucial elements of his theory, Radcliffe-Brown tends to give a far less prominent position to matters of terminology: several times he insists that kin terms are "part of the system" (e.g. 1930/31: 427; 1941: 53) but only one part among others. They do hold a special position only in so far as they offer "the best possible approach to the investigation and analysis of the kinship system as a whole" (1941: 62), "because they frequently, or indeed usually, reveal the method of ordering relationships" (1950: 10). This role of importance presumes obviously that there exists some sort of link between kinship terminology and social practices, and such is indeed Radcliffe-Brown's position: "There are important correspondences between kinship nomenclature and social practices" (1941: 61), "there is a very thorough functional correlation between the

kinship terminology of any tribe and the social organization of that tribe as it exists at present" (1930/31: 427). Radcliffe-Brown also makes it clear that such an assumption must be demonstrated by field work and comparative analysis, which he claims to have done throughout his studies. And while he does show for a great many cases that the kinship nomenclature is "one very obvious and natural means of distinguishing and classifying a person's kin" (cf. 1950: 23), he does not show (if indeed it can be shown) that there is a necessary correlation. Of course the number of cases in which the correlation can be established, is generally (though usually implicitly) esteemed to be high enough to claim the existence of such a correlation as a general rule, but neither Radcliffe-Brown nor many others seem to have reflected upon the implications of the lack of correlation in certain cases, though Radcliffe-Brown's use of such expressions as "normally" and "as a general rule" would seem to indicate he was aware of the existence of exceptions. In a way it is to be expected in human sciences that theories are seldom without exception as they are dealing with fluctuating, ever-changing realities, all the more unstable because of the ever-present unpredictable element of rules being broken by man, voluntarily, or involuntarily through error or forced by circumstances. But ignoring the incongruities too easily may well lead to circular reasoning and begging the question. There is some suspicion in my mind that this is what happened in the case of Radcliffe-Brown. He claimed that the study and analysis of terminology for kinsmen afforded the best

possible approach to the study of kinship, and he then went on to say: "This, of course, it could not do if there were no real relations of interdependence between the terminology and the rest of the system" (1941: 62), an interdependence he finds confirmed in his studies in the field. While appreciating the difficulties in coming to terms with the analysis of systems so foreign to our understanding, one would have liked to see more caution expressed, or else one might say: No wonder one finds the interdependence confirmed in one's analysis if the presumption of it provided the key to the problem in the first place.

When looking in more detail at his way of dealing with kin terms, we notice some similarity with Malinowski, on the point of primary meaning and its extensions, in Malinowski clearly linked with his psychological approach to the problem. Radcliffe-Brown, who, as we have seen, rejected the psychological approach, seems to have a comparable approach at times: "Every term in an Australian system of terminology may be regarded as having a primary meaning" (1930/31: 45), or:

"This extension from the mother's brother to the other maternal relatives is shown in the Ba-Thonga tribe in the kinship terminology. The term malume, primarily applied to the mother's brother, is extended to the sons of those men, who are also malume" (1924: 29 footnote).

But though they are referred to by the same term and a similar customary behaviour is required towards them, they are not totally equal in all respects: "Within the class of persons denoted by one kinship term, the individual distinguishes degrees of nearness or distance" (1930/31: 45);

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"The attitude and behaviour of a person towards a particular relative is affected not only by the category to which he belongs but also by the degree of nearness or distance of the relationship" (1950: 9). But apart from these points Radcliffe-Brown formulated thoughts which reflect much more clearly his brand of structuralism in connection with classificatory terminology. The function of this kind of terminological system is to serve as a means of establishing and recognising categories of relatives (cf. 1941: 61), this is the "functional correlation between the kinship terminology and the social organisation of a tribe" (cf. 1930/31: 427), a function which allows the social system to work and continue to exist. The precise form of a classificatory terminology has the function to be primarily "a mechanism which facilitates the establishment of wide-range systems of kinship" (1950: 9). In simple language this means that this system assures that all the people one comes into contact with are classifiable and classified, in terms of social structure, in terms of expected behaviour and of rights and duties. But Radcliffe-Brown has been much more specific about the social functions of classificatory systems (cf. 1930/31: 428 ff.). The most important point about such systems is what he calls the principle of equivalence of brothers: this means that if someone stands in a certain relationship to Ego, that person's brother stands in the same relationship to Ego. So in Zulu (Radcliffe-Brown, 1922: 6)

ubaba means Fa and FaBr and FaFaBrSo,

unama means Mo and MoSi and MoMoSiDa.

This implies that all children of all ubaba and umama are Ego's brothers and sisters, umfo watu and udada watu. This function of sociological solidarity brings about stability, it makes the continued existence and efficiency of a social organisation much safer as it does not depend on a bond between individuals but between an individual and a group. In the case of the death of a marriage partner this principle may become operative in such institutions as levirate and sororate. Sapir, as we have seen, saw in the levirate the explanation for the grouping together of a number of men under one kin term in as far as they were all potential husbands to the same woman. Radcliffe-Brown makes it clear that there is no causal link between terminology and special forms of marriage (such as the levirate): "they are both applications of the one structural principle" (1941: 81).

A second principle is the distinction between the father and the mother, and therefore between relatives through the father and relatives through the mother. This distinction can be observed in the difference in closeness in relation between Ego and his father and mother respectively. It also appears in the fact that they clearly belong to separate groups, which is often expressed in the terminology for MoBr and FaSi: to continue with the example from Zulu (Radcliffe-Brown, 1922: 6 f.):

FaSi = Ubaba i.e. "father"

MoBr = umaluma i.e. "male mother"

Another principle is that of distinction as to generation: in simple societies there is a definite need for a clear authority, life depends on unwritten rules and customs being

handed down from one generation to the next. The very survival of the group depends on this. This importance sometimes results in a distinction being made even within one generation, e.g. in Zulu:

umna = eBr (or any son, irrespective of age
vis-à-vis Ego, of FaeBr)

umnawa = yBr

Often this necessary strong authoritarian relationship between parents and children is counterbalanced and compensated for by a very relaxed, familiar relationship between grandparents and grandchildren, a familiarity which almost seems to imply being on a foot of equality. This too can come through in terminological features, e.g. in Fanti, a tribe in Ghana, where there is a reciprocal term nana, 'grandrelative', indicating both GrPa and GrCh (Kronenfeld, 1973: 1579).

Finally there is the important principle of reciprocity in marriage, again a question of maintaining a balance: a group which provides a bride must receive compensation. Sometimes this is achieved by giving a bride-price, sometimes by exchanging women systematically between groups or sub-groups ("moieties") in a highly regulated manner. If the latter option is taken, it may be the case that this reciprocity is also perceived in the kinship terminology: we then often see for example the same term being used to refer to FaSi and MoBrWi, e.g.: in Dieri (South Australia) papa = FaSi and MoBrWi (cf. Korn, 1973: 147 f.).

3.2.3 As I have indicated above, the nature and the extent of the interdependence of terminology and social system is - to my mind - insufficiently clarified. The importance of the study of kinship terms was stressed, some correlation was observed though not a necessary one, more a possible application of an underlying principle: but one had the feeling all the time that much remained to be specified.

Looking at the same question from what Radcliffe-Brown says about language and about meaning may give us some interesting complementary insight into his thinking, but basically we are left with the same impression of a need for further elaboration. The social relevance of language is based on the link between language and a speech-community (cf. 1940: 196). Radcliffe-Brown's general orientation, and his linking speech with a community make his interest in language sociological.

In a general sense he mentions the existence and size of speech-communities; more particularly he discovers social relevance where certain features of a language in a society are not a matter of accident but of history: the spread of language, the unification of a number of separate communities into a single speech-community, the reverse process of subdivision into different speech-communities, difference of speech usage according to different social classes. But what Radcliffe-Brown rejects explicitly is a link of one-sided or mutual determination between the characteristics of a language (phonology, morphology, and even to a great extent vocabulary) and the characteristics of the social structure

of the community within which the language is spoken. Any coincidence here is historical accident. I have difficulty in reconciling this point of view with the importance he attached to the study of that part of vocabulary which is the kinship terminology, as the "best possible approach" to the kinship system as a whole. Radcliffe-Brown's failure to reflect on the consequences of the lack of correlation between the terminology and the social structure to which I have pointed already, may well be at the root of these possibly conflicting statements. On the whole his views on language are not systematically developed. He has dealt briefly with the problem of meaning, setting in parallel the meaning of words and the meaning of elements of culture (cf. 1931: 16f.), but no clear picture emerges about his views on language. He speaks about meaning of a word as "the set of associations that it has with other things in his mind, and therefore the place it occupies in his total thinking, his mental life as a whole. (...) The meaning of a word in a language is constituted by the associations normally clustering around the word within that community". There is a psychological ring about this definition which would have suited Malinowski more than it does the sociologically minded Radcliffe-Brown, it would seem that Radcliffe-Brown may well be reproducing ideas which he really had not digested properly. Sometimes one gets the impression he sees meaning in terms of life of a speech-community, a functional Malinowskian outlook; at other moments he stresses the interrelatedness of the various elements which carry meaning (be it in language or in culture) as

the crucial aspect, a properly structural attitude.

The absence of clearly developed thoughts about language, the fact that he collected kin terms systematically but used them only sparsely in his writings, seem to bear out a criticism which has sometimes been levelled at Radcliffe-Brown that he was interested in kinship terminology only as a means to an end, not as a subject deserving attention for its own sake. And while it would be wrong to deny anthropology the right to look at kinship in terms of social structure primarily, any use made of language material in such a pursuit must respect the special nature of such material.

3.3 Kingsley Davis and W. Lloyd Warner.

3.3.1 When I include a paragraph on the contribution made by K. Davis and W.L. Warner to the development of the study of kinship terminology, I do not want to discuss the whole work of either of them in this respect. I just want to discuss an article they wrote jointly and which constitutes a landmark, pointing the way from Kroeber's thoughts on categories of relationship to present-day formal analysis (Davis and Warner, 1937). It would not seem to have had an immediate impact, but as a witness to an ongoing tradition it cannot remain unmentioned. At the same time it touches explicitly on several points of great importance, points which were rarely dealt with explicitly or assessed on their real significance in earlier studies. We shall consider briefly the views put forward by Davis and Warner in their article, first on the

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issue of kinship, then on language and terminology approached from the field of kinship.

3.3.2 In trying to clarify the discussion about kinship the authors wanted to come back to what they saw as the essentials. They were notably unhappy about kinship always being studied as an index of something else rather than for its own sake: they intend to take up where Kroeber left off in 1909, by presenting a purely internal analysis of kinship. But while their attention is focussed on internal structures, their wider interest lies very much with comparison and typology. Comparison only makes sense on a basis of partial similarity and partial difference: some sort of general framework is required to serve as a yard-stick with which systems can be measured and their differences be established. If one wants to compare the kinship systems of mankind, a universal basis is needed. And the comparative approach is precisely what we are looking for, so that these observations are very relevant. For it would seem that the thesis of the interdependence of language and culture by its very essence is bound to explain how similar things are expressed differently from one language to another: by definition this implies comparison.

The comparative approach was strongly advocated by Radcliffe-Brown and in fact interest in non-Western cultures, in any aspect of those cultures, has always been triggered off by wonder at differences, at the unexpected, which invited closer inspection and comparison. And it has always been assumed in our particular field of kinship that we were studying something basically similar

though different in form and appearance. Where Davis and Warner have special merit, is in making these underlying assumptions explicit and in trying to develop from there a scheme of systematic analysis.

"Kinship is a concept that touches two levels of phenomena. On the one hand it refers to a relatively fixed biological structure, on the other to a relatively variable social pattern based on this biological structure. Hence the place to look for universal elements is on the biological plane. Then an endeavour can be made to find out how these elements are used to build the different concrete sociological structures represented by terminologies" (1937: 62).

Consequently it is this biological structure which they study first, though they mention that the distinction between the biological and the social is "purely theoretical" (ibid.: footnote), the cultural can never be excluded from the biological. They distinguish five elements in this biological order. First there is birth and birth-cycle, connected with reproduction, relationship through birth. As a rule man participates in this process of reproduction in two ways, as a child and as a parent, separated by a cycle of time, by a generation. This parent/child link is the most fundamental one in kinship structure. The next element in kinship structure is the link between siblings. Thirdly comes birth-order between siblings, an element of less significance. Another element is difference in sex, an important one, but a special one in that it extends beyond the kinship group. Finally there is the procreative union of marriage. These five elements provide the skeleton around which a systematic conception of kinship is built:

"Every terminological system will give recognition to them in its own characteristic way. (...) They thus furnish an intrinsic basis for distinguishing between kinship types" (ibid., 64).

They can be divided into two sets, along two axes. Along a vertical axis, indicating time, ascending/descending order, we have the elements of birth-cycle and birth-order; along the horizontal axis we find sibling-link, sexual union, and sex similarity or difference. The sibling-link is not unconnected with the factor time as one has to know at what birth-cycle the sibling-link is situated which binds collateral lines to the central line of Ego. The union of marriage will exist normally between partners of the same birth-cycle or generation, or else it will make the partners equal in this respect socially, in the appreciation of the community.

With the help of these five elements relatives can be located in biological space, measuring their distance to Ego. While there is, statistically speaking, a rough approximation between biological and sociological kinship distance, social space and distance are more difficult to measure. This is the reason why the authors limit themselves to the study of terminological space only, because they see terminology as being closely related to social organization as it is the instrument in which people think about kinship (cf. p.69 ff.).

3.3.3 Davis and Warner do not offer any further direct proof of the close link between terminology and social organisation: this kind of proof can only be found in the testing of hypotheses about the way in which the two

relate. For this kind of testing they provide the analytical framework though their own objectives may be slightly different.

The background to their thinking is that language creates a socially experienced world, using part of reality only: language is not a mirror-image of reality. Each language does this in its own way: this is where comparison is useful (as well as the universal biological framework) to see which elements of reality are selected, which ones are excluded, and on the basis of what principle (p. 69-70). They also draw the conclusion, quite rightly, that if the linguistic order is *sui generis*, it is imperative that each term should be seen as part of the whole set of terms and not in isolation: we have here, in other words, the essence of the concept of a linguistic field (cf. p.80). But there is a certain Bloomfieldian flavour about their conception of linguistics when they seem to consider the study of meaning as being outside linguistics. When discussing primary and combined terms ("merely a linguistic device" in their words) they say: "It is extrinsic to kinship logics, a question of linguistics or word structure rather than of semasiology or word-content" (loc.cit. 74).

In their analysis they then proceed along the lines of Kroeber's categories of relationship, using their five biological elements. And in the same way in which they tried to make the enumeration of elements more fundamental and systematic, they now proceed to clarify first some basic points which had caused much

discussion and misunderstanding following Kroeber's article. They stated that the discussion about the appropriateness of the term 'classificatory', challenged by Kroeber, failed to distinguish between two different conceptions, whether in fact terms were seen as designating persons or relationships. I have pointed out already that Kroeber challenged Morgan on something he never said, and this effort at reconciliation is more directed toward the discussion which had followed Kroeber's article than toward clarifying the original problem. But Davis and Warner have done more than trying to put an end to a discussion; they have developed their line of thought into a more refined distinction between isolating, descriptive, and classificatory terms. They base this distinction on their five biological categories: isolating terms designate one person only and are specified with regard to all five categories (e.g. English "father"); descriptive terms are specific for all major categories, i.e., all except birth-order (e.g. English "son"), while classificatory terms are not specific for one or more of the four major categories (e.g. English "uncle").

Separate from this classification on the basis of word-content come a number of what they call 'linguistic devices', to do with word structure. Here they distinguish: primary terms, i.e. single kinship terms that can be used alone or can be combined with other primary or categorical terms; categorical words that are not by themselves kinship terms but merely symbolize one category of relationship (e.g. 'grand', 'in-law'); and combined terms that are formed by combination of other elements.

An actual analysis of kinship terms will be carried out with reference to the way in which terms represent the biological categories, and must answer the following questions:

- how many categories of relationship does each term in the nomenclature express?
- how many categories does the system as a whole rely upon?
- how does the system combine the categories and subcategories on which it does rely?

Once these questions have been answered and for practical purposes carefully symbolized, one can develop a typology, and correlate types of nomenclature with types of societies and features of kinship with features of social organisation.

3.3.4 In conclusion I think one cannot stress enough the importance of the issues raised by this article. The opposition between the biological and the social aspects of kinship is a crucial but delicate point. I have pointed out already the importance of some kind of framework for objective reference to enable comparison between systems, comparison being essential to any thesis on linguistic relativity. This 'objective' framework would also seem important in relation to the question of primary and secondary or extended meanings of kinship terms, as well as for the precise delimitation of the domain of such terms. This point will have to be carefully examined, for unless the necessary reservations are made and the underlying assumptions clearly perceived, one could only too easily be begging the question.

One of the points to be clarified must be what

to understand by 'biological'. This is not just quibbling about words: I do have the feeling that there is something unsatisfactory about the grouping together of these five elements as 'biological'. Apart from the category 'sex' which is admittedly different in that it extends beyond kinship boundaries, all categories have, or can have, to a greater or lesser extent a social side to them. And even when we consider 'sex' we could take the view, with Radcliffe-Brown, that classificatory systems are wide-range systems which determine our possible sexual relationships with all the people we are likely to meet: in this sense there could even be a social side to the category 'sex' in so far as it contributes to regulate marriages.

From a more strictly linguistic point of view some equally important, and partly related, matters are raised. Several remarks made stress - often implicitly only - the autonomous character of linguistics, and this point of view is worth remembering even when one would want to include semantics in linguistics. The mention of kinship terms forming an interdependent set of terms is a good indication of this. In a similar vein the authors speak of the terminology "bridging the gap" between the biological and the social. In a way this is a fortunate expression because it stresses the link with both without identifying it with either. But it may be useful to deepen this idea and to try to examine how it links up with both and what it achieves in doing so. There is however the problem in the background that the distinction between biological and social may be merely

an analytical-theoretical one (Davis and Warner, 1937: 62; cf. above 3.3.2).

The notion of 'linguistic device' as distinct from kin terms proper is attractive and useful: but one may have to define it more carefully and for each language specifically. For all linguistic signs are arbitrary and, in a sense, devices: there are no existential links between words and realities. What we will want to know is: what can be called a 'device' in a particular language given its process of word-formation, its word-classes, its own internal structure.

3.4 George Peter Murdock.

3.4.1 G.P. Murdock is a figure of some importance in the study of kinship and while his views on the linguistic side of kinship - the kinship terminology - or on the nature and role of language in relation to social structures are not terribly original, there are good reasons for including him in a survey of the study of kinship terminology. These reasons are his influence on students in this field, his particular method, and the assumptions and implications of this method. It is in fact his method which has earned him his influential position.

Murdock did his own anthropological fieldwork on kinship and social behaviour but the main bulk of his work is perhaps best characterized by the epithet 'eclectic' in the sense that he gets his material from various sources, from published data and research reports. It is eclectic furthermore in quite another sense, in that he gets his

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methodological inspiration from different sciences, viz. sociology, historical anthropology, behavioristic psychology, and psychoanalysis (cf. Murdock, 1949: XII-XVII). But there is more to Murdock's work than eclecticism: he uses his material with a specific aim of his own in mind, what he has called the "cross-cultural survey". And it is here that Murdock's original contribution lies.

Cross-cultural studies are a variety of comparative studies; they are, in the words of Francis Hsu (1969: 52), "works involving a few variables in a large number of societies employing statistical techniques". This 'genre' of comparative study represents a respectable tradition in scientific work: the search for explanatory factors and theory which transcend the anecdotal and have a universal value. In social science its field of interest is the whole of human social life in all its dimensions, the dimension of time, of geography, of form of culture. It was this universal interest which was at the root of his work of establishing the "Cross-cultural Survey", later to develop into the "Human Relations Area File", a coded abstract of all available ethnographic material. For what Murdock wanted to establish in social science was the equivalent of the "laws" of natural science. He realised, however, that in social science 'laws' would be more like 'tendencies', verified statements of correlation between social phenomena. And the Cross-cultural Survey was his means of drawing up those statements and putting them to a quantitative test. This quantitative test is necessary in Murdock's view because there is no hope of

discovering laws without exceptions in social science, and experiments on human beings and human society are not possible. A quantitative test is done with the use of statistics, statistics require the tabulation of data, and for this the Cross-cultural Survey provides the means.

It may come as a surprise perhaps to see that Murdock combines an assumption of universality with behaviourism, i.e., with a stimulus/response theory where certain types and forms of culture are the response to stimuli of biological and other needs, a response which is learned through experience after birth and is in fact inculcated through teaching (cf. Murdock, 1940). Murdock reconciles these two things - universality and learned culture in response to given needs - by posing a theory of learning which works on the same principles everywhere: as the principles of learning are the same, the results should reflect a universal common factor; if societies survive there must be something they have in common which makes them survive. Moreover, culture is what Murdock calls "ideational", i.e., it has ideal norms (as distinct from actual behaviour) which are conceptualized - or: verbalized - group habits: the universality of these mental processes guarantees some universality in culture. While Murdock thus assumes that certain factors contribute to the universals underlying all human culture, and consequently make comparison possible and meaningful, other aspects of culture contribute in their way to making the comparative approach recommendable. First there is the fact that culture is what Murdock calls "adaptive", i.e.,

culture changes when adapting to geographical and social environment and to the biological and psychological demands of the human organism. This explains why cultures are different or, if it comes to that, why a surface similarity between two cultures does not necessarily point to both cultures having undergone an identical process of change; for the number of actually used adaptations in the face of social and other demands, tends to be limited. Another aspect of culture is that it is "integrative", i.e., the elements which make up a given culture tend to form a consistent and integrated whole. But as integration takes time, there always is a "cultural lag" (cf. Murdock, 1949: 118).

This is the background to Murdock's Cross-cultural Survey: the assumption of universal elements combined with the adaptive and integrative qualities of culture. And the objective of the Survey is to formulate and test generalizations on the basis of correlations found in different unrelated cultures. The method Murdock uses to present and test the correlation between a particular kinship determinant and the terminological features that it tends to produce, is the postulational method of enquiry (cf. Murdock, 1940; 1949: 127 ff.). This method requires the elaboration of hypotheses which, after rigorous logical analysis, lead to a basic postulate or set of postulates. A postulate will be far too general to be capable of direct validation: therefore one deduces from a postulate as large, as diverse, and as representative a group as possible of testable theorems. Only after having performed all logical or deductive operations does

one apply the empirical tests to verify the theorems quantitatively. An anticipated correlation between two aspects of culture is thus tested in the whole sample and the results, positive and negative, are tabulated and expressed statistically in some way. If the positive correlations prevail in a significant enough quantity, the postulate is considered tentatively verified. Correlation due to historical factors should be discerned and taken into account: a valid cross-cultural hypothesis must hold true under any circumstances. The negative cases must equally be examined: if no explanation can be found for the divergence (such as e.g. cultural lag), the whole principle is weakened and becomes suspect. Basically there are then two tests to be passed for valid generalization: a quantitative statistical test, and an analytical historical test.

3.4.2 The key word, when we come to consider Murdock's analysis of kinship, is adaptation, the adaptive force in culture which is behind the correlations that can be established between different forces at work in the kinship system. At one time Murdock accepted three types of explanation for co-occurrence of elements: genetic (common origin), diffusionist (borrowing as a result of geographical contact), and functional-adaptive (cf. Barnes, 1971: 16/17). More recently he has rejected the validity of results if genetic or diffusionist ties are not ruled out first: cross-cultural studies of a quantitative nature require the comparison of mutually independent cultures in order to ensure that the associations established are truly objective (cf. Murdock, 1967: esp. 111-112).

Even before Murdock developed his thinking to this point he felt that previous types of analysis of kinship were too incidental (cf. 1949: 113 ff.). He sees Kroeber as one of the leading figures among those who explain kinship terminology from multiple historical influences: he agrees to the multiplicity, he is not happy with Kroeber seemingly excluding other than historical factors, especially sociological ones. Because there are limitations de facto to the possible responses which people make to the demands of social life through their systems of kinship terminology, historical explanations alone are inadequate. Differential principles of word formation or linguistic morphology are insufficient explanation by themselves as well: for one thing there are too many examples of related languages with different principles of classification, while the phenomenon of cultural lag reduces moreover considerably the possible use of linguistic indices in this matter. Elementary psychological or logical processes have also been put forward: some people have held that the terms of relationship are expressive of a manner of thought, of association, and of generalization. Murdock does not accept that psychology can explain any cultural phenomenon, it only provides a mechanism by which historical and other influences are translated into patterns of behaviour under particular social conditions; as Murdock puts it:

"The social scientist must resort to the psychologist for answers to the question 'how?'. But for a solution of problems concerned with 'what?', 'when?', 'where?' or even 'why?' he must look to history for the independent variables" (1949: 131).

While Murdock comes down clearly on the side of sociology, against psychology, he has no time for the so-called 'universal sociological principles' from e.g. Radcliffe-Brown: he calls these "mere verbalizations reified into causal forces". He refuses equally to reduce the explanation of determinants of kinship terminology to the influence of a single particular social institution, be it special forms of preferential marriage, or the constitution of kin and local groups. But it is in social institutions - provided they are general, not exceptional like the re-marriage of a widow etc. - that Murdock sees the source of the various types of classification, albeit not in just one institution: the causal factors are always multiple.

We have seen that Murdock saw culture as being integrative, in search of balance: in this same sense we have to see these multiple factors at work, often balancing one another out. The one which finally tips the scales may be fairly insignificant but the real cause is to be found in the whole field of forces. With so many factors operative simultaneously it is to be expected that the statistical correlations will often be far from perfect, further affected by the time lag which we referred to when discussing this integrative character of culture. A moderate statistical coefficient may therefore still reflect genuine and very significant causal relations.

Murdock's understanding of kinship terminology takes as its starting point the nuclear family with its eight characteristic relationships between husband and wife, parents and children, and between elder and younger

siblings. Here the first interpersonal relationships are learned, and this behaviour tends to be 'extended' or 'generalized' to persons outside this nuclear family as time goes on and one's personal contacts are broadened. Normally each human being is part of two nuclear families in different roles at some time during his life, a family of orientation and a family of procreation. This fact gives rise to kinship systems. The relatives within one nuclear family are primary relatives, the primary relatives of one's primary relatives outside the nuclear family are secondary relatives, the primary relatives of secondary relatives who are not primary or secondary relatives to Ego are tertiary relatives, beyond that it is sufficient to speak of distant relatives. Of all these relationships some are affinal, some consanguineal. Of course, a distinctive behavioural pattern towards each potential relative would be impossible; consequently, to make the whole thing manageable the number of categories is reduced by grouping or coalescing. The various ways in which this is done result in the different kinship structures; the behaviour characterizing relationships also contains a verbal element, the terms used to refer to relatives. These kinship terms can be classified in three ways:

- as regards mode of use, a distinction can be made between terms of address and terms of reference: the latter ones are not part of the relationship itself;
- as regards linguistic structure, one can distinguish elementary terms, derivative terms (a kin term plus some other lexical element, not

primarily with a kinship meaning), and descriptive terms (compounds of two or more elementary terms);

- as regards range of application, we have denotative terms which apply only to relatives in a single kinship category as defined by generation, sex, and genealogical connection, and we have classificatory terms which apply to persons of two or more kinship categories as these are defined by generation, sex, and genealogical connection. It should be noted that it is particular kinship terms, not whole systems, which are called 'classificatory', and it is this device especially which reduces the number of kinship categories.

Classificatory terms are possible only if one ignores one or more fundamental distinctions between relatives. Following Kroeber and Lowie, Murdock recognizes nine criteria in kinship terms, six major ones (generation, sex, affinity, collaterality, bifurcation, polarity), and three subsidiary ones (relative age, speaker's sex, decedence). Ignoring any one of these produces classificatory terms. Important as these criteria are for the analysis of kinship, they do not of themselves explain differences in kinship terminology: such explanation can only be derived from discovering which factors led people to select the particular set of criteria they use for differentiating or equating certain categories of kinsmen.

Meanwhile it remains true that kin terms play a role in the relationship between relatives. The terms

of reference denote a status, and status can be defined in terms of expected behaviour: this is sufficient reason to assume that there must be some sort of fairly close functional congruity between terms of reference and the way people interact. This congruity is an empirical generalization but is not absolute: behaviour may be different towards people referred to by the same kin term, or in other cases a different term may be used to refer to different people towards whom one behaves in identical fashion. Moreover, behaviour patterns are not as sharply differentiated from one another as the associated terms are, nor are their differences across cultures necessarily comparable: bilateral societies tend e.g. to have far less differentiated behaviour patterns than unilinearly organized societies with their complex forms. This is how Murdock concludes and summarizes his findings:

"That kinship nomenclature is closely correlated with culturally patterned norms of behavior toward relatives must be assumed. This assumption accords with 'a priori' reasoning, with the overwhelming testimony of the data surveyed for the present study, and with the experience and the declared or admitted views of nearly all competent anthropological authorities. Further exploration of the subject would become primarily an exercise in semantics, a study of the relation between words and the things they denote. Moreover, it would be irrelevant, for the real scientific problem is not to derive terminology from patterned behavior, or 'vice versa', but to explain both phenomena on the basis of causal factors lying outside of the kinship complex. (...) Such factors can be expected to exert an influence on both behavior patterns and nomenclature. In some cases they may affect both at the same time and in like degree. In others they may change initially only the patterns of kinship behavior, setting in motion an adaptive process which with the passage of time

produces congruent modifications in terminology. Sometimes, perhaps, they may even alter first the kinship terms, with behavior undergoing subsequent adjustment, but this is probably relatively rare since new words and new meanings of old words do not ordinarily precede the things they designate. In any event, the ultimate effect of an outside causative factor is to alter both relationships and terminology, which always retain their essential integration" (Murdock, 1949: 112).

It is by these outside causative factors that we can account for the cross-cultural differences in kinship terminology, and these factors must be non-universal features of social structure, especially in Murdock's view, economic factors (cf. 1949: 137). These factors are called social differentials and social equalizers. The interplay of the inherent analytical categories and the cultural factors accounts for the extension and differentiation of kinship terminology. This is the essence of Murdock's Postulate (1949: 138) which he then puts to the test quantitatively in 30 theorems and propositions (ibid., p.139-179).

3.4.3 In assessing Murdock's work on kinship two points must be noted. The first is that, although the analysis of kinship terminology holds a place of some importance in his studies, there is not much that would interest a linguist directly, not in his analysis of terminological systems nor in his ideas about the nature and role of language generally. But on the other hand there are aspects in his contribution which highlight to my mind real problems in the methodology of a certain type of study, not just in Murdock's field of anthropology but also, *mutatis mutandis*, in the field of linguistics. I am thinking here especially of such issues as comparativism, universals (cultural or

linguistic), and what constitutes an explanation or what is mere restatement of facts.

Murdock's concept of culture has come in for a fair amount of criticism and though it would be possible to establish a parallel with problems in delimiting the notion of language, I prefer to confine myself to pointing out those points of criticism which would appear to be more relevant in the framework of this present study. Murdock does not always make a clear distinction between culture and society but in fact the two do not always coincide. There may be more cultures within one society. Equally there may be more than one language in one cultural area, or the same language or variants of one language may be spoken over a culturally diversified area. In Murdock's case this is at least partly explained by his exclusive concentration on small, simple, societies, bypassing our more complex industrialized societies. The study of kinship terminology would seem to suffer from a similar lack of balance: the amount of work done on the kin terms of simple societies, certainly from the point of view of a possible link between terminology and social structures, would seem to outweigh too heavily work done on those matters in respect of complex societies. Related with this is Murdock's implied assumption that cultural norms and actual behaviour coincide: while life at close quarters in a small society certainly leaves less room for deviations from standard behaviour, it still remains a question in how far this assumption is justified. What is of interest for us is that Murdock holds that culture is adaptive where others might feel that it is rather in the

actual behaviour that the adapting takes place: this insistence on ideal norms rather than on actual behaviour (though not consistently pursued) clearly is predominant in his view of kinship and kinship terminology. Murdock tends to neglect the importance of environment in a wide sense in various aspects of culture, something which cannot be neglected, I feel, when studying the social relevance of kinship terminology.

As regards his method and his type of comparativism Murdock has been accused of not making comparisons but just providing illustrations (cf. Evans-Pritchard, 1963). This is not entirely fair. There are basically two ways of doing comparative work. One can work on the basis of a few cultures and formulate generalizations which are gradually refined to be ever more widely applicable, or one can simply state general associations between elements of culture and measure the strength of these associations. Murdock favours the latter approach, he looks - as Barnes (1971) put it - for "Safety in Numbers", and in order to achieve this he uses a statistical method. It may be good to point out briefly the possibilities and limitations of the use of statistics.

In general methodology statistics comes under the simplest form of scientific method, what Max Black (1962: 220) has called the 'scale model'. Such a model is a representation of the real thing with which it has a likeness and of which it preserves the relative proportions: but it changes the size by either reducing or magnifying it. By definition this implies that there is no such thing as a perfectly faithful model: it only is possible to produce

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a scale model by being unfaithful, i.e., by ignoring some irrelevant or unimportant features. There is a partial identity only of properties combined with an invariance of proportionality. Black describes the usefulness of this type of model as follows (op.cit., p.221): "We try to bring the remote and the unknown to our own level of middle-sized existence". But one has to be very careful in 'reading' such models, for a change of scale inevitably introduces some distortion and, more seriously, it may upset the balance of factors in the original. Hence inferences from any scale model to the original are always precarious and in need of supplementary affirmation and correction. Murdock knows this, and that is why he does not speak of 'certainties' but of 'tendencies'. I wonder if we should not approach it from the other side altogether and say that the certainties we do obtain in this way are negative: for a negative outcome is conclusive, a positive one is only provisional or partial and in any case requiring interpretation. One of the factors which make such reservations necessary is the fact that an element which is identical in external appearance in two cultures, may not be suited for comparison and association because of a basic difference in its underlying cause. And the final question remains: is a statistical association really an explanation or rather a way of re-arranging the facts and making them more accessible while they still require an explanation? I would be inclined to accept the latter answer; and if this is true, it would reduce the value of the statistical method and put it into perspective.

Apart from these general reflections on the use and usefulness of the statistical method there are some particular points which have attracted much attention. A basic problem is that of the sample used. The one used for 'Social Structure' (Murdock, 1949) was not satisfactory and Murdock has worked on improving it since (cf. Murdock, 1966/1967/1968), but the way he went about it has brought a new wave of criticism. And one could even ask the more fundamental question whether there really is such a thing (either a natural or an analytical unit) as a universe of human cultures. But these questions are beyond the scope of this work.

Another question is that of coding, of the choice of categories according to which cultures are compared, i.e., the columns in the 'Human Relations Area File'. This obviously is a particular problem of Murdock's venture, but it would seem there is a general lesson to be drawn from it. From a practical point of view it reminds us of the dangers of ethnocentrism in selecting features in a feature analysis. All too easily categories are selected for convenience of application cross-culturally: in the process a great deal of information may be lost as less attention is given to what is particular to one or a few cases only in favour of what is universally found. Related to this is the requirement of a universal framework for cross-cultural comparison. It has been suggested by Kluckhohn (1962: 314) that, in order to validate cross-cultural comparison in Anthropology, biological, psychological, and sociosituational universals must be established. I believe the same requirement holds for

comparative ethnolinguistic research, though the precise nature of this universal frame of reference will have to be specified further, and specified ad hoc.

Finally Murdock's theory of balance between the elements of a social system draws attention to the problem of synchrony vs. diachrony. For on the one hand his analyses are so to speak a-temporal or, as it is sometimes phrased, in the ethnographic present. But the notion of balance introduces a time factor since changes take place all the time and a new balance has to be found after each change. Murdock's interest is indeed very much in the evolution of social structures: this is shown by the fact that he sees no point in internal structural analyses. An analysis only makes sense if it refers to causes outside the structure itself. This, by the way, rules out the usefulness of 'linguistic field' studies in Murdock's view, at least by implication (cf. 1949: 135). This means further that imperfections, deviations from the balanced ideal, are seen as by-products of a process of transition rather than as a proper element of structure to be accounted for: this focuses our attention on the problem of the exceptions for which any cross-cultural theory must account.

Summing up then, I would say that Murdock's great merit lies in the range of material he opens up and in the challenge of his assumptions, rather than in the depths of his insights.

3.5 Claude Lévi-Strauss.

3.5.0 There are two reasons for including C. Lévi-Strauss in this survey. One might perhaps think here of the fact that he is a figure of major importance in the study of kinship; this fact alone, however, would not be sufficient to justify inclusion: other prominent scholars in the field have been left out. But first of all Lévi-Strauss has made a highly original contribution - albeit a highly controversial one as well - to the study of the link between linguistics and anthropology, of how they must be integrated to provide an understanding and interpretation of human society and communication. And secondly discussion of his work is required to complete the picture of universals and cross-cultural research represented in G.P. Murdock's work.

B. Scholte has compared the two approaches (Scholte, 1966), and though his aim of bringing the representatives of both schools of thought closer together may not have been an unqualified success, the value of his description of the state of the problem in its broad outlines cannot, to my mind, be questioned. He distinguishes an empiricist and a rationalist tradition. The empiricist tradition concentrates on the observable behavioural acts and favours quantitative and descriptive procedures; explanation is a question of quantitative statistical evidence, to explain is to anticipate. Empiricism is inductive and insists on verification. For rationalism ideas and actions derive from fundamental categories of the human mind: the permanent structure of the human spirit

ultimately explains human behaviour. Observable data are not of themselves intelligible but require a supra-empirical model for their explanation: eventually one has to return to the data, but the usefulness of models lies precisely in the fact that they tell us something more and differently from the initial data. Verification and falsification against the observed facts are not the ultimate. In last instance this goes back to the problem of the subjective/objective distinction, or more precisely to its denial: it is held that all our observation is coloured by subjectivity, there is no such thing as objectivity, and consequently there is no possibility of objective verification through reference to observed facts. The only way we can overcome our subjectivity is not by going across to an empirically established objectivity which is illusory but by transcending our subjectivity. It is in this non-empirical pre-subjectivity that we find reality according to Lévi-Strauss, not in what we observe; the variations we observe are but transformations of this ultimate reality.

3.5.1 Though Lévi-Strauss has contributed directly to the philosophical discussion and, indirectly, has set into motion a much wider debate, it would lead too far to discuss all the relevant issues here. Instead I would like to sketch briefly the more concrete ideas which guided Lévi-Strauss in his work and which would seem to be crucial for a proper understanding of his views on kinship which we shall discuss in the next section.

Lévi-Strauss differs from his fellow anthropologists in that he does not ask the question how society works: he

wants to study man, gain a deeper understanding of the mystery of man. This is a search for what is universal. He recognizes as an anthropologist - what others in their way have recognized, say, as linguists - that there is a great variety of phenomena in the world: people are different, they are organized differently in social life, they speak differently. These differences can be seen along two axes, one of time and one of place. The mistake of the historically orientated scholars of the 19th and early 20th centuries was to equate people living in our time but in far-away places with the people living in our place a long time ago. In his own, rather abstract way Lévi-Strauss is so repeating what other critics of historical methods have been saying or are saying: but Lévi-Strauss does not reject history, on the contrary, he claims to subsume history under his new universalist approach which wants to study man in his totality. He has written about this on many occasions, in connection with kinship studies notably in chapter VII of Les structures élémentaires de la parenté (Lévi-Strauss, 1949), entitled "L'illusion archaïque", and it is possibly the most important theme of his book La pensée sauvage (1962). Synchronic structure and diachronic historical interest are not in opposition, they are complementary, but history is less fundamental than the systematic in human phenomena (cf. Lévi-Strauss, 1962: 308):

"Loin donc que la recherche de l'intelligibilité aboutisse à l'histoire comme à son point d'arrivée, c'est l'histoire qui sert de point de départ pour toute quête de l'intelligibilité. Ainsi qu'on le dit de certaines carrières, l'histoire mène à tout,

mais à condition d'en sortir" (1962: 348).

The difference of perspective between history and ethnology (and this can be extended to social sciences generally which, for Lévi-Strauss, would include linguistics) is that history deals with events, with what is conscious, while ethnology - and the social sciences - concentrate on what is permanent and fixed and unconscious, "l'histoire organisant ses données par rapport aux expressions conscientes, l'ethnologie par rapport aux conditions inconscientes de la vie sociale" (1958: 25). Passing from the conscious to the unconscious means passing from the particular to the general (ibid. 28), it bridges the gap between the subjective and the objective mentioned above (cf. 1950: XXX): in fact the unconscious underlies all social activity of man, individual behaviour is but a modality in time of the universal laws which make up the unconscious activity of the human mind (cf. 1958: 67 and 224).

This notion of the unconscious has come to Lévi-Strauss from linguistics:

"Car c'est la linguistique, et plus particulièrement la linguistique structurale, qui nous a familiarisés (...) avec l'idée que les phénomènes fondamentaux de la vie de l'esprit, ceux qui la conditionnent et déterminent ses formes les plus générales, se situent à l'étage de la pensée inconsciente" (1950: XXXI).

Lévi-Strauss sees a double usefulness in linguistics:

language is a choice field in which to trace those

"structures mentales inconscientes" (ibid. XXXIX), and

linguistics, as the most advanced and most scientific of social studies, may point the way as far as method is

concerned for the other social sciences. For Lévi-Strauss

there is more to this than just a similarity of interest or the fact that some fresh inspiration may be gained from looking at some successful other science. Linguists and social scientists "s'attachent à l'étude du même objet" (1949: 612). This is because they are all working at what one day will be "une vaste science de la communication" (1950: XXXVI), based on what is specifically human, the symbolic function (cf. 1958: 224). What Lévi-Strauss here refers to as 'linguistics' without any further specification is in fact phonology - "la linguistique, conçue comme une étude phonologique" (1949: 611) - more precisely the Prague school of phonology as founded by Trubetzkoy and developed by R. Jakobson: and through the intermediary of these linguists Lévi-Strauss discovered the work of F. de Saussure. Lévi-Strauss's understanding of Prague phonology made him conclude there were four basic aspects of phonology which could revolutionize social science generally as they had revolutionized linguistics: the unconscious nature of the underlying structure, the fact that the important thing is not the elements but the relations between the elements, next the fact that these elements in their mutual relations form a system, and finally that there are universal laws which underlie all specific (sound-) systems. What further attracted Lévi-Strauss about linguistics was the formal way in which rules could be phrased, especially the binary choice coding of the Jakobson/Halle distinctive feature analysis.

3.5.2 When we come to consider more particularly Lévi-Strauss's views on kinship, his conception of the four major tenets of Prague phonology is reflected exactly

in this field:

"comme les phonèmes, les termes de parenté sont des éléments de signification; comme eux, ils n'acquièrent cette signification qu'à la condition de s'intégrer en systèmes; les 'systèmes de parenté', comme les 'systèmes phonologiques', sont élaborés par l'esprit à l'étage de la pensée inconsciente; enfin la récurrence, en des régions éloignées du monde et dans des sociétés profondément différentes, de formes de parenté, règles de mariage, attitudes pareillement prescrites entre certains types de parents, etc., donne à croire, que, dans un cas comme dans l'autre, les phénomènes observables résultent du jeu de lois générales, mais cachées" (1958: 40-41).

Though it would be tempting to equate phonemes and kinship terms and study the latter along the lines of a distinctive feature analysis of the type initiated by Davis and Warner (cf. 3.3), Lévi-Strauss rejects this approach for various reasons (1958: 42-43). He esteems that this kind of analysis is too abstract and instead of bringing us closer to reality moves us away from it; moreover instead of simplifying the system and making it more transparent, it complicates it and makes it more difficult to interpret; and finally it does not explain anything, neither the nature nor the origin of the systems. Basically the reason for rejecting the equation is that kinship terms may be like phonemes but in the end they are words, arbitrary signs, carriers of meaning (ibid. 44).

But a kinship system does not consist of a terminological system only, it is also a behavioural system; and in relation to this Lévi-Strauss comes up with an interesting thought. When the subject of phonology was developed, the function of language - communication - was known; the new discovery concerned its structure. Since Morgan the structural aspect of kinship terminology has been

known, what we ignore is its function; of the behavioural system we know the function which is one of assuring the cohesion and equilibrium of the group, what we do not understand is the nature of the structural relations between the various forms of behaviour. So the parallelism exists, not between phonemes and kin terms, but between forms of behaviour and phonemes (ibid. 44-45). It is this assumed similarity of function, namely communication, between language and social behaviour which Lévi-Strauss develops at length in his major work on kinship, Les structures élémentaires de la parenté (1949), in a very ingenious way. It is all based on the idea of exchange, launched by Marcel Mauss in his Essai sur le don of 1923: man exchanges words in order to communicate through language, he exchanges goods and so creates an economy, and he exchanges women in order to assure, through marriage regulations, the continued existence of the group, which might be threatened by the universal incest prohibition (see for a summary Lévi-Strauss, 1949: 615 f.). Although this theory is interesting in an indirect way for its interdisciplinary character involving linguistics, it does not bring us any further on our topic of kinship terms.

Lévi-Strauss admittedly does not have much to say on this topic but with some difficulty we can piece together some basic ideas which I hope to illustrate further with a contribution by Pierre Maranda. In the light of Lévi-Strauss's epistemology as outlined in 3.5.1 it should not surprise us to read that "the study of kinship systems should remain first and foremost a study of models rather than of empirical realities" (1965: 17). The core

of all kinship systems is the elementary structure of relationships defined in terms of social structure, this in contrast to complex structures where individual appreciation comes in, allowing for a certain freedom of choice. The elementary structure consists of four terms: the wife-giver (MoBr), the wife-receiver (Hu), the wife herself, and - as marriage is orientated towards procreation - the child; between these four persons three kinds of relationship obtain, those of siblingship, of marriage, and of generation (cf. Maranda, 1963 and 1964).

This definitely implies that for Lévi-Strauss a kinship system is a means to an end, viz. "to generate marriage possibilities or impossibilities" (1965: 14). In taking this line Lévi-Strauss separates himself from some students of the kinship field while directly rejecting others. He separates himself from those who concentrate their efforts on studying the internal coherence of kinship systems, mainly those scholars who are working on componential analyses and other forms of formal semantic analysis. He directly rejects the opinions of a number of scholars who, like himself, are interested in the meaning and purpose of kinship systems. He groups the answers given in this matter into three categories: the first group sees a kinship system as the product of one or several efficient causes, psychological or sociological, but always external and heterogeneous; the second category's (e.g. Radcliffe-Brown's) point of view is that of a homologous social configuration, i.e., there is a close correspondence between, on the one hand, the structure of a kinship system and its terminology, and, on the other, the

network of rights and obligations; the third opinion, his own, is that of a kinship system as a final cause, i.e., acting as an operating agent to a system of matrimonial exchange within the community (cf. 1965: 13-14). It is in this context of the element of kinship as a teleological operational unit that we must see his scant references to kinship terminology.

A kinship system regulates marriages which are either prescriptive or preferential: in both cases there is a certain randomness, for in a prescriptive marriage there is still some flexibility as there are normally several possible candidates for a given marriage, while in the case of a preferential marriage a certain type of marriage, though statistically more likely, is not absolutely imposed. Opposite these two factors of randomness there is a third factor which makes up the kinship system, viz. what Lévi-Strauss calls "the rigid terminology" (cf. 1965: 18 and 20). It is not the case that patterns of behaviour reflect the nomenclature automatically, on the contrary,

"elles (sc. 'the behavioural patterns')
apparaissent souvent comme des élaborations
secondaires destinées à résoudre des contra-
dictions et à surmonter des insuffisances
inhérentes au système des appellations"
(1958: 46).

These rather vague suggestions about terminology as a complement and counterbalance to behaviour make it quite clear that kinship terminology is not among the real preoccupations of Lévi-Strauss, his main interest lying with kinship as a way of regulating marriages. Pierre Maranda has worked more directly on kinship terminology starting from Lévi-Strauss's concept of the element

of kinship: I would like to add his contribution here to show at least one way in which the ideas of Lévi-Strauss can be used in the study of kinship terminology (Maranda, 1964).

Maranda starts from the four elementary terms: a woman, her brother, her husband, and her child. Between these four terms three relations obtain, two simple ones, viz. that of sibling and that of alliance, and one complex one, viz. generation. To refer to these he uses the symbols S, A, and G. S and A are 'synchronic' and can be written on a horizontal axis; G is 'diachronic' and can be written on a vertical axis. G can take the values + and -, or G and \bar{G} .¹ Strictly speaking S and A should be written S and \bar{S} , but S and A are preferred because they are less abstract.

Kinship terms are essentially relational, they express the state of relatives. "Accordingly, a kinship term (w) is always the dependent variable of the independent variable x in $f_x(w)$ " (loc.cit. 519). In kinship analysis x will be the relation R which will have to be rewritten every time according to its specificity:

$$f_x \rightarrow f_R \longrightarrow \begin{cases} f_S(\text{ibling}) \\ f_A(\text{alliance}) \\ f_G(\text{eneration}) \end{cases}$$

If $x=0$, then w = unspecified man or woman.

It now becomes possible to draw up a basic set of kinship relationships which generates kinship terminologies (loc.cit. 520). This set will consist of relationships of the first

1. G and \bar{G} mean "generation and its converse" or "parent of and child of".

order, $f_x(w)$, of the second order, $f_{xy}(w)$, and of the third order, $f_{xyz}(w)$. One could give the following examples:

- first order, $f_x(w)$

x	σ^7	w	ϕ
A spouse	Hu		Wi

- second order, $f_{xy}(w)$

xy	σ^7	w	ϕ
AA spouse's spouse	WiHu		HuWi
+GA stepparent	MoHu		FaWi
+2G grandparent	FaFa		FaMo
	MoFa		MoMo

- third order, $f_{xyz}(w)$

xyz	σ^7	w	ϕ
+GSA avuncular by alliance	FaSiHu		FaBrWi
	MoSiHu		MoBrWi

The second order relationship AA does not occur in monogamous societies.

In the first order four relationships are possible. In the second order there are eleven possibilities, viz. AA, A+G, A-G, AS, +GA, +2G, +GS, -GA, -2G, SA, S-G. For the third order the number is 32, for the fourth 92, etc.

The four elementary components are not equally powerful in generating relationships. Maranda claims that +G is more often a dimension of a relationship than is -G, presumably because kinship is a previously established network into which one enters. A is said to be more powerful than S. This is the result, as far as I can see, of the fact that - in the second order and upwards - certain combinations containing S are excluded, viz. SS (=S),

S+G (= +G), and -GS (= -G). He claims in fact that A occurs twice as often as S, and +G twice as often as -G from a combinatorial point of view. Particular kinship systems could be examined from this angle and comparisons made as to their degree of divergence from the abstract model. No kinship terminology will express all the possible relationships. This type of analysis makes it possible to rank different terminologies according to the ratio of actual expressions to potential expressions (loc. cit.: 522 f.). It can also help to establish correlations between type of marriage, type of descent, authority, and other forms of behaviour, in as far as the axis of deviation from the model found in a given society brings out the major factors which give rise to certain patterns of behaviour and structure (loc.cit.: 526).

Maranda opposes the denotative meaning unveiled by componential analysis, to the connotative meaning of the system of attitudes (loc.cit.: 518), and he concludes that "the systemic relationship of connotation and denotation can be tackled more easily if approached denotatively first" (loc.cit. 526). Apart from Maranda's suggestion about the heuristic value of the axis of deviation from the terminological model, it would seem that much remains to be done to evaluate in a more precise manner this systemic relationship which Maranda mentioned in conclusion.

3.5.3 This is not the place to give an overall assessment of Lévi-Strauss's work: I shall limit myself to the way he has transplanted methods from linguistics to anthropology, the kind of linguistics he has been relying upon, and generally the prospects and chances of the kind

of all-embracing theory he has been advocating. This in itself is already so vast a matter that I shall only be able to offer a few comments rather than write a fully fledged critique.

G. Mounin has discussed the work of Lévi-Strauss from the point of view of a linguist (Mounin, 1970: 191-214) and, after some flattering opening remarks, has criticised him very severely. He reproaches him first of all for not being very clear about what exactly he is trying to do in referring to linguistics, secondly for having misunderstood most of the linguistic theories he has been using, and finally, where he did understand theories correctly, for having followed theories which were wrong, viz. certain theses of Jakobson Mounin disagrees with. Apart from this subjective preference for a certain type of linguistic theory many of the points of criticism are fairly predictable: the four principles Lévi-Strauss gives are not a good summary of Trubetzkoy's phonology; it is misleading to say that the function of language was known before its structure was perceived; phonemes as such (this in opposition to kin terms) do not have meaning.

There are, however, two things which must be noted here. Lévi-Strauss repeatedly expresses reservations about the use of this linguistic model, "le caractère précaire et hypothétique de cette reconstruction" (1958: 74), and he insists that verification of his hypothesis against the facts is essential. But it is true that he does too little to verify his theories; he is somehow more of a visionary than a man of science. And his masterful and suggestive prose, while making for compulsive reading, easily distracts the

mind from the substance of the argument. I suppose this contributed to the suspicion which led Mounin to consider the real Lévi-Strauss to be the man who misunderstood and misused linguistics, and to treat his reservations and warnings as little more than pro forma protestations of good will and restraint. I would be inclined to give Lévi-Strauss more credit than Mounin seems to do for concern with the factual truth, notwithstanding his seeming disregard for negative evidence: after all, the scope of his hypothesis is so wide that it will take a much longer time either to prove or disprove it. But the second point in Lévi-Strauss's defence is that the value of his main thesis does not depend on the quality of his linguistic knowledge as he does not reduce social life to language, nor derive the former from the latter as from its origin: he reduces social life to the symbolic processes of the human mind, and it is from this point of view that he considers social life to be similar to other systems of communication, all deriving from the same unique source. Linguistics simply happened to be the most advanced branch at one given moment in time; but when at a later stage Lévi-Strauss thought that another parallel would be more fruitful, he abandoned the linguistic model: in his study of mythology it is from music that he draws his model (cf. Simonis, 1968: 294).

While I am satisfied that this interpretation of Lévi-Strauss exonerates him up to a point of the charge of misuse of linguistics I am well aware of the fact that it may also diminish the value of Lévi-Strauss's work towards an understanding of how language and culture/society

interrelate. This brings me to the value of his transplantation method. Lévi-Strauss uses the term 'analogy': examining the notion of the analogy model may help us to see more clearly the advantages and dangers of this method of scientific inquiry. Moreover, if our assessment of Murdock's model being a scale model (cf. above 3.4.3) and Lévi-Strauss's an analogy model is correct there would be no question of a clash between the two approaches from the methodological point of view - they might possibly be complementary with the latter being the more powerful of the two. I shall follow again Max Black (1962) for the general notions of types of model with this proviso that I incorporate the re-arrangement proposed by Bertels and Nauta (1969: 133 f.) who introduce an extra general distinction between empirical and theoretical models¹, resulting in the following presentation.

Black's 'analogue model' (op.cit., 222 f.) now becomes the empirical analogue model, his 'theoretical model' (op.cit., 226 ff.) the theoretical analogue model or transplantation model. In both kinds of model a change of medium is involved while the structure is being preserved; one could think of the example of the simulation of life processes on the electrical circuits of a computer though this is an example of dynamic analogy or isomorphism of operations rather than the static analogy or isomorphism of structures Lévi-Strauss would seem to be concerned with. In the case of an empirical analogue model the aim simply

1. Empirical models consist of concrete entities, theoretical models of conceptual entities.

is to make it easier to manipulate something. In the case of a theoretical model there is a transplantation of a familiar, well-understood theory onto a new field of study. Significant similarity of structure justifies this procedure but such analogy cannot just be assumed, it must be demonstrated. There are degrees of application of this model: when we describe a thing as if it were in a certain way, we are on the level of fictitious interpretation, there is no explanatory power, just a heuristic value; when we describe a thing as being the other medium, we have an existential interpretation, we get a real explanation but at the same time expose ourselves to a considerable danger of self-deception. With scale models and empirical analogue models we create hypothetical constructs which will never show us how the things work in their total reality; the theoretical model is not built, the heart of the method consists in talking in a certain way: the model, in other words, is described. This leaves a greater freedom, there is no hindrance from accidental or irrelevant properties; but the controls which come from actual construction are equally lacking and unless independent tests are available, it is easy to go astray. Such a test must ultimately establish to what extent there is in fact isomorphism between model and field of application. If this works we shall discover new connections which a separate analysis of either the model or the field of application would not have born out.

Looking at Lévi-Strauss I am satisfied that there is a prima facie case for assuming the required

significant similarity in structure between parts of language and aspects of social organization to justify the use of the theoretical analogue model. At the same time I must say that to my mind the scale at which Lévi-Strauss has worked was too grandiose for proof or disproof ever to be possible: if more attention had been given to detail, to work on a smaller scale, it might have become possible to make specific and define more precisely Lévi-Strauss's basic intuition. In that case he might have gone beyond one form of phonology to discover perhaps a more useful model within the field of linguistics.

Talking of what might have been implies that there is hope for a general theory of communication. Chomsky has said that in kinship systems and folk taxonomies nothing so far has been discovered that is even roughly comparable to language (1968: 65). It is right to insist on the unique character and quality of human language and there is nothing to be gained by blurring real distinctions. But there is the tradition of which Saussure is probably the best known representative, a tradition which sees linguistics at the same time as autonomous and as integrated into a wider field of study, semiotics, the study of sign systems, of systems of communication (cf. Jakobson, 1973: 32 ff.). While maintaining the individual, separate character of the various branches of this study, the concept of semiotics would seem to provide the justification on the one hand for the continued search for a better understanding of the relation between the various communication systems, and on the other hand for the special position of language within semiotics and consequently within our

interdisciplinary study: a special position, because the pre-existence of language is always implied, verbal performances often accompany all forms of communication, and all these forms are or can be verbalized. But this is just a starting point: all remains to be done. We have this intuition that there is some special relation between systems of communication, and language would seem to provide the best entry into the problem; but what relation is there? It certainly is not a relation of identity, at the most one of similarity. But, as Lévi-Strauss suggested, this similarity could in fact just as well be a similarity of function based on a complementary opposition in the way of working. There certainly will be no easy, simple answer: the answer given will have to allow for flexibility and degree, yet provide a really new insight and throw light on aspects which otherwise might have remained in the dark. There is still a long way to go: and while Lévi-Strauss's fundamental intuition has not resulted yet in firm results, there is enough reason to continue this particular type of research. Gradually we may come a bit nearer to the truth, in whichever direction it may be found.

3.6 Leo Weisgerber and Neo-Humboldtianism.

3.6.0 Some of the crucial issues which were at the root of structuralism as presented by Lévi-Strauss were also very prominent in Germany though they have been dealt with there in a very different manner. They were the problem

of the unity of mankind with the diversity of culture in all its forms of expression, and the question of the subjectivity or objectivity of our knowledge of reality in all its forms and manifestations. Lévi-Strauss rejected empirical knowledge as too subjective and not leading to the universally human. The Kantian tradition is equally wary of empirical knowledge, as e.g. Cassirer shows:

"Truth ... is not to be attained so long as man confines himself within the narrow circle of his immediate experience, of observable facts. Instead of describing detached and isolated facts science strives to give us a comprehensive view. But this view cannot be attained by a mere extension, an enlargement and enrichment of our ordinary experience. It demands a new principle of order, a new form of intellectual interpretation. Language is the first attempt of man to articulate the world of his sense perceptions" (Cassirer, 1944: 208-209).

In this German tradition, then, the problem is seen as an epistemological question as far as philosophers are concerned though linked with the question of language, especially in connection with a priori knowledge, and as a question of linguistic relativity as far as linguists are concerned. The most important names on the philosophical side are I. Kant and E. Cassirer, and among the linguists W. von Humboldt and L. Weisgerber.

3.6.1 Very briefly the points which concern us in all this can be put as follows. Every word is a word about something, some part of reality, material, spiritual, or social. The question then arises, which is the primary element: language which creates our image of reality, or reality which is reflected (mirrored, copied) in language?

Those who hold the last view will accept the objectivity of reality; the first view is held by idealists like the Kantians: instead of an objective reality they speak of the creative mind which creates a 'Weltanschauung'. Though inevitably oversimplified this option between two mutually exclusive alternatives would seem to be a basically correct reproduction of the argument. The fact that there may well be a third option (language being a subjective copy of an objective reality) is worth remembering for future reference but need not concern us here where we are simply trying to sketch the facts about the background of Neo-Humboldtian linguistics.

The clearest philosophical statement in the Neo-Kantian tradition has been made by Ernst Cassirer in his various publications, one of which actually has the title "Le Langage et la Construction du Monde des Objets" (1933) where he said among other things:

"La représentation 'objective' ... n'est pas le point de départ du processus de formation du langage, mais le but auquel ce processus conduit" (loc.cit. 44).

Cassirer turns Kant's critique of pure reason into a critique of culture (1953: 80) and here, as there, objectivity is guaranteed by the human spirit:

"Thus, with all their inner diversity, the various products of culture - language, scientific knowledge, myth, art, religion - become parts of a single great problem-complex: they become multiple efforts, all directed toward the one goal of transforming the passive world of mere impressions, in which the spirit seems at first imprisoned, into a world that is pure expression of the human spirit" (ibid. 80-81).

Reality, as Cassirer has pointed out elsewhere (1944: 23), is not a unique and homogeneous thing for different organisms:

their experiences, and consequently their realities are incommensurable with one another. It is a matter for each living organism to come to terms with the surrounding world, to make the world its world; and man does so by using symbolic systems (cf. 1944: 23-25):

"In language, in religion, in art, in science, man can do no more than to build up his own universe - a symbolic universe that enables him to understand and interpret, to articulate and organize, to synthesize and universalize his human experience" (ibid. 221).

A. Schaff, discussing Cassirer's theory, explains his concept of symbolic forms (cf. Schaff, 1973: 33 f.): symbolic forms are a special mental energy and their function consists in creating our image of the world. There are various symbolic forms, various forms of spiritual energy, that produce the various images of the world. These forms include language, myth, art, and scientific cognition, and they differ from one another. Language is, for Cassirer, the fundamental symbolic form since it is used both by myth and by science, and it is interpreted as a kind of spiritual energy which creates the image of the world a priori, i.e. loose from empirical observation.

It is this view - though in a less extreme form - which we find in the linguistic work of W. von Humboldt, and later in that of L. Weisgerber and other Neo-Humboldtians.

3.6.2 The work of W. von Humboldt is very extensive, very rich in ideas and far from simple. There can be no question of presenting his linguistic theory here in a few words. On the subject of linguistic relativity and related matters several studies have been published over the last

years (see e.g. R.L. Brown, 1967; R.L. Miller, 1968; Julia M. Penn, 1972) which have thrown light on a difficult matter from different angles. All I want to do here is to present some of the major themes of his great posthumous work Ueber die Verschiedenheit des menschlichen Sprachbaues (1836) in the light of what we have seen about the general background of this school of thought.

Humboldt accepted that language had originated suddenly: one moment it was not there, the next moment it was. Obviously the question which then arises is how to explain the differences in language structure, their "Verschiedenheit", and to determine what is proper to each one. The answer to this question is to be found in the "Geist", the creative spirit which is at work in and through the language.

"Ihre wahre Definition (i.e. of language) kann daher nur eine genetische sein. Sie ist nämlich die sich ewig wiederholende Arbeit des Geistes, den articulirten Laut zum Ausdruck des Gedanken fähig zu machen" (op.cit. 41).

These differences extend well beyond just the language, they affect whole nations in their total being: but this own character of nations is expressed most clearly in the language.

"Die Geisteseigenthümlichkeit und die Sprachgestaltung eines Volkes stehen in solcher Innigkeit der Verschmelzung in einander, dass, wenn die eine gegeben wäre, die andere müsste vollständig aus ihr abgeleitet werden können. Denn die Intellectualität und die Sprache gestatten und befördern nur einander gegenseitig zusagende Formen. Die Sprache ist gleichsam die äusserliche Erscheinung des Geistes der Völker; ihre Sprache ist ihr Geist und ihr

Geist ihre Sprache; man kan sich beide
nie identisch genug denken" (op.cit. 37).

Because the presence of the spirit can only be thought of in activity and as an activity, language too must be seen as an activity, not as something static: it is in this context that Humboldt says that language is not an Ergon but an Energeia, "kein Werk, sondern eine Thätigkeit" (op.cit. 41).

This dynamic quality is present throughout his theory of language. In common with many 19th century scholars he saw language as an organism, an organic whole, after the example of biology; and it was the example of comparative biology which inspired his comparison of languages. His dynamic view prevails here too: language is not a kind of dead weight in the dark of our minds while having the inner coherence of an organism. It does have that inner coherence, and we must study the parts which make up the whole, i.e. the static side of language, but there is more:

"Da sie, in unmittelbarem Zusammenhange mit der Geisteskraft, ein vollständig durchgeführter Organismus ist, so lassen sich in ihr nicht bloss Theile unterscheiden, sondern auch Gesetze des Verfahrens, oder ... veilmehr Richtungen und Bestrebungen desselben. Man kan diese, wenn man den Organismus der Körper dagegen halten will, mit den physiologischen Gesetzen vergleichen, deren wissenschaftliche Betrachtung sich auch wesentlich von der zergliedernden Beschreibung der einzelnen Theile unterscheidet" (op.cit. 105).

So language is like a law which regulates the functioning of our power of reflection. This link between language and thought is crucial to Humboldt's thinking:

"Die Sprache ist das bildende Organ des Gedanken. Die intellektuelle Thätigkeit, durchaus geistig, durchaus innerlich, und gewissermassen spurlos vorübergehend, wird durch den Laut in der Rede äusserlich und wahrnehmbar für die Sinne. Sie und die Sprache sind daher Eins und unzertrennlich von einander" (op.cit. 50);

and this congruence between language and thought, claimed here for sound-complexes, is also assumed for grammatical categories (ibid. 180). This inner and purely intellectual aspect is the essential part of language, the so-called inner form of language (op.cit. 91). Language is a synthesis of the outer and inner form of language, of the "Lautform" and the "innere Sprachgesetz" (op.cit. 101 ff.), or put in another way: language is a manifestation of the 'spirit' after the example of its inner form. It is through language that man can turn subjective perception into objectivity (op.cit. 52): one can think of language as "eine zweite, von dem Menschen nach den Eindrücken, die er von der wahren empfängt, aus sich selbst heraus objectivirte Welt" (ibid. 74). Language is thus not just a means of communication allowing mutual understanding, it is a real world, "eine wahre Welt ..., welche der Geist zwischen sich und die Gegenstände durch die innere Arbeit seiner Kraft setzen muss" (op.cit. 205). Through language man comes to terms with the world at large, and each language does so in its own way, creates its own particular way of looking at the world: "so liegt in jeder Sprache eine eigenthümliche Weltansicht" (op.cit. 58). This 'Weltansicht' is the basis of Humboldt's linguistic relativity, and it is a recurrent theme among

German linguists, as illustrated, for example, by the following quotation from Georg van der Gabelentz (1901: 76):

"Jede Sprache verkörpert eine Weltanschauung, die Weltanschauung einer Nation. Sie stellt eine Welt dar, das heisst zunächst die Gesamtheit der Vorstellungen, in denen und über die sich das Denken eines Volkes bewegt; und sie ist der unmittelbarste und bündigste Ausdruck für die Art, wie diese Welt angeschaut, für die Formen, die Ordnung und die Beziehungen, in denen die Gesamtheit ihrer Objekte gedacht wird. Wer sie so versteht, - und nur der versteht sie wissenschaftlich, - zu dem redet durch sie das Volk: Dies ist mein Standpunkt, dies also mein geistiger Gesichtskreis und die Perspektive, in der sich für mich die Dinge gruppieren, - und dies ist die Eigenart meines geistigen Auges, womit ich die Welt betrachte, und das sich an und in dieser Welt geschult hat".

3.6.3 The man who has most systematically elaborated Humboldt's ideas and, in doing so, has regularly used kinship terminology to illustrate his point, is Leo Weisgerber. He recognizes that many people have tried to deal with the phenomenon of language, yet no one seems to have come up with the right approach, providing all the answers. As he sees it, it is the starting point which matters, from what point of view one looks at language; and his answer is formulated in terms of what he calls "Das Menschheitsgesetz der Sprache" (Weisgerber, 1964).

Language is a law, something imposed; from the point of view of the individual there is "das Gesetz der Muttersprache", and the study of this aspect is "Sprachpsychologie"; from the point of view of the community we find "das Gesetz der Sprachgemeinschaft", covered in the "Sprachsoziologie"; from the point of view of mankind

as seen in its basic existence we discover "das Gesetz des sprachbedingten Dasein" in a "Sprachphilosophie". Together these three laws make up the "Menschheitsgesetz der Sprache". For our purposes it will be enough to look at the "Sprachsoziologie": it is here that we shall discover the contribution made by the Neo-Humboldtians to our field of interest.

The main ideas behind Weisgerber's theory are taken from Humboldt though Weisgerber has added new elements to complete and systematize his thoughts in a synthesis of his own. The most basic point is the fact that language is not an 'Ergon' but an 'Energieia', not a 'Werk' but a 'Wirkende Kraft', not static material but an active, dynamic force (cf. Weisgerber, 1962a:9 f.). This implies two things. First of all it means that language has the "Daseinsform einer 'Wirklichkeit' " (1962a:11 f.), in other words, is a reality, something real. While in the past a two-way distinction was made between "real things" and "mere abstractions", Weisgerber claims that sociology has revealed to us a third possibility, the reality of the "soziale Objektgebilde", the reality of which lies not in observable appearances but in what they achieve, more specifically in what they do to make possible and sustain forms of human social life. Playing on German synonyms Weisgerber says that what we have here is not "Realität" but "Wirk-lichkeit". And this leads to the second implication, the "Leistung einer mitgestaltenden Kraft bei allem geistig bestimmten Tun der Mitglieder (einer) Sprachgemeinschaft" (ibid.): language is present as a creative force in every spiritual activity of a

speaking community, and this force can take three forms, "geistschaffende, kulturtragende, und geschichtsmächtige Kraft" (1964: 33).

At present we are interested in the first form only, the principal one:

Die primäre Leistung jeder Muttersprache besteht darin, dass sie ihrer Sprachgemeinschaft den Weg eröffnet, die Lebenswelt in das Eigentum ihres Geistes umzuschaffen" (1962a:12),

or to adapt a sentence from Basilius: to reconstitute human experience ideally and to make this idealization overt (cf. Basilius, 1952: 98). This refers to Humboldt's "sprachliche Zwischenwelt" or "intermediate world" (cf. Bynon, 1966: 470). This force is at work on the level of the lexicon as well as on that of grammar, it concerns the meaning of words as well as the meaning or function of grammatical categories and constructions.

Restricting ourselves to the realm of words we find that Weisgerber subscribes to the now generally accepted opinion on the conventional nature of language: there is no direct link between sound-complexes and things. Words are arranged in a special way which produces as a result the "gedankliche Zwischenwelt". It is here that Weisgerber gives the example of kinship terminology (1964: 35 ff.). There is, he says, a general scheme of possible kin relationships between men, and this scheme is objectively the same for all; this scheme is built up with five relationships: "Vater, Mutter, Gatte (Gattin), Sohn, Tochter". When we look at a particular language we see how the manifold possible kin relationships are regrouped

in a "gedankliche Ordnung", an order created by the human mind.

Looking in this way at just one language certainly gives us some idea of how the world is being 'reconstituted ideally' in it, but the full impact of this notion does not become clear unless and until the specific character of each language is brought out by comparison: historical comparison with earlier stages of the same language, or a-temporal comparison with the languages of other "Sprachgemeinschaften":

"Im Vergleich wird das sprachliche Weltbild offenbar, die Bindung der 'geistigen Zwischenwelt' jeweils an eine Muttersprache einsichtig, und die Möglichkeit, Richtung und Ergebnis der in jeder Sprache wirkenden weltgestaltenden Kraft zu erkennen und zu beurteilen, greifbar" (1964: 97).

Weisgerber shows how contemporary German kin terms compare with the Middle High German system and, within the confines of Indo-European languages and cultures, with the classical Latin system. Using Kirchhoff's typological classification of kin terminological systems (Kirchhoff, 1932) he then illustrates the diversification in "Weltgestaltung" on a world-wide scale (cf. 1964: 45-50).

This world-view is unconscious and Weisgerber has developed his own style of analysis to examine this phenomenon, an analysis in four stages which he has explained at length in his book Die vier Stufen in der Erforschung der Sprachen (1963), taken up again in his Menschheitsgesetz (1964). The four stages are (in the translation of the terms as given by T. Bynon, 1966): formal analysis, content orientated analysis, creative potential orientated analysis,

and effect orientated analysis. The first two are static in nature, they are a "notwendiger Umweg" (1964: 75): "diese statische Arbeit schafft die Unterlagen, die nötig sind, um zu der energetischen Betrachtungsweise überzugehen" (1962: 183). Our interest begins with the second stage which "deals with the structure of content in its own right" (Bynon, loc.cit. 472). To establish what the content of a word is we cannot go to reality for there is no direct link between word and reality; nor can it be done through parallelism between sound pattern and content given such things as homonymy and metaphorical extension.

"Der Inhalt eines solchen Wortes muss in erster Linie aus der intern sprachlichen Bestimmtheit abgeleitet werden, auf der seine Geltung beruht" (1964: 64).

This is achieved through the concept of linguistic field, generally attributed to J. Trier and with slight modifications used by many German scholars (cf. S. Öhman, 1953): in different ways the notion has gained wide usage (cf. A. Lehrer, 1974). The set of kinship terms is such a field: the definition of the content of items of a field has to be seen in the light of the interdependence of item and total field.

In the third stage this field must be re-interpreted "energetically"; instead of "Sprachinhalte" this stage deals with "Sprachzugriffe", i.e., "Wirkungsformen der Sprachkraft" (1962b:176) or the process of "wording the world" (das Prozess des Wortens der Welt, cf. Bynon, loc. cit. 475). In this stage the linguist examines in what direction this process develops, what forms it takes, and

also in relation to which reality it operates, i.e. what reality is being 'worded'.

In the final stage 'life' is brought back into the centre of our attention for language is not an aim in itself. In this effect orientated analysis one studies the influence of language on society and culture. Words have a 'value' and we tend to experience these values of our own language as natural, as self-evident. Here Weisgerber refers once again to his example of kinship terms, especially seen comparatively, to prove that these values are not natural or self-evident (1964: 89). "Language", as Bynon concludes, "is, in fact, no mere mirror of culture but one of its most active and effective participants" (loc.cit. 477).

3.6.4 It is inevitable that these few remarks do not do justice to the full complexity of Humboldt's thinking or Neo-Humboldtian systematization, but I believe that the essential points have been indicated. If I now give some reflexions and observations one should keep in mind both the limitations and the purpose of this survey.

Weisgerber does not share Lévi-Strauss's reservations about the nuclear family as the objective frame of reference, he does not even mention it as a problem. Yet in anthropology the debate on the interpretation of kinship as a system of genealogy or of marriage regulations continues. I am convinced that, for scholars interested in the relation between language and culture, this is an issue one cannot avoid: the relevance of the linguistic field theory for this topic depends on it.

For some people there has been a problem of cause and effect in the matter of the interdependence of language and culture/society. Weisgerber works with a "totality concept" which might seem to transcend that question by integrating all those elements simultaneously. Yet, as may be concluded from the philosophical presuppositions to which he subscribes implicitly and explicitly, there is a priority of the "Geist" and this "Geist" is identical with language. The combination of, on the one hand, this priority given to the human mind and, on the other hand, the acceptance of an objective frame of reference in the outside reality, viz. the nuclear family, seems slightly inconsistent; it also seems to cover up a basic weakness of idealistic philosophy. And though our prime interest is not in philosophy, it may make us wary of the systematic side of Neo-Humboldtian theory however much one might feel attracted by some of its ideas and intuitions.

The very special terminology of this school of thought makes it difficult to assess it properly: it is easy to do it injustice by judging it in terms of a different theory, but it is only too easy as well to get caught in the net of a system which - on its own terms - shows a strong internal coherence. All the same one keeps wondering what real value is added to the static analysis by the "energetic" stages: these are supposed to be more fundamental, yet in some ways they would seem to be much more ephemeral or marginal, more part of language use, of performance, of pragmatics. I find it difficult to see how this would give us the final and deepest insight

into language even if one is ready to admit that it is part of it: I could see that it might tell us more about man and the human mind in a philosophical way within the framework of a given philosophy, but that is not really what we are after. Accepting that the Ergon/Energeia distinction seems useful at first sight with regard to the matter of linguistic relativity one has to say that it still lacks clarity and needs to be examined further.

I have spoken on occasion of the importance of exceptions (cf. 3.4.3) in connection with explanatory theories for types of kinship terminologies. The notion of linguistic field, invaluable in itself, has suffered initially from a lack of attention to what might also be called 'exceptions': it is generally admitted now that we have to expect such things as gaps or overlaps in a field, that fields are not always the closed, well-defined sets they were thought to be, and that associations linking words can sometimes be rather loose and unpredictable. And even then we are only dealing with the paradigmatic relations between words: the syntagmatic aspect is hardly touched upon. It is unfortunate that Weisgerber did not pay more attention to the implications of the weaknesses of field theory: it might have formed a good counterbalance against a tendency towards absolutist idealism.

Humboldt is considered to be one of the most original and inspiring thinkers about language, and I would subscribe to that. But I am equally convinced that the value of his intuitions about language is in no way a confirmation of the Kantian idealistic philosophy which influenced him and which,

in part, determined his way of formulating his ideas. Weisgerber chose to be both a Neo-Humboldtian and a Neo-Kantian: I would settle for being Humboldtian in inspiration.

4. Recent Developments: Theoretical Issues and Applications to English Kinship Terminology.

4.0 In the previous chapters we have followed the development of ideas by singling out some of the main contributions to the study of kinship terminology. In each case we have tried to specify the intellectual assumptions from which the respective authors operated; and we have concentrated, even though passing through an inevitable amount of anthropology, on what they had to say about kinship terminology and, to some extent, on their views on language. As quite a few of those scholars were primarily anthropologists, they tended to be more concerned with the meaning of kinship in the sense of kinship's relationship to the whole of society: in such a perspective the linguistic side is bound to get less attention and must, in some cases, be reconstructed indirectly.

It would have been possible to continue along the same lines, i.e. by selecting a few representative authors and evaluating their work. The names of such scholars as F. Lounsbury and W. Goodenough come to mind. But for a number of reasons we will not follow this procedure any longer. First of all - now that the main outlines of the problems have been sketched - it might become too much of a repetition, especially since quite a few of the practitioners of kinship terminology analysis share similar basic assumptions. Furthermore, there would seem to be a change in style: kinship terminology is often no longer a fairly small aspect or part of a much wider, more global study of a given society. It now tends to be studied

for its own sake, as a system of classification, or as part of the lexical component of the grammar of a language. This greater "autonomy" of the study of kinship terminology has somewhat diminished the need for a wider background knowledge which was necessary as long as opinions on kinship terminology were contained in the wider framework of a global theory of kinship and its meaning in society. Finally it can be said that scientific discussion takes place in a different way: periodicals are at least as important as large size books, and articles often tend to be complementary.

The study of the subject, though slightly changed in character, is still as intensive and as widespread as ever. Given the increase in the number of field-workers and researchers it has now reached the point where the sheer amount of published material makes it impossible to present anything like a complete picture. Our main interest, however, lies with methodology: in this respect we are very fortunate in that all recent forms of analysis have been applied to (American) English kinship terminology. By concentrating in the present chapter on these studies we shall get a complete picture of recent developments with the added bonus of the extra clarity of insight gained by dealing with familiar material.

4.1 Preliminary Remarks.

4.1.1 No subdivision of so rich a field of study as kinship terminology can do justice to all the factors involved. While a classified grouping according to major

themes may provide a more coherent view of the matter, details of individual contributions will sometimes stand out less clearly. I have nevertheless opted for a more thematic treatment. Without claiming perfection for the order here adopted it would seem to be an adequate and reasonable way of grouping the various contributions.

Lévi-Strauss in his lecture on the future of kinship studies (1965: 13 f.) made a two-way division in that field. Some scholars are interested in the meaning and purpose of kinship systems, others in the internal coherence of kinship nomenclatures. The latter group consists of those engaged in the various forms of formal analysis, the former of those who try to explain the meaning of kinship systems by causal factors. It is here that Lévi-Strauss's own work has brought about the clear and explicit separation between the supporters of an efficient cause theory and his own final cause explanation.

Frank Wordick (1973) has proposed a slightly different distinction between types of analysis. He distinguishes a cultural type of account, a causal one, a cognitive one using componential analysis, and a generative one. The causal one is the one which has been dominant in one form or another from Morgan to Murdock. The cultural type is found in Schneider's work. Componential analysis was developed and applied to kinship terminology by both Goodenough and Lounsbury in the mid-fifties, while the generative approach was originally developed by Lounsbury and first presented in his "The Structural Analysis of Kinship Semantics" and "A Formal Account of the Crow- and Omaha-Type Kinship Terminologies" (1964 a and b). The

causal type needs, after the preceding chapters, no further explanation; the cognitive and generative types are the kind of analysis which concentrate on what Lévi-Strauss called "the internal coherence of kinship nomenclatures". Wordick associates componential analysis and cognition: this connexion has in fact existed from the beginning when, for instance, Goodenough wrote about paradigmatic structures: "their analysis can in turn tell us much about human cognitive processes" (1956: 97). 'Cognitive processes' refer to the ways in which the minds of men who use a given language work, the ideas and concepts they have, and the way in which they classify these. I shall conform to this historical association and deal with the problems of cognition and kinship terminology immediately after the discussion of componential analysis. After that I will discuss what Wordick calls the "generative type" of analysis: I will take this to include all types of formal analysis by means of sets of rules. In that section I shall deal both with anthropologically inspired analyses and with work done in connection with Transformational Generative Grammar.

There remains the question of what Wordick calls the "cultural analysis" as practised by D. Schneider. Lévi-Strauss classified Schneider with the advocates of efficient cause explanation (Lévi-Strauss, 1965: 14), understandably so since Schneider, in collaboration with G. Homans, had attacked Lévi-Strauss's final cause approach (Homans and Schneider, 1955). I am not sure what made Wordick classify him separately, but I will follow his lead for the following reasons. One reason - though from my point

of view not a very important one - is that Schneider claims his position to be different from the one taken by all other anthropologists (cf. for example Schneider, 1972). From my point of view the really decisive reasons for including Schneider's work here rather than in the preceding chapter are the following. Firstly, Schneider has made a substantial contribution to the study of American English kinship terminology. Secondly, the discussion of some of the crucial issues dealt with in this chapter - polysemy, primary and secondary meaning, denotation and connotation - would not be complete and perhaps not even fully intelligible without taking into account what Schneider has to say on the subject.

It is with the cultural analysis that we shall begin this survey of analyses of English kinship terminology. But before doing so I would first like to mention briefly some publications of general interest and importance for the study of kin terms.

4.1.2 In 1968 I.R. Buchler and H.A. Selby brought out their book Kinship and Social Organization. An Introduction to Theory and Method, a book not unlike my present work in some ways but with a quite different orientation all the same. First of all their book is meant to be a handbook in anthropology. This is particularly clear from their survey of the study of kinship and social organization: all purely linguistic contributions are absent and so are the exclusively linguistic aspects of the work of anthropologists. The greater part of the book is about recent developments. The authors take the view that one should aim at a study of social organization done "more mathematico". However, the

subject is too vast to be tackled in its entirety from the start; instead, they say, one should start from the most formalized part and advance from there. Since World War II this means beginning from the various types of formal analysis of kinship terminology in the hope of achieving eventually a high degree of formalization in the study of social organization at large. It is in this context that they discuss theoretical background and techniques of analysis by reviewing the work done on American English kinship terminology. Their book is not recent enough to take fully into account the implications of the development of extensionism though they discuss extensively the descent and alliance theories from the anthropological point of view. Their account seems to be extremely well-balanced and, while they recognize that descent theory based on Rivers' genealogical method definitely has the edge over the alliance theory, they do point out quite rightly that most of the available evidence was originally collected with a genealogical orientation in mind. The genealogical method stresses the opposition of the lineal vs. the collateral relatives, thus singling out the nuclear family: this leads almost naturally to the notion of extension.

The authors are equally prudent in their assessment of the issue of a correlation between terminology and social organization: they prefer to use the word "hope" in this context rather than making too firm statements which their good sense tells them to be insufficiently supported or demonstrated.

Very valuable would seem to be their attention to the element of choice in all forms of behaviour, to what is not totally predictable. To deal, for example, with the sections system¹ which determines who may marry whom, they use methods taken from information theory to show how the uncertainty, contained in that question, is gradually reduced. Elsewhere they bring in a useful distinction from Games Theory between ground rules and strategy rules: the former account for what is universal and completely predictable, the latter for what is only partially predictable. And they point out that eventually the relationships between these two sets of rules will prove to be a crucial issue. One may be slightly sceptical as to whether a treatment "more mathematico" is really going to work in these delicate areas of social interplay: but there is no reason why the authors should not pursue such a hypothesis: and I cannot find fault with the prudent way in which they have approached the subject.

Another book of a fairly general nature is Lawrence E. Nogle, Method and Theory in the Semantics and Cognition of Kinship Terminology. Though the book is dated 1974 it does not contain references to literature published after 1968. It is limited moreover to the study of American English kinship terminology. The book only deals with componential analysis and with Bock's generative analysis (Bock, 1968). From this point of view alone there would be sufficient reason to try and bring this survey up to date.

1. A section system is a way of dividing a tribe up into groups; it results from marriage arrangements.

There is even more reason to do so as the author has concentrated on a very specific approach and on one particular problem mainly. The approach is what is known as ethnoscience, or ethnographic semantics, or New Ethnography. According to W. Sturtevant who has discussed this direction in anthropological studies, ethnoscience "refers to the system of knowledge and cognition typical of a given culture" (Sturtevant, 1964: 99). Research in this field has concentrated on classification as reflected by native terminology, in order to discern how people construe their world of experience from the way they talk about it. The principal concern of this method was to overcome the tendency to superimpose one's own analytical categories onto another culture. This concern explains the emphasis which Nogle lays on the problem of psychological reality and on the place of folk definitions. The author does not add anything to the description by previous analysts: in effect he adopts Goodenough's analysis (see below 4.3.2.): but he has introduced new tests to investigate the link between semantic analysis and other facts. His conclusion contains more questions than answers, some of which will occupy us later, e.g. variation of usage in kin terminology, secondary meanings, the genealogical vs. sociological basis of classification.

A book which does not deal with English kinship terminology but must be mentioned, is A Study in Structural Semantics. The Siriono Kinship System by Harold W. Scheffler and Floyd G. Lounsbury (1971). This is an extremely important book on the subject of kinship terminology in general where the case for the extensionist theory is put

very extensively and very ably. I shall return to it when I discuss the extensionist theory (cf. below 4.3.3.).

4.2 The Cultural Approach.

The cultural approach is very much a social theory rather than a linguistic theory: but the attention given in it to kinship terminology obliges us to look at it carefully. The method owes much for its theoretical background to the work of Talcott Parsons who sees society as a system of action. This general system of action has four constituents: a social system, a cultural system, a personality system, and a behavioural organism (cf. e.g. Parsons, 1971). The distinction between social and cultural systems is important, as we shall see, for an appreciation of Schneider's position.

Parsons' own work is sociological but in his article on "The Kinship System of the Contemporary United States" (1943) he has made a few remarks about the terminological aspects of Kinship which are worth quoting:

"It can perhaps be regarded as established that, with proper precautions, analysis of kinship terminology can serve as a highly useful approach to the study of the functioning social structure" (loc.cit. 22).

He goes on to point out that extra precautions are called for in the case of English. There is no significant terminological difference between British and American English in the domain of kinship so that it is difficult to bring out what is distinctively American (or British) by this means. In fact there are only a few minor differences between modern European languages in this respect: all analysis

of terminology can do is indicate a very broad type.

This leads Parsons to the question: "How far can this distinctive terminology be said to 'reflect' the actual institutional structure of kinship?" (loc.cit. 27).

Parsons says it does so in a broad way, but all his illustrations are negative in character, e.g.: we have no extended kin groupings, no rules for exogamy, no preferential marriage rules. If, however, one wants to get a clearer idea about the specific structure of kinship in our Western societies one has to turn to a different order of evidence, in Parsons' view - which he does by giving a sociological account of American social structure in the remainder of his article.

I think Parsons' remarks are a very important reminder of how difficult the problem is of the social relevance of vocabulary. The size of the problem can easily be obscured by what may look like perfect examples of isomorphism between the structure of society and that of kin terminology in so-called primitive societies: and even there the matter is not straightforward at all, as I hope to show later. But the main challenge of Parsons' remarks would seem to be whether a meaningful linguistic account which is at the same time socially relevant can be given of kinship terminology, regardless of the type of society one is dealing with.

D. Schneider, in the article he wrote together with G. Homans (Schneider and Homans, 1955) and in his later work on kinship, owes much to Parsons' theoretical work. It all revolves around American kinship, the American Kinship System, in a society where other institutions such

as the economy and technology have become dominant to the point of "pushing kinship to the wall". This is his main interest; he has studied American kinship terminology for the light it may throw on the problem of the American kinship system.

In order to understand Schneider's ideas correctly I think it is worthwhile to see what exactly he is trying to explain. He has made this clear on several occasions, e.g. very explicitly in his 1972 article "What is Kinship all about?". Parsons distinguished, as I mentioned before, between social and cultural systems. The primary function of the social system is integration and the components of this system are norms, rules. The primary function of the cultural system is pattern maintenance, to provide a meaningful social order: the components of this system are values. Schneider claims that all scholars whose names have been associated with the study of kinship have been interested in the facts of human reproduction and how man integrates this into society: this is the social system or social organization point of view. He himself concentrates on the cultural system, the system of symbols and meanings embedded in the normative system. On the cultural level one asks: What does this world consist of?, whereas on the normative level the question would rather be: Given the world to be made up the way it is, how does a man proceed to act in it? It is on the level of symbols and meanings that Schneider situates his study of kinship and of kinship terminology.

"Our problem is not to see what correlation the cultural symbols called kinship terms have to the functional roles. Rather it is to see how the whole cultural-symbolic

system is differentiated and how one set of symbols - kinship terms - relates with other sets of symbols - the norms for relations among kinsmen" (1965: 294).

Culture, as Schneider sees it realised in American society, is the outcome of the action of human reason on nature. What is good in nature is selected, discovered, chosen; and rules and regulations (the order of law) are established to maintain and perpetuate what is good.

"The classification of relatives in American kinship is built on the same set of premises set in the same relationship to each other. The relative in nature is at one extreme, the relative in law is at the other extreme. The first is but a relationship of nature, fundamental as that is. The second is but a set of artificial rules or regulations for conduct, without substantive or natural base. But the blood relative, related in nature and by law, brings together the best of nature modified by human reason; he is thus the relative in the truest and most highly valued sense" (1968: 110).

This basic distinction between 'substance' and 'code-for-conduct' can be found in every aspect of the kinship system in one way or another. Its most important example is the fundamental distinction between the relative as a person and the person as a relative.

The person as a relative is so characterized by a set of distinctive features which are defined and differentiated by a single symbol,

"the central symbol of sexual intercourse/love. It defines what a relative is in the abstract. It states what the relationship between relatives is by definition. It consists of a set of conceptual elements and their interrelationships" (1968: 59).

These distinctive features are the same as those which define

the family. Love has a double, opposite function: it unites opposites, and it separates what is united. It unites the opposite sexes, male and female, in the husband/wife relationship; this unity gives birth to a separate entity, the child, son and daughter. In this way husband and wife become parents, father and mother. This unity of the family is bound to be undone, for the brother/sister relationship falls under the universal incest prohibition so that the children will leave their family to found one of their own (cf. 1968: 39-40). The family is a paradigm for how kinship relations are to be conducted and to what end: those relations are specified as relations of love, or - as Schneider has it in his favourite formula - of enduring diffuse solidarity (ibid. 50).

"Solidarity, because the relationship is supportive, helpful, and cooperative; it rests on trust and the other can be trusted. Diffuse because it is not narrowly confined to a specific goal or a specific kind of behavior. Two athletes may cooperate and support each other for the duration of the game and for the purpose of winning the game, but be indifferent to each other otherwise. Two members of the family cannot be indifferent to one another, and since their cooperation does not have a specific goal or a specific limited time in mind, it is enduring" (1968: 52).

"Quite apart from the distinctive features that define the family and its members, each member is also a person and as a person is constructed out of not one, but many different elements, each drawn from many different sources" (1968: 43).

The relative as a person is the personification in actable terms of a variety of different symbol systems, including the symbol system of sexual intercourse/love (cf. 1968: 111).

"The person has either male or female sex as defined by the sex-role system. The person has age attributes as defined by the age-role system. The person has class characteristics as defined by the class system. The person may have occupational, religious, political, or a variety of other attributes, each defined by reference to its own self-contained set of symbols from its domain" (1968: 59-60).

The relative as a person is made up of distinctive features other than just those of kinship whereas the person as a relative is defined exclusively by the distinctive features of kinship. Schneider has said repeatedly that one of the major problems of the description and analysis of the American system is that of variance, the variety of alternate terms (cf. e.g. Schneider and Homans, 1955: 1195; Schneider, 1968: 16 f.). The difference between the person as a relative and the relative as a person is precisely that in the former case there is no variance in the use of kin terms whereas in the latter there is. In fact, we really have two systems here, one restricted to a set of distinctive features, the other encompassing a variety of symbol systems.

The concept of 'person' can be both concrete and abstract.

"On the one hand, the relative as a person is a concrete construct in that it refers to the person as a living human being, a real individual. On the other hand, the relative as a person is a normative construct, a construct consisting of normative guides and standards in terms of which behavior should proceed" (1968: 76).

The matter of kinship terms at the level of the relative as a person can best be approached by means of the classic question 'Who calls Whom What?'. Looking at it in this way from the angle of the person both as a

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concrete and as a normative construct, one is allegedly able to give a satisfactory account of the variance in American kinship.

We have to remember that there are two distinct elements in American kinship, substance and code-for-conduct: as these elements are distinct, each can occur alone or they can occur in combination. On the level of the concrete person the question 'Who calls Whom What?' comes down to a decision as to whom to count and whom not to count as a relative. This decision can be based either on the element of substance, or the element of code-for-conduct, or on both. Another element which comes into the decision-making is the notion of distance. The two elements, substance and code, are not of equal value: substance is higher in value than code, while the combination of both has the highest value. This means that a person at some distance will more easily be counted as a relative if there is only the substantive element present than if there is only the code-for-conduct element present. The notion of 'distance' used here is simply

"the statement of kinship in quantitative terms. That is, on the one hand it is a measure of the degree to which two persons share common biogenetic substance, and on the other hand it is a statement of the magnitude of the claim on diffuse, enduring solidarity" (1968: 65).

The reason why this notion comes in in answering the question whether or not a relationship obtains, is a characteristic of the American kinship system which Schneider has indicated with the two phrases "fuzziness of boundary" and "fade-out principle" (1965: 289 f.). 'Fuzziness of boundary' points to the fact that there is no clear, categorical limit to the

domain of kinsmen, while 'fade-out principle' defines the real limit beyond which people are too distant to be counted as relatives. In American kinship distance means three things: physical distance, where people live, how often one sees them if ever; socio-emotional distance, a feeling of identity or difference, emotionally or socially; and genealogical distance, going back to a common ancestor (1968: 72 f.). So much for the delimitation of the domain of kinship by personal decision from a 'cultural' rather than a 'social organisational' point of view.

Now from the point of view of the person as "a construct consisting of normative guides and standards in terms of which behavior should proceed" (1968: 76), 'Who is called What and by Whom?'. It is here that both the earlier article by Schneider and Homans (1955: 1195) and Schneider's book (1968: 83 ff.) point to the fundamental characteristic of the wide variety of alternative terms in the American kinship system: for there are far more kinship terms and terms for kinsmen than there are kinds of kinsmen, or categories of kinsmen.

"Mother may be called 'mother', 'mom', 'ma', 'mummy', 'mama', by her first name, nickname, diminutive, 'old woman', and a variety of other less commonly used designations. Father may be called 'father', 'pop', 'pa', 'dad', 'daddy', by his first name, nickname, diminutive, 'old man', 'boss', and a variety of less commonly used designations. Uncles may be addressed or referred to as uncle-plus-first-name, first name alone, or uncle alone. Similarly for aunts" (Schneider and Homans, 1955: 1195).

The use of possessive pronouns and specifying whom the relationship is to, increase the variety still further.

The variation, it is claimed, depends on the situational context, not on the traditional distinction between terms of reference vs. terms of address: for it is not possible to give a single form of reference or a single form of address for a given class of kinsmen that would be used in all contexts of reference or of address.

While the variety might give an impression of uncertainty or ambiguity, nothing could be further from reality. Speakers know exactly which one to use, and the differences in practice among various speakers are accepted as legitimate alternatives, not as deviant usage. For the explanation of this fact one has to come back once again to the two distinct elements which can be there, each one separately or in combination. In this light the word 'relative' can be considered to have three meanings: and the word is often used without a sharp distinction being made between these three. In this way, it can be just as correct to say that someone is a relative as to say that he is not a relative. At all events, kinship terms can be applied to persons who are definitely not relatives by blood or kinsmen; in many cases this will be indicated by some such modifier as step, in-law, foster, added on to the basic kin term. These modifiers, however, are by no means required, for the terms by themselves can indicate the code-for-conduct alone. This means that, for Schneider, the set of step-mother, mother-in-law, and foster mother, also contains den mother, mother superior, etc. Yet, father, mother, etc., remain kinship terms because, as distinct from any other kind of term, they have as one of their many meanings the biogenetic relationship. (cf. Schneider, 1968: 100-101).

Kinship terms, then, are not restricted in their use to kinsmen; the other way round it must be said that kinsmen are referred to or addressed by non-kinship terms. This throws wide open again the question 'Who calls Whom What?' (ibid.: 102 ff.). Non-kinship terms can be names, words, or phrases, while kinship terms can combine with names. Both kinship and non-kinship terms divide into formal and informal or intimate terms. The concept of equality has to be introduced here. Symmetrical usage of terms is the mark of equality, asymmetrical usage of inequality. As far as kinship terms are concerned, their use as markers of status difference or equivalence is a special aspect of the more general rule that they mark a code for conduct.

The cultural relevance of this system of designation of relationships seems to be the following. The fairly frequent use of first names brings out as the dominant quality of the system considerations of a personal kind (Schneider and Homans, 1955: 1201). But this requires a qualification: it is elders who use first name towards a younger generation while these use kinship terms for the older generation. Even vis-à-vis higher generations the use of the name plays a role in personalising the more "classificatory" terms such as uncle: they practically become descriptive in this way. This same concern for individual persons is born out by the frequent use of possessive pronouns with kin terms, so personalising them (ibid.: 1203). This value of personal qualities, of the individual, is also stressed within the over-all social arrangements (ibid.: 1206).

"American culture is oriented about achieved rather than ascribed status Ascribed status as a family member is insufficient to carry most people through life. If status is to be achieved, it has to be achieved, according to American values, on the merits of the person, his qualities and accomplishments as a person".

The senior, designated by the kinship term, represents the established system, the achieved status; the junior, referred to by name, is the individual who still has to carve out his position. This dominant cultural value of personal achievement has to be transmitted, to be taught to children, in a social setting. The first social system every human is part of is the kinship system: in this reduced system those dominant values are present for the children to be initiated into. Kinship teaches them more than mere kinship: it teaches them the fundamentals of the whole culture as well (ibid.: 1208).

I have spoken earlier on of the challenge contained in Parsons' remark that kin terminology only reflects social structure in a broad manner: he clearly did not expect much light from the study of kinship terms for his social studies. Schneider too is interested in social studies, but he expects more from the study of kinship terminology, perhaps because he does not expect a simple, not to say simplistic, one-to-one relation between concrete features of the terminological and social systems. Schneider has certainly faced up to some real and important problems, notably the delimitation of the domain of relatives, and the existence and use of alternative terms and forms of reference and address. However, I do not think that his idea of doing away with the reference/address

distinction is particularly fortunate. It is true, of course, that there is an enormous area of overlap between the two and only rarely, if ever, does one find a clear-cut division of kin terminology along these lines. But this does not mean that this distinction is not an effective factor in the process of choosing the correct term for a relative in a given situation. L.S. Lewis has tested some of the Schneider-Homans ideas in the area of terms for parents in American English (Lewis, 1963), and, while roughly in agreement with Schneider-Homans, he did object to the rejection of the reference/address distinction and to the fact that the notion of "situational context" (Schneider-Homans, 1955: 1195) had not been developed. Lewis first tested the preference for various alternative terms in the system of address, in four types of situation, viz. neutral, intimate, conflict, and casual/joking; then he repeated the test for the terms of reference, again in situations of different degrees of formality, viz. reference in the presence of close relatives, close friends, college professors, etc. I would agree that the reference/address distinction is too important to be ignored, but I equally feel that to speak of two sets of terms is, with such a high degree of overlap, artificial. I would rather call them two different modes of use of kinship terms, two types of context. Considered, however, as type of context reference and address are not quite the same thing as the various degrees of formality of which Lewis spoke. The specification of this notion of 'type of context' is certainly something to be looked into; for the moment it will suffice to recognize that Lewis has

made a valuable point in a practical manner.

Returning to Schneider's contribution I feel he did make a good point when he introduced his concepts of 'distance' and of kin terms as status-markers: for it is concepts like these, over and above basic distinctive features, that are required to be able to deal with such facts as fuzzy boundaries and fade-out, and with the variety of alternative terms. On this point Schneider takes a different view from the practitioners of componential analysis of which he has said:

"It is not an analysis of terms for kinsmen. It is not an analysis of kinship terms. It is an analysis of the way in which kin types are classed by kinship terms" (1965: 304):

this, he says, is due to the fact that the domain which componential analysis deals with, is defined by the narrow control question 'What kin-relationship, if any, is he (she) to you?'. Schneider does not deny the validity or usefulness of this approach. What he does say is that it copes with this particular analytic domain: but this analytic domain is not identical with the semantic domain. I think the limiting influence of the control question is generally recognised, indeed intended. Another matter is if it is very helpful to deny the label 'semantic' to this kind of analysis. The question what is part of semantics and what is not, is certainly important. For the moment, however, I shall limit myself to Schneider's ideas about semantics.

When we come to discuss the extensionist theory we shall see that its main claim is to have taken fully into account the polysemic nature of many words. Schneider starts from exactly the same premise:

"[Words] have one fundamental characteristic which must be taken into account. A word never has a single meaning except in one, limiting set of circumstances. When a word is being used within the very narrow confines of a rigidly controlled scientific utterance where the meaning is explicitly defined in unitary terms for that particular occasion or that particular usage, any other meanings that word might have are suppressed and the defined meaning is its only meaning. But since words are seldom used in this way, and rarely if ever in 'natural' culture, this limitation can safely be ignored while the polysemic nature of words is kept firmly in mind" (1968: 4).

We have seen what this means in practice when we mentioned that Schneider holds that there are really three meanings to the word "relative". He agrees that it is not enough to know that there are several meanings: one still has to know which meaning applies when and under what circumstances - this Schneider has shown very well in my opinion - and how the different meanings of the word relate to each other. On this last point Schneider will be attacked by the extensionists: and one has to admit that Schneider's case is perhaps not totally convincing. At first sight the logical possibilities would seem to be that, when there are several meanings for a word, these meanings are either all on the same level and depend on some more general covering concept, or they are not on the same level but organised hierarchically with respect to each other. Schneider's view would not seem to fit either of these possibilities, though it is closer to the first than to the second. His view is of a kind of juxtaposition of meanings of different value or strength, held together, I would say, not by a more abstract cover concept but by a cultural theory about values in society.

There are various matters which require further attention: the role of theory, the place of non-linguistic considerations in semantics, the nature of metaphors. Having considered the good things and the weaker points of the Cultural approach, I think we should now hear the case for other approaches.

4.3. Componential Analysis and Cognitive Linguistics.

4.3.1 The Beginnings and Methodological Basis of Componential Analysis.

The form of semantic analysis known as componential analysis has had strong links with the analysis of kinship terminologies from the very beginning. In this field it served basically as a translation procedure, i.e. a way of explaining a body of unfamiliar kin terms by relating them to more familiar ones through the intermediary of shared components. I think it is fair to say that the original impetus came from anthropological quarters though there has been a linguistic side to it from the start. For one thing, concepts to be analysed are known through or in language:

"The categorizations of phenomena and events ... by which a community's members deal with one another and with their surroundings ... are represented largely, though far from completely, by the words and expressions in their language" (Goodenough, 1967: 1204).

Moreover, anthropologists tend to stress the fact that language is part of culture: "The relation of language to culture is that of part to whole" (Goodenough, 1957: 169). But the strongest, clearest link comes from the fact that

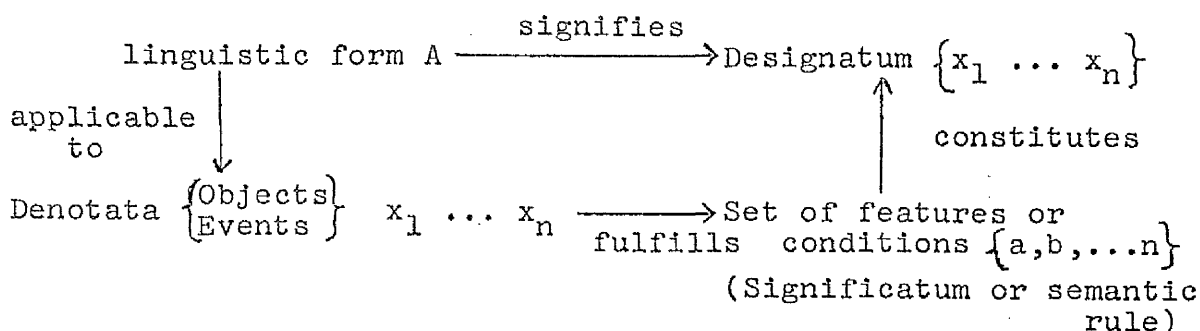
linguistics has provided the model for this type of analysis.

In fact, analysis into constituent components has a long tradition in the study of language, most notably perhaps in the Prague School of Phonology, but the main influence in this present development has come from the work of American distributionalists on phonemics and morphemics, as Goodenough has put it:

"The methodological problem of ethnography thus viewed seems to me to be identical with that of descriptive linguistics" (1957: 168).

Lounsbury has equally referred to the work by Hockett and Harris on morphemics, with portmanteau morphs, a submorphemic level of features, and paradigmatic sets (1956: 160-161), and has drawn a parallel between kin classes and phonemes (ibid.: 191-192). American linguistics did not pay much attention, however, to the study of meaning: yet, for a cultural anthropologist the aim is not to know culture as a material phenomenon but to know the organization of things, the forms of things which people have in mind as signified by what people say and do, by their social arrangements and events. That is why linguistics does not provide the whole of the background: a supplementary inspiration came from the work of Charles W. Morris on the theory of signs (esp. Morris, 1938). From this work came a wider, fuller view of the study of language, not only covering 'syntactics' (the relations of signs to one another), but also 'semantics' (the relations of signs to the objects to which the signs are applicable), and even 'pragmatics' (the relation of signs to interpreters)(cf. Morris, 1938: 6). The further

explication by Morris of the notion of semantics is important because it is at the root of a certain amount of the criticism levelled against componential analysis. It is easiest displayed in the form of a diagram (cf. Brekle, 1972: 34):



or, to put it briefly in words: the meaning of a term is the set of conditions which must be fulfilled if the term is to denote. Meaning is referential, referring to all members of a class. Apart from denoting objects or events an expression can also connote other images or concepts that people associate with the expression's designatum, but this is disregarded in componential analysis: it deals only with signification (cf. Goodenough, 1967: 1204). Lounsbury has summarized it as follows:

"A term belonging to a paradigm can be defined componentially in terms of its coordinates in the paradigm. The definition represents a bundle of features: one from each of several, or of all, of the dimensions of the paradigm. This bundle of features states the necessary and sufficient conditions which an object must satisfy if it is to be a denotatum of the term so defined. Terms having single denotata are the exceptions; multiple denotation is more generally the case. The class of all possible denotata of a term constitutes its designatum. The defining features of this class - i.e. the necessary and sufficient conditions for membership in it - are its significatum.

The componential definition of a term is the expression of its significatum" (Lounsbury, 1964 a: 194).

It is good to realize that one obtains minimal definitions in this way; the rules obtained will only generate back the original data. And in fact the whole operation is carried out on a restricted, carefully delimited domain: this is given as a prerequisite by Lounsbury (1956: 193). The field has been delineated as an "ideal space ... a genealogical one" (Goodenough, 1967: 1204), for which Goodenough has recourse to Rivers' genealogical method, "in order to minimize problems arising from metaphorical usage" (Goodenough, 1956: 98). There is nothing particularly new about this: J. Trier had already posited the need for the semantic field as a closed unit by way of working hypothesis in order to make analysis possible at all (cf. Öhmann, 1953: 127). My reason for stressing this point here is that it has attracted the criticism of circularity: certain terms or meanings are excluded from the analysis because they do not form part of the field which has been delineated to cover only certain terms and meanings. We shall have to return to this point.

The actual practice of the technique of componential analysis consists of two steps: "to make a record of the specific images or concepts that informants say an expression may denote" and "to find a set of definitive attributes that will predict what informants say may and may not be denoted by the expression" (Goodenough, 1967: 1204). Another way of putting it would be that one proceeds from extensional definitions (definitions by listing of denotata) to intensional definitions (definitions by

specification of distinctive features)(cf. Lounsbury, 1964 a: 194).

In previous chapters we have seen how various scholars have tried to relate kinship terminology to social institutions of one kind or another. In a strict understanding of formal feature analysis it could be argued that this point does not arise:

"Since a formal analysis emphasizes internal consistency, completeness, and form, it must first be evaluated in these terms. ... The question of external relevance is no more significant at this stage than is the question: 'Does the "real world" correspond to the operations of algebra?' " (Tyler, ed., 1969: 192).

Tyler may reflect a view here of formalism which prevailed ten years after the epoch-making articles by Goodenough and Lounsbury of 1956: but there is no unanimity on the point and certainly at first no such outspoken position was taken. From early on Goodenough has stressed the link between language and culture: language is part of culture and a major instrument for learning it (1957: 172). Sometimes kinship terminologies reflect not only properties of genealogical space but also features of social organization. In such cases it would be impossible to carry out a componential analysis without referring to social matters (1967: 1208). Lounsbury saw things in the light of the distinction between semantics and pragmatics, semantics dealing with referential meaning, pragmatics with behavioural meaning; for an anthropologist the first would merely be a tool for the second. Eventually the different kinds of meaning would be related to one another, this being "the essence of the 'language and culture' problem" (1956: 189). But there never was any doubt in his mind,

I think, that there is a link: from the outset he stated:

"A language reflects the spirit or the culture of a people. ... The reflection, in language, of such distinctive features of the real or the cultural world, then, must be taken for granted" (1956: 159).

He also, however, refers in this article to Kroeber's opinion that kin terms reflect psychology rather than sociology (ibid.: 184). Lounsbury himself felt quite justified in positing a social explanation, viz. a marriage rule, for the Pawnee terminology he was analysing. But the fact that he did make this reference to Kroeber is, to my mind, very important for a proper understanding of the way attention was shifting from sociology to psychology. Kroeber's categories of relationship are among the factors which have stimulated distinctive feature analysis of kinship terminologies. As such forms of analysis result in 'minimal meaning', the meaning of items within a set in relation to other items in that set, there would seem to be no scope for external factors to be taken into account. The result achieved is rather a conceptual model, reflecting the logic of the mind. Hence, I believe, it was inevitable that recent research should predominantly be interested in the psychological relevance of kin terminological structure rather than in its sociological relevance.

The concept of 'minimal meaning' has another consequence which is at the origin of much discussion: it leads to metaphorical uses of terms being disregarded, at least for the time being, in a componential analysis (Lounsbury, 1956: 193; 1964 a: 206). For componential analysis requires unitary definitions for each class; composite

classes are really two classes:

"Were we to compromise on this point and admit disjunctive definitions (class sums, alternative criteria for membership) as on a par with conjunctive definitions (class products, uniform criteria for membership), there would be no motivation for analysis in the first place" (Lounsbury, 1964a: 194).

What is required then, is to limit oneself exclusively to the terms that fall within the field defined as genealogical kin. Unfortunately Lounsbury discusses at one point an example of emotional extension of class by both a kin relationship and a non-kin metaphorical extension (1956: 175) which would seem to confuse the issue. But he has discussed multiple denotation within a field separately from metaphors in a later article (1964 a: 207). All this, however, leads on to the question of the extensionist hypothesis which will be discussed below (4.4.3.). For the time being it will suffice to have shown how the premises of componential analysis require conjunctive unitary definitions.

4.3.2. Componential Analyses of English Kinship Terminology.

The componential analyses of English kinship terms start off in a fairly simple, basic form; then gradually, they become more comprehensive, more sophisticated. But all along they share a common aim: they want to establish which conceptual criteria are being used by the native speakers. Semantic processes are, therefore, cognitive processes. Inevitably the discussion of cognitive and psychological aspects holds a prominent place in these analyses. As there are a number of studies which deal exclusively with this

subject I have preferred to bring this whole discussion together in the next section (4.3.3.).

The first analysis is the one by Anthony Wallace and John Atkins (1960). Anthropologists have traditionally rendered kinship terms by matching them with primitive English terms or relative products of two or more of these English terms. This way of proceeding is based on Rivers' genealogical method and the results are given in one of several - very similar - forms of kin-type notation. The danger of ethnocentrism in basing oneself on English kin-types is obvious though it may be useful as a tool for anthropologists, especially those with a typological interest. But, as I have pointed out already, the emphasis switched from 'meaning to the ethnologist' to 'meaning to the users of a set of terms': and here it is precisely the principle of grouping relatives which is the crucial point, so that this 'is the one thing above all else which must not be prejudged.

Wallace and Atkins proceed in a straightforward manner (1960: 60): recording a set of terms, defining the terms in kin-type notation, identifying - in the principle of grouping - the conceptual dimensions with their values ('components'), defining each term as a combination of components, and stating the semantic relationship among the terms, the structural principles of the terminological system.

For the first step - recording a set of terms - Wallace and Atkins limit themselves to a restricted set of consanguineal relatives: grandfather, grandmother, father, mother, brother, sister, son, daughter, grandson,

granddaughter, uncle, aunt, cousin, nephew, niece. Their reasons for restricting the set are practical, "for simplicity of expression" (loc.cit.: 61). In a second step they define these terms in kin-type notation (loc. cit.: 61):

grandfather	: FaFa, MoFa	aunt	: FaSi, MoSi, FaFaSi,
grandmother	: FaMo, MoMo		MoFaSi, etc.
father	: Fa	cousin	: FaBrSo, FaBrDa, MoBrSo,
mother	: Mo		MoBrDa, FaSiSo, FaSiDa,
brother	: Br		MoSiSo, MoSiDa, FaFaBrSo
sister	: Si		FaMoBrSo, MoFaSiDa, etc.
son	: So	nephew	: BrSo, SiSo, BrSoSo,
daughter	: Da		SiSoSo, etc.
grandson	: SoSo, DaSo	niece	: BrDa, SiDa, BrDaDa,
granddaughter	: SoDa, DaDa		SiDaDa, etc.
uncle	: FaBr, MoBr,		
	FaFaBr, MoFaBr, etc.		

Next they identify the conceptual dimensions of the set:

- (A) sex of relative for all terms but one (cousin)
- (B) generation, specified in some terms
- (C) lineally or nonlineally related to Ego

with the following values:

- (A): a_1 = male
 a_2 = female
- (B): b_1 two generations above Ego
 b_2 one generation above Ego
 b_3 Ego's own generation
 b_4 one generation below Ego
 b_5 two generations below Ego

- (C): c_1 lineal (ancestors or descendants of Ego)
 c_2 co-lineal (non-lineals all of whose
 ancestors include, or are included in,
 all the ancestors of Ego)
 c_3 ab-lineal (consanguineal relatives who
 are neither lineals nor co-lineals).

In the fourth stage one defines the selected terms by components; the absence of a subscript in the notation indicates that the term does not discriminate on a given dimension.

grandfather	: $a_1 b_1 c_1$	grandson	: $a_1 b_5 c_1$
grandmother	: $a_2 b_1 c_1$	granddaughter	: $a_2 b_5 c_1$
father	: $a_1 b_2 c_1$	uncle	: $a_1 b_1 c_2$ and $a_1 b_2 c_2$
mother	: $a_2 b_2 c_1$	aunt	: $a_2 b_1 c_2$ and $a_2 b_2 c_2$
brother	: $a_1 b_3 c_1$	cousin	: $a b c_3$
sister	: $a_2 b_3 c_1$	nephew	: $a_1 b_4 c_2$ and $a_1 b_5 c_2$
son	: $a_1 b_4 c_1$	niece	: $a_2 b_4 c_2$ and $a_2 b_5 c_2$
daughter	: $a_2 b_4 c_1$		

The same thing can be represented in the form of a paradigm (loc.cit.: 62):

	c_1		c_2		c_3	
	a_1	a_2	a_1	a_2	a_1	a_2
b_1	grandfather	grandmother	uncle	aunt	cousin	
b_2	father	mother				
b_3	[EGO]		brother	sister		
b_4	son	daughter	nephew	niece		
b_5	grandson	granddaughter				

This analysis shows the semantic relationship among the terms and the structural principles of the terminological system. Wallace and Atkins do not claim that theirs is the best representation; only that it is adequate to define the set of terms chosen.

Wallace and Atkins have devoted the major part of their article to unsolved problems in relation to componential analysis; and they have seen very clearly that these depend at least to some extent on implicit or explicit assumptions about componential analysis (*loc.cit.*: 63 ff.). They mention in the first place the problem of homonyms and metaphors, distinguishing non-kinship homonyms or metaphors from kin extensions. The former are excluded by definition, the latter only, as was claimed from the beginning by Lounsbury and Goodenough, when a term cannot be given a unitary definition. This kind of definition is either a single value on one dimension, or a simple class product, i.e. a combination of one value from each of two or more dimensions; in other words, it is a simple class or relative product without the words (or their corresponding operators) "and/or" being used in the definition. Lounsbury and Goodenough tried to avoid as much as possible composite definitions, either by introducing extra conceptual variables (Goodenough, 1956: 118 f.), or by hypothesising that the choice of label is contextually determined, e.g. that formal or informal context might lead to a classification on the basis of different features (Lounsbury, 1956: 180).¹ But,

1. An example would be the use of grandparental terms for affinal relatives of the second ascending generation in familiar use but not when properly defined.

as Lounsbury admits, it would seem to be "a blemish in the system" (ibid.). Wallace and Atkins therefore suggest that 'unitary definition' is an absolutely inadequate criterion for homonymy and they define homonyms instead as

"two or more words, phonemically or graphically identical, which cannot be economically defined on the same set of dimensions without overlap or inclusion of one by the other" (Wallace and Atkins, 1960: 66).

I think that the approach by Wallace and Atkins is much more realistic than the rather rigid insistence on unitary definition. Whether it brings us any nearer to an adequate treatment of homonymy and polysemy in practical terms is another matter. A fuller discussion of these problems is better postponed till we discuss the extensionist theory.

Wallace and Atkins also discuss the problem of definition and connotation, related, as we have seen, to the delimitation of the domain. They do introduce, however, the related matter of synonymy (loc.cit. 67 f.) or what Schneider referred to as the "wide variety of alternate terms". Father, dad, daddy, pop, and old man can all be defined as $a_1b_2c_1$ and in this sense are synonyms. But they clearly do not mean the same thing. Again the problem is created in part by the assumption of minimal meaning within a given set of terms. Wallace and Atkins do not offer a solution: they do, however, propose three different options. One can consider them as same-language synonyms with different connotations: the connotata have then the force of significata, at least as long as one is dealing with culturally or linguistically enjoined connotations, not of course when dealing with idiosyncratic or

optional connotations. Alternatively, one could regard the terms as belonging to different (English) languages: this gets rid of the problem of synonymy but creates a (rather unlikely) problem of translation with a prospect of a multiplicity of componential analyses. Finally, one could make the terms non-synonymous, i.e. give them different significata: this would greatly increase the number of dimensions in the matrix.

The authors discuss the matter of regular reduction of terms in connection with the possibility of displaying them in a single paradigm (loc.cit.: 69 ff.), and finally they go on to discuss noncommutative relational concepts (loc.cit.: 73 f.). It appears that, though it may be possible for the ethnographer to analyse terms as class products, users of many kinship lexicons may well define, cognitively at least, some of their terms relationally, i.e. by means of possessive relational concepts, for example, "my husband's father" for FaLa. It may well be profitable to use a combination of both class and relational calculi: much depends on how one defines the purpose of one's investigation. And it is at this point that Wallace and Atkins reaffirm their view of the aim of componential analysis: "to state the meaning of the terms to the native users of the terms" (loc.cit.: 75). At the same time, however, they recognise that their own psychological interest is not the only one possible. There must equally be room for a social-structural type of analysis. This leads to their distinction between psychological reality and structural reality, i.e. the reality which an individual perceives in his own terms, as opposed to a world of meanings

applied to a given society or individual which is real to the ethnographer but which is not necessarily the world of any other individual. Yet, in the field of the semantics of kinship terminology the two must be related extensionally as they cover the same sets of persons or kin-types denoted by expressions. This points to two problems: ethnocentrism, and indeterminacy or the possibility of a multiplicity of extensionally valid solutions. This is the problem we shall consider in the next section (4.3.3.).

The next description of English kin terms by means of componential analysis was done by A. Kimball Romney and Roy Goodwin D'Andrade (1964). Again we shall only go into the actual analysis here, leaving the second part of their article which deals with the cognitive implications of the various analyses till the next section.

Romney and D'Andrade have introduced a new notational system, not in order to convey a different kind of information from the traditional systems: "The difference is that in this notation, all information is represented explicitly" (1964: 151).

They start with a basic set of symbols (loc.cit.: 148):

- m represents male
- f represents female
- a represents persons of either sex
- = represents marriage bond
- O represents sibling link (used only where
 individuals share both parents, i.e.
 "full" siblings)
- + represents parent link

- represents child link

() represents an expansion

superscripts represent number of expansions

subscripts represent sex correspondences

When this notation is applied to the set of terms with their kin-types analysed by Wallace and Atkins we get the following list:

grandfather:	a+m+m a+f+m	nephew:	aOm-m aOf-m aOm-m-m aOf-m-m etc.
grandmother:	a+m+f a+f+f		(also the following not included by Wallace)
father:	a+m		f=mOm-m m=fOm-m etc.
mother:	a+f		
brother:	aOm	niece:	aOm-f aOf-f aOm-m-f aOf-m-f etc.
sister:	aOf		(also the following not included by Wallace)
son:	a-m		f=mOm-f m=fOm-f etc.
daughter:	a-f		
grandson:	a-m-m a-f-m	cousin:	a+mOm-m a+mOm-f a+fOm-m a+fOm-f a+mOf-m a+mOf-f a+fOf-m a+fOf-f a+m+mOm-m a+m+fOm-m a+f+mOf-f etc.
granddaughter:	a-m-f a-f-f		
uncle:	a+mOm a+fOm a+m+mOm a+f+mOm etc.		
	(also the following not included by Wallace)		
	a+mOf=m a+fOf=m etc.		
aunt:	a+mOf a+fOf a+m+mOf a+f+mOf etc.		
	(also the following not included by Wallace)		
	a+mOm=f a+fOm=f etc.		

The list of kin-types following each term is called the range of that term. The next step will be to reduce the range of each term to a single notational expression, by means of the following rules (loc.cit.: 149):

Rule 1. Rule of minimum difference within range.

"Where two kin types within a range are identical except for a difference in sex markers in the same position, the two kin types may be written as one with an a in the contrasting position". This rule applies first. e.g. Grandfather (a+m+m and a+f+m) may be written: a+a+m.

Rule 2. Rule of sequence difference within range.

"Where two expressions are identical except for one additional 'link' (i.e. a pair consisting of one sex and one relation marker), the 'link' may be written in parentheses".

In this second rule the parentheses indicate an optional expansion. The rule may be applied in sequence but must be labelled with a superscript indicating the number of reductions made, e.g.:

$$\begin{array}{l} m+fOm \\ m+fOm-m \\ m+fOm-m-m \end{array} \quad m+fO(m-)^{0,1,2}_m$$

This second rule is necessary to be able to complete the reduction to a single expression for uncle, aunt, nephew, niece, and cousin. If one were dealing with other kinship systems two more rules might be required, a rule of paired sequence difference within ranges in systems with a parallel vs. cross distinction, and a rule of reciprocals within ranges where one finds complete reciprocals.

The application of Rules 1 and 2 to the ranges of the English system gives the following result (loc.cit.: 150):

grandfather	$a+a+m$
grandmother	$a+a+f$
father	$a+m$
mother	$a+f$
brother	aOm
sister	aOf
son	$a-m$
daughter	$a-f$
grandson	$a-a-m$
granddaughter	$a-a-f$
uncle	$a+a(+a)^{0,1}O(f=)m$
aunt	$a+a(+a)^{0,1}O(m=)f$
nephew	$a(=a)O(a-)^{0,1}a-m$
niece	$a(=a)O(a-)^{0,1}a-f$
cousin	$a+a(+a)^{0,1,2}O(a-)^{0,1,2}a-a$

The analysis must be completed by a structural analysis which is to reveal structural principles rather than produce reduced expressions of ranges. The analysis must proceed on the basis of the following set of ordered rules to be applied to the reduced expressions (loc.cit.: 151 f.):

- sex of relative. Symbol R with two values, R_1 and R_2 .
e.g. grandfather and grandmother differ only by the final sex marker. This value can now be taken out and the expressions rewritten as $a+a+a$, or more simply $+++$.
- sex of speaker. Symbol S, with two values. This variable does not occur in English.

- relative sex. Symbol D, with two values, D_1 (different) and D_2 (same).
- relative age. Symbol A, with two values, A_1 (older) and A_2 (younger).
- reciprocity. Symbol P, for polarity, with two values, P_1 (senior or ascending generation) and P_2 (junior or descending generation).
- sex of intervening relative. Symbol C, with two values, C_1 (cross) and C_2 (parallel).

Application to English (loc.cit.: 152):

Term	Extracted Components	Remaining Expression
grandfather	$R_1 P_1$	/ + + /
grandmother	$R_2 P_1$	
grandson	$R_1 P_2$	
granddaughter	$R_2 P_2$	
father	$R_1 P_1$	/ + /
mother	$R_2 P_1$	
son	$R_1 P_2$	
daughter	$R_2 P_2$	
uncle	$R_1 P_1$	/ + (+) 0 /
aunt	$R_2 P_1$	
nephew niece	$R_1 P_2$	
niece	$R_2 P_2$	
cousin	R P	+ (+) 0 (-) -
brother	$R_1 P$	0
sister	$R_2 P$	

There are various ways of classifying and arranging these five remaining expressions or range-sets.¹ Parsons used to stress the distinction between the nuclear family or "primary" relatives, and those outside the nuclear family. This would give the following paradigm:

Primary	Secondary	Tertiary
/ + /	/ + + /	+ (+) 0 (-) -
0	/ + (+) 0 /	

Wallace and Atkins based their main groupings on the principle of lineality, which results in the following paradigm:

	Lineal	Co-lineal	Ablineal
+2	/ ++ /	/ + (+) 0 /	+ (+) 0 (-) -
+1	/ + /		
0	ego	0	

Romney and D'Andrade prefer a solution which emphasises the core or kernel kin type: this is particularly clear from the fact that cousin is given as a zero generation relative. In the following paradigm the range-sets are contained within the solid lines (cf. loc.cit.: 153):

1. A range-set is a group of kin-type expressions which share characteristic features.

	Direct		Collateral		
	male	female	male	female	
+2	GrFa	GrMo	Un	Au	+
-2	GrSo	GrDa			
+1	Fa	Mo	Ne	Ni	-
-1	So	Da			
0	Br	Si	Co		0

The possibility of several adequate analyses raises once again the problem of cognitive validity and psychological reality. What Romney and D'Andrade think about this matter, and indeed the tests which they have introduced to argue their position, will be discussed in section 4.3.3.

The third major componential analysis of American English kinship terminology was produced by W. Goodenough (1965). In the beginning of his article he refers to the lineality feature which forms the basis for the principal subdivision of the set of terms in the analysis by Wallace and Atkins. They had in fact found this concept in Goodenough: but he himself is no longer satisfied with this way of grouping relatives, basically because - as the paradigm given by Wallace and Atkins shows - the terms brother and sister get classified away from the nuclear family represented by the terms father, mother, son, and daughter, and this is intuitively felt to be unsatisfactory. Goodenough acted as his own informant for this analysis. On the whole

his study is very much in line with the basic approach of componential analysis in the sense that he only deals with terms used in reference, and used in a carefully controlled context of elicitation. He holds the view that kinship follows from biological procreation though it is assumed that biological procreation, marriage, and social responsibility for progeny all go together according to the ideal pattern and usual expectation. The basic denotata are represented in kin-type notation (Hu, Wi, Sp, Fa, Mo, Pa, So, Da, Ch, Br, Si, Sb); all other denotata are represented as relative products of these. When Goodenough gives the complete list of English kin terms (1965: 264 ff.) he notes not only the denotata in kin-type notation but adds descriptions of how the kinship terms are used when the usual expectations about biological procreation, marriage, or social responsibility for progeny are not met. Goodenough's list is, to the best of my knowledge, the most complete one available for English. I shall limit myself to reproducing from this list what is essential to follow his argument plus here and there a specimen of the kind of information he gives about certain terms over and above their denotatum.

1. My father: Fa. A male who has succeeded ego's genitor by virtue of legal adoption, or who otherwise has fully assumed the genitor's place, provided ego has not previously established a positive relationship with the genitor as my father and provided the genitor has fully abdicated (by death or otherwise) all public responsibility for ego, and provided further that ego has been incorporated into the nuclear family of the

- genitor's substitute.
2. My mother: Mo. A female who has succeeded ego's genitrix by virtue of legal adoption, or who otherwise has fully assumed the genitrix' place etc. (as under My father, mutatis mutandis).
 3. My son: So. Male adoptee.
 4. My daughter: Da. Female adoptee.
 5. My brother: Br, FaSo, MoSo. This designatum is given as brother₁ and has three sub-classes:
 - 5 a) My brother₂: Br
 - 5 b) My full brother or my fullbrother: Br
(synonymous with 5 a))
 - 5 c) My half-brother: FaSo, MoSo¹
 6. My sister: Si, FaDa, MoDa. (as under My brother, mutatis mutandis).
 - 7-12. My stepfather, -mother, -son, -daughter, -brother, -sister.
 - 13-18. My father-in-law, mother-in-law, etc.
 - 19-24. My foster father, foster mother, etc.
 25. My husband
 26. My wife
 27. My uncle: PaBr, PaPaSo, PaSiHu, PaPaDaHu. Anyone to whom ego's Pa refers as "my brother", or "my brother-in-law". A PaSi's or PaPaDa's second Hu is less assuredly "my uncle" than the first Hu if ego has already established a relationship with the first Hu as "my uncle".
 28. My aunt: PaSi, PaPaDa, PaBrWi, PaPaSoWi. (As under my uncle, mutatis mutandis).
-
1. It seems unfortunate to use two different criteria in making this subdivision: a lexical distinction between brother and full brother, and a kin-type notational one between Br and FaSo/MoSo.

29. My nephew: SbSo, PaChSo, SpSbSo, SpPaChSo. Anyone who is So to anyone who is ego's Br, Si, BrLa, or SiLa.
30. My niece: (as under my nephew, mutatis mutandis).
31. My grandfather: GrFa (is actually one word as is shown by juncture and stress accent).
32. My grandmother (one word)
33. My grandson (one word)
34. My granddaughter (one word)
35. My cousin: PaSbCh, PaPaChCh. Any "descendant" (cf. 41) of the Br or Si of any Fa, Mo, GrFa, GrMo, "ancestor" (cf. 39), or "ancestress" (cf. 40), without regard to whether ego and alter are in the same or different generations.
36. There is a theoretically infinite set of expressions involving terms 27-34 modified by the adjective great or repeated applications thereof.
37. There is another set of kinship expressions that may be formed with 35 and the numerical adjectives "first, second, third, etc."
38. The expressions "once removed, twice removed, three times removed, etc." may be added to expressions constructed with "first, second, etc." in combination with term 35.
39. My ancestor
40. My ancestress
41. (My descendant)

Owing to the formulation of the control question ("What kin relationship, if any, is he (she) to you?") such kin terms as parent, grandparent, child, grandchild,

sibling, spouse, forefather, are excluded; likewise
 excluded^{are} relative products of the enumerated terms
 since they are not unit lexemes.

I mentioned earlier on Goodenough's concern for not breaking up the integrity of the group of terms associated with the nuclear family. It is with this principle of the integrity of blocks of terms in mind that the analysis is organized. The set of terms for primary consanguines serves as a base for handling the affinal terminology; the distinction between the nuclear family and the group of personal kindred beyond it - which includes relatives to whom lifetime obligations are owed - is based on the same principle.

The terms listed fall into five obvious groups, with some derivative groups (loc.cit.: 270):

- group 1: terms 1-6. None of these terms combines with 36 or 37. Derivative groups: 1 a) terms 7-12: "step-"
 - 1 b) terms 13-18: "-in-law"
 - 1 c) terms 19-24: "foster"
- group 2: terms 27-34. All and only the terms in this group may enter into constructions with the adjective "great (grand)" (36). None of the terms in this group may combine properly with "step-, -in-law, foster", or enter into constructions with the numerical adjectives "first, second, etc."

Derivative group 2 a): terms formed with "great", e.g.
great grandparent.

- group 3: term 35. This term alone may enter into constructions with the numerical adjectives "first, second,

- third, etc." and with the expressions "once removed, twice removed, etc.". It may not enter into constructions with "step-, -in-law, foster" or "great (grand)".
- group 4: terms 25 and 26. They denote only affinal kin-types and do not combine with any modifier.
 - group 5: the secondary set of terms 39-41.

The major lines of division run between terms denoting consanguineal kin-types (groups 1, 2, 2 a), 3, 5) and terms denoting affinal derivatives (groups 4, 1 a), 1 b), 2 in part, 2 a) in part), with group 1 c) apart on its own.

Rather than trying to propose a comprehensive solution for all the terms from the very beginning Goodenough first sets out to establish what variable or variables differentiate the various groups. Looking first at the ones denoting consanguineal kin-types, one sees that group 3 stands out from groups 1, 2, 2 a), and 5 by the degree of collateral distance. To differentiate further groups 1, 2, 2 a), and 5 one has to introduce as variable the degree of genealogical distance between ego and alter, with four values. The next step will be to define variables which will differentiate terms within the several groups. Here Goodenough remarks: "It is at this point that alternative ways of conceptualizing the semantic structure become readily feasible" (loc.cit.: 274). For example, to deal with the terms for the nuclear family one could introduce a variable 'Generation seniority' with three values: senior generation (father, mother), ego's generation (brother, sister), and junior generation (son, daughter). However, if one introduces another variable 'Lineality of relationship' in order to single out brother and sister,

two values will be sufficient for the variable 'Generation seniority'. If, on the other hand, one were dealing not just with the terms of group 1 but also with those of group 2, one could envisage another variable instead of 'Lineality of relationship', viz. 'Relative nearness of alter's generation to ego's': this is a matter of emphasising generational rather than collateral distance. It is difficult to decide which solution to prefer: Goodenough, who acts as his own informant, confirms that he can think about the data in either model. Therefore both have cognitive validity (loc.cit.: 276). The decisive factor then becomes which emphasis fits the rest of the data better: for the present problem, to opt for the (relative) generational emphasis would make it impossible to treat group 5 terms as substitutes for expressions in group 2 a). And as this substitution would seem to be perfectly reasonable it is better not to press the generational aspect (ibid.: 280 f.). But sometimes various ways of looking at the terms will provide equally "true" models, bringing out different aspects of the semantic structure (ibid.: 285 f.).

One variable which is fairly obvious but has not been mentioned before because it is not relevant for group 3 is 'Sex of alter'; this variable also applies in the case of terms for affinals to which we shall turn now.

A variable is needed first of all to identify the affinal terms, i.e. the terms of groups 4, 1 a), 1 b), 2 in part, 2 a) in part. After that it will be a matter of establishing whether or not - and if so: to what extent, with what restrictions - there is a structural equivalence

between certain affinal kin-types and consanguineal ones. This equivalence in fact exists in, and accounts for groups 1 a) and 1 b). There also are occurrences of equivalence in the case of some group 2 or group 2 a) terms, e.g. when a PaSiHu is called uncle: the condition is, however, that the senior partner to the relationship is directly involved in the affinal, marital tie. In other cases descriptive constructions must be used, e.g. "my wife's uncle". And normally the kin term will be reserved to the first person known to fill a certain affinal position: this restriction becomes operative in cases of divorce, for example. If one wants to integrate the affinal and the consanguineal sets the variable dealing with "the degree of genealogical distance between ego and alter" must be enlarged with "or alter's structural equivalent". An extra variable added to the one of 'Lineality of relationship' will take care of the distinction between full brother/sister and half-brother/sister.

There would seem to be a certain inconsistency in the application of the variable of 'Degree of genealogical distance' in that the terms uncle, aunt, nephew, and niece may be extended up and down the generations to any non-lineal consanguineal relatives (excepting group 3) other than ego's own generation, i.e. brother and sister, while for the lineal relatives the modifier "great" must be used the appropriate number of times. This can be rectified by adding an extra variable to deal with "great".

The description of a few terms will suffice to illustrate the basic procedure. I have selected the following terms: [ego], husband, father, mother, stepfather, father-

in-law, brother₁, son, grandfather, grandson, uncle, nephew, and cousin. The following discriminant variables are necessary to analyse these terms, with their numbering as given in Goodenough (1965):

1. Degree of collateral distance, with two values:
less than two degrees, and two or more degrees.
12. Degree of genealogical distance, with three values:
zero distance, one unit of distance away, and more than one unit away.
8. Absence/presence of marital tie.
9. Structural equivalence to a primary consanguineal kin-type, with two values: equivalent/non-equivalent.
4. Lineality of relationship, with two values: lineal/non-lineal.
5. Generation seniority, with two values: senior/junior.
6. Sex of alter, with two values: male/female.
13. Number of units of genealogical distance beyond one, or "great" rule: this rule is in theory open-ended.

Kin terms	Variables							
	1	12	8	9	4	5	6	13
[ego]	1.1	12.1	8.1					
husband	1.1	12.1	8.2				6.1	
father	1.1	12.2	8.1		4.1	5.1	6.1	
mother	1.1	12.2	8.1		4.1	5.1	6.2	
stepfather	1.1	12.2	8.2	9.1	4.1	5.1	6.1	
father-in-law	1.1	12.2	8.2	9.2	4.1	5.1	6.1	
brother ₁	1.1	12.2	8.1		4.2		6.1	
son	1.1	12.2	8.1		4.1	5.2	6.1	
grandfather	1.1	12.3	8.1		4.1	5.1	6.1	13.1
grandson	1.1	12.3	8.1		4.1	5.2	6.1	13.1

	1	12	8	9	4	5	6	13
uncle	1.1	12.3	8.1		4.2	5.1	6.1	13.1
nephew	1.1	12.3	8.1		4.2	5.2	6.1	13.1
cousin	1.2		8.1					

(cf. Goodenough, 1965: 285).

Goodenough's treatment of English kinship terms covers a remarkably wide range: this is reflected in the number of variables required. Yet in his actual analysis he does not touch upon alternative kin expressions (dad, mummy, etc.), let alone upon matters of connotation or metaphor. It is difficult to see how such matters could be dealt with in the framework of a componential analysis and still keep the analysis within manageable limits.

Not unrelated to this reflection is the way I feel about the foster, step, and in-law relatives which Goodenough includes in his analysis. Generally speaking, I would be in favour of including these terms in an analysis as I have reservations about delimiting the domain artificially. But to treat them all on the same level by enlarging the matrix and number of discriminant variables would seem to falsify the relative importance of the various terms within the one domain. This way of proceeding causes a term like cousin, after all one of the consanguineal kin, to be overshadowed by foster or stepkin: the relative importance of variable 1 compared with, e.g. variable 9 is in no way marked and cannot be marked easily, I would think, in this kind of framework.

Robbins Burling (1970 b: 27-33) has given a slightly simplified analysis of Goodenough's material. He

uses only five dimensions of semantic contrast. The first one, which he calls 'genealogical distance', is based on which modifiers English kin terms can combine with: step and in-law, or great/grand, or first, second, etc. and once removed, twice removed, etc.. This categorization is based upon grammatical rather than semantic principles but, when applied, it appears semantically reasonable. The concept of genealogical distance combines those of both generational and collateral distance, and is given the following values: 0 = ego; 1 = Pa, Br, Si, So, Da; 2 = GrPa, Un, Au, Ne, Ni, GrCh; great-2 = GrGrPa, GrUn, etc.; 3 = cousin. Two more dimensions come from the distinction between consanguineal and affinal kinsmen. The consanguinity dimension is given three values: blood relatives or consanguineals, structurally equivalent affinals (i.e. stepkin), and nonstructurally equivalent affinals (i.e. in-laws). The other dimension derived from the field of consanguinity and affinity is based on the difference whether an affinal relationship results from a link where the marriage bond is in the senior generation, or from one where the bond is not in the senior generation. Added to these three dimensions are the simple contrasts of generation and sex. These five dimensions adequately distinguish the majority of English kinship terms from one another: only some modifiers remain to be defined. "Great" can precede certain kin terms and can be applied recursively: with each use the value of the sign of generational distance is increased by one. The various possible modifiers of cousin either specify the degree of collateral distance (first, etc.) or show

how many generations separate the speaker from alter (once removed, etc.). Burling speaks of "dimensions of semantic contrast and the definitions of their various components" on the one hand, and on the other of "a description of the operators great etc." (loc. cit.: 33). I expressed previously some reservation concerning the fact that Goodenough's juxtaposition of features did not seem to account for the relative importance of the various factors at play in the terminological system. A distinction, however, of the kind proposed here by Burling could, to some extent, assuage these feelings.

Cecil H. Brown has reviewed the essential points of the work on componential analysis (C.H. Brown, 1974), concentrating mainly on the question of the psychological reality of analyses. This aspect of his article will be considered in the next section. In the present one I would just like to point out in what way Brown's analysis differs from the previous ones.

Brown agrees with Schneider that componential analysis has its weaknesses, the most obvious one being the limitations imposed on it by the way the analytical domain is defined (cf. Schneider, 1965). Yet, his major criticism does not concentrate on this aspect of previous work but rather concerns the concept of 'meaning' which his predecessors used. Brown - under the influence of Wittgenstein - sees meaning as related with grammatical rules and with ordinary use and he defines it as follows: "The meaning of a word is its actual use according to rules" (loc.cit.: 425). Consequently, he claims, a semantic

analysis should not study "perceptual properties of named objects" but the names those properties are given, and the linguistic uses these names are put to, i.e., the grammatical relationships they enter into with other names. This is what a componential solution should illustrate.

Classification on the basis of sex and relative age correspond obviously to ordinary use and so do the reciprocal uses to which Romney and D'Andrade have drawn attention as well as the bound-form usage which Goodenough made the basis of his division of terms into groups. The following two paradigms illustrate these last two approaches:

Romney and D'Andrade

R_2	+	GrFa	GrMo	Un	Au	+
	-	GrSo	GrDa			
R_1	+	Fa	Mo	Ne	Ni	-
	-	So	Da			
b_2		Br	Si	Co		
		R_4		R_5		

Goodenough

Fa	Mo	a_1
So	Da	
Br	Si	
GrFa	GrMo	a_2
GrSo	GrDa	
Un	Au	
Ne	Ni	a_3
Co		

b_1 : imperfect reciprocals

b_2 : perfect reciprocals

R : specific reciprocal group

a_1 : terms used with foster,
-in-law, and step-

a_2 : terms used with great
(grand)

a_3 : terms used with first,
second, third, etc.,
and with once removed,
twice removed, etc.

Brown now proposes to integrate these two outlooks into one analysis, though this analysis is not, of course, a complete analysis of all the ordinary uses of the terms.

	(a ₁)		a ₂			
	(R ₁)		R ₂		R ₃	
b ₁	Fa	Mo	GrFa	GrMo	Un	Au
	So	Da	GrSo	GrDa	Ne	Ni
b ₂	Br	Si	Co			
	(R ₄)		(R ₅)			
			(a ₃)			
	A		B			

a₁: terms used with foster, -in-law, and step-

a₂: terms used with great (grand)

a₃: terms used with first, second, third, etc., and with once removed, twice removed, etc.

b₁: imperfect reciprocals

b₂: perfect reciprocals

R: specific reciprocal group

(): feature redundant with respect to partitioning

A: can be a member of an ego's "family"

B: cannot be a member of an ego's "family"

(cf. Brown, 1974: 426-428).

The symbols A and B which are proper to Brown's paradigm reflect the structural reality of American society, viz. the importance attached to the nuclear family. The terms which combine with foster, -in-law, and step- are

in fact the only types of consanguineal relatives who belong to ego's "family": as a matter of sociological fact other relatives do not come under ego's "family". This means that the ordinary bound-form usage makes sense sociologically. Conversely, all other bound forms relate to the removal or distance away from the "family": they are in a sense 'degree' words, indicating the degree or extent of the removal or distance, a characteristic which is apparent in the recursive nature of these forms. This leads Brown to the conclusion that his analysis, as opposed to previous attempts, gives us the structural as well as the psychological reality.

This issue of psychological reality is what Brown's article is chiefly concerned with. Before going any further I would like to discuss this matter at greater length especially since the next type of analysis, relational analysis, is - at least in part - inspired by the quest for psychologically more real accounts of kinship terminology.

4.3.3. Cognitive Aspects of Semantic Analysis.

4.3.3.1 Some Preliminary Remarks.

As I have pointed out (4.3.1.), Kroeber's influence on feature analysis has resulted in a strong psychological interest among the practitioners of the art. They are out to find 'conceptual models' which would reflect in some way the logic of the mind. The analysis of language is a tool to help them to achieve this aim. But when I say 'conceptual model' I am not expressing

properly and fully what many analysts really want to achieve, which is to find the actual conceptual criteria applied by the users of a given language: "An ethnographer should strive to define objects according to the conceptual system of the people he is studying" (Frake, 1962: 28).

I mentioned that componential analysis is, in a way, a translation procedure, born from the conviction that there is no easy one-to-one correspondence on a surface level between items in two diverging cultures and languages, e.g. between English uncle (FaBr or MoBr) and Latin avunculus (MoBr only). Frake (loc.cit.) put it like this:

"Instead of 'getting words for things'
the task of the ethnographer is redef-
ined as one of finding the 'things'
that go with words".

He means by this that we must look at people's cognition.

The interest in the cognitive side of language has attracted attention ever since - earlier on in this century - extensive contact with non-European languages showed the untenability of ethnocentrism in linguistics: languages are not just "itemized inventories of reality" (cf. R. Brown and Lenneberg, 1954). One answer to this problem of linguistic ethnocentrism has been the theory of linguistic relativity, linked with the names of Sapir and Whorf. This is not really a single, clearly outlined theory but rather a wide range of views on the matter of the relation between the language of a people and its culture, mentality, world-view, beliefs, or whatever other notion may have been used in this context. Whorf claimed that each language embodied and helped perpetuate a particular world-view, i.e. he claimed that the world is experienced differently in different linguistic communities,

and that language is causally related to these cognitive differences in both its lexical and its grammatical features.

While few people would support the more extreme forms of linguistic relativism it would seem difficult to deny that language plays an active role in man's mental activity. The process of thought itself, though not identical with language, depends on it to some extent. With A. Schaff (1973: 145 ff.) we could define cognition as "a process of thinking which results in a description of reality". Extreme relativism would claim that the entire structure of cognition would depend upon language, that language shapes our image of the world. A more moderate claim would be that language affects the way in which we perceive reality and - more importantly perhaps - that the conceptual possibilities of a given language have an influence on the kind of questions we ask about reality and on the way in which we ask them.

"Cognition at whatever level is a structuring activity The struct-
uralizations are patterns . . . which
the organism imposes on the environment"
(Fearing, 1954: 62).

The cognitive function of language is a way of "digesting" our experiences. It has not been possible to prove the extreme form of relativism:

"The analysis of a culture's termin-
ological systems will not, of course,
exhaustively reveal the cognitive
world of its members, but it will
certainly tap a central portion of it"
(Frake, 1962: 30).

This quotation is perhaps more intuitive and hopeful than conclusive and precise. But supposing there is such a

thing as a conceptual structure in the minds of the speakers of a given language, and supposing that language would give us access to that structure at least in part, one would perhaps have to extend the criterion of adequacy - the central criterion of any formal analysis - to cover this aspect of psychological validity as well.

It would not seem that there is anything in the nature of componential analysis that would make it absolutely necessary for this kind of analysis to be psychologically relevant. But given the insistence on the need to concern oneself with the meaning of terminological systems to their users, one would expect the resulting rules which predict back the data to be as much as possible like the processes by which ordinary users decide which term to use for whom. The value of componential analysis does not depend entirely on this: for comparative or typological purposes it can definitely be a very useful and accurate tool, quite independently of the question of psychological reality. It would, however, limit its relevance from a psycholinguistic point of view if there were no psychological validity at all to componential solutions.

4.3.3.2 The Psychological Validity of the Analysis of Kinship Terminology.

Ever since kin terms have been analysed componentially there has been a certain apprehension about the possibility of alternative solutions. Goodenough was faced with a choice between a number of possible alternative conceptual variables in his analysis of the Trukese cognitive world (1956: 119): he merely indicated the

different possibilities and left the choice open.

Others would seem to be less ready to settle for what could possibly be construed as the easy way out. Wallace wrote for example:

"The commitment to describe the psychological reality of culture requires that not just any model which predicts some overt class of action be accepted, but only that model which is used as a system of reckoning by the actor" (1962: 356).

Yet Wallace and his co-author Atkins were - as we have seen - well aware of what they called the problem of indeterminacy or multiplicity of solutions (cf. above 4.3.2.; Wallace and Atkins, 1960: 76 ff.). They accept that more than one definition may be psychologically real - "in the sense of representing how users think with and about that term" (loc.cit.: 78) - while not every structurally real definition will be real in more than a purely structural sense. Structural reality is achieved when the usage of a term can be predicted accurately from the definition. With psychologically real definitions things are not so easy: ways have to be found to identify the solutions which will be nearest to psychological reality. Wallace and Atkins are unwilling to rely on their definitions being in harmony with observed social facts in order to claim the likelihood of reality for a particular solution. Instead they insist that one should study the semantics of individuals, by asking for simple verbal definitions, by activities of matching and sorting, by getting them to answer hypothetical questions and describe relationships, all this "in order to reveal methods of reckoning" (loc. cit.: 78). Their claim for the outcome of such studies is

very modest:

"the degree of psychological reality achieved in ethnographic reporting is not only uneven but on the average probably rather low. Social-structural reality can be achieved; psychological reality can only be approximated" (loc.cit.: 79).

Even so Wallace and Atkins maintain that it is important to work towards even such imperfect, approximate definitions.

They really leave the question of psychological validity wide open, except for this final remark that psychological descriptions are of greater value. This point is not proved or illustrated and the reason why this is not done could well be the fact that it is one of their basic assumptions: this is what ethnography is about as far as they are concerned, to give

"not naturalistic or statistical descriptions of regularities in overt behavior but descriptions of the rules which the actors are presumably employing, or attempting to employ, in the execution and mutual organization of this behavior. ... a set of such related rules forms a calculus which describes cognitive process" (Wallace, 1962: 351).

Romney and D'Andrade (1964: 153 ff.) discuss the question of alternative componential structures and of isomorphism of componential analysis with cognitive structure. They share the view of Wallace and Atkins on the multiplicity of possible solutions:

"It is our feeling that there will usually be several alternative analyses possible for any set of kin terms. If we are to talk about psychological or cognitive implications of an analysis, we must specify what these implications might be. Probably some analyses will be more useful for some purposes and less useful for others. Thus there may be no single best solution for a given system" (loc.cit.: 154).

They also agree that special procedures will be required to evaluate cognitive aspects of kin terms. This is the point they develop in the second part of their article:

"The general prediction we have made from componential analysis to cognitive measures is that the more components any two terms have in common, the greater will be the similarity of response to these terms. This prediction is derived from the assumption that the components of a term constitute the meaning of that term for an individual; hence, the more components which are shared, the more similar the meaning" (loc.cit.: 154).

Three techniques are used in these evaluation procedures: a listing of kin terms in free recall, the semantic differential, and direct judgements of similarity and difference with the triad method.

In the listing task people were asked to list all the names for relatives and family members they could think of. The result was intended to give an insight into the cognitive structure of the kin terms by the order of recall, the frequency of recall, and the productiveness of modifiers. The position of a certain term on the list and the number of subjects remembering it, both indicate the saliency of that term: on the whole the two measures of saliency gave the same result. An interesting result was that terms were in fact recalled in pairs. The productiveness of the modifiers confirmed the anthropological view that terms for distant kin are less specific or differentiated than those for near kin. Modifiers go with sets of terms; as Romney and D'Andrade had come to discern the same range-sets by componential analysis they see this

as support for the claim that terms are classified according to their componential structure.

The semantic differential technique was used to investigate the effect the componential composition of a term has on other verbal responses made to that term, or, put differently, to see if subjects make a semantic differential rating of a term on the basis of the components which constitute the referential meaning of the term, or respond uniquely to that term as an indivisible lexical item. Though on the whole subjects tended to respond similarly to terms sharing a given component, there are too many unexplained discrepancies for this test to be really conclusive. One possible reason could well be that the semantic differential procedure was never meant to deal with the kind of minimal denotative meaning which is the characteristic aim of componential analysis. The triads test consists in designating from among three given terms the one most different in meaning, the assumption being that the more components are held in common by any two terms, the more similar those terms will be in referential meaning. It appears that all high-frequency pairings which result from this test differ in only one component according to the Romney and D'Andrade analysis but sometimes by more than one if one follows the Wallace and Atkins analysis. It is claimed that the same conclusion also holds good for middle and low-frequency pairings though it is less clear there and more difficult to prove. And there are indications that the subjects use distinctions in their sorting which are on the whole isomorphic with the components of componential analysis. Finally Romney and D'Andrade

stress the need for further work in order to validate the method fully.

I would like, at this point, to make a few observations. If one accepts - as Romney and D'Andrade do - a possible multiplicity of componential solutions there would not seem to be any reason why this could not simply be because there are actual different cognitive systems just as there are different analyses. Moreover, Romney and D'Andrade attributed the rather weak result of the semantic differential procedures to the presence in words of a connotative meaning. To my mind connotative and emotive factors are just as likely to affect the free recall listing and the triad test. For the whole purpose of these tests is to get the reactions of untrained observers and I would expect such people to recall more easily types of relatives who are important to them and group together relationships experienced more strongly by them. The last point seems to be confirmed by the fact that subjects from the college age-group tended to undervalue the relationships of which they had as yet no personal experience. Finally Romney and D'Andrade state that "a major conclusion of this paper is that people respond to kinship terms as if each term contained a bundle of distinct meanings" (loc.cit.: 168). I find it difficult to accept such an atomic concept of meaning, the idea of meaning as a simple collection of a number of features. I would see meaning as a structured whole, a "Gestalt". And a 'Gestalt' has as prime characteristic that it "als Ganzes mehr [ist] als die Summe ihrer Teile" (Porzig, 1950: 199). The elements of such a structure form a stable

configuration and are ordered hierarchically with one element often more prominent. But this criticism concerns perhaps more the general orientation of the componential analyses which we discussed rather than just the point of their psychological validity.

The assumption about psychological validity was not just met by critical questions of a practical nature which can be answered by practical tests: a more theoretical discussion of the issues involved was initiated at about the same time in a short paper by R. Burling, "Cognition and Componential Analysis: God's Truth or Hocus-Pocus?" (Burling, 1964 a). In it he made two points. First he showed that the number of possible solutions in dividing items of a set among individual cells becomes very large indeed as soon as there are more than a couple of items in the set; and the possibilities are practically infinite when certain complicating factors are added to the straightforward, mathematical possibilities. These y / factors are homonymy, the occurrence of empty semantic spaces, the possible use of non-binary components, the presence of parallel components, and the acceptability of redundant solutions. This first point is perhaps not a very good one. Hymes, replying to Burling, has remarked: "The total number of logical possibilities is fully pertinent only if all solutions have an equal chance of being arrived at" (Hymes, 1964: 116). He claims that solutions do not all have this equal chance because componential analysis is question-dependent, i.e. dependent upon the questions asked by the participants in the culture: this restricts the number of solutions actually possible. One

has to discover the questions which reveal the sortings made by the native speakers; only afterwards does one make the assignment of semantic features to the dimensions of the sorting, features which are discriminative semantically for native speakers. It is in the sorting that Burling's calculations of possible solutions apply according to Hymes; in a rejoinder Burling has accepted this clarification (Burling, 1964 b: 120). Hymes claims that making the right sorting is "cognitively empty since it involves only putting the discriminated items into relation with each other" (loc.cit.: 117). I think that it is going too far to make such a sharp separation between 'making the sorting' and 'assigning the semantic features to the dimensions of the sorting'. When we assume that we are talking, not about scientifically trained analysts but about ordinary native speakers, I believe we could say that people tend to recognize the organization of a set at least in part because they know what to expect and look for. They are using in fact, even if unconsciously, the semantic features of the items they are classifying in order to make their sorting. To call this 'cognitively empty' is perhaps not a very fortunate phrase. Burling's second point of contention concerns the cognitive status of solutions. We can assume that Hymes is right in saying that Burling's first criticism lacked foundation because only some solutions will in fact prove possible. If I am right, moreover, in thinking that in this limited number of actually possible solutions the processes of sorting and of assigning semantic features of which the cognitive status is here under discussion are to some extent interrelated,

then this second point really is the major if not sole difficulty. Burling accepts that one or more solutions may predict which term to use to denote an object, by means of a set of rules which state the criteria for such a decision: this is in fact the most modest claim for what componential analysis can achieve. But this does not mean that, because a solution predicts terms accurately, this analysis represents the way in which people reach their decision or construe their world: this more ambitious claim is called an "illusory goal" by Burling. In his view the usefulness of componential analysis is limited to formulating rules which predict the use of terms and providing a check on the completeness and adequacy of one's data. In other words, he considers an analysis as a set of rules which happens to work ("hocus-pocus"), not as the discovery of some psychological reality which would be present in speakers ("God's truth").

As for the tests against behaviour, proposed by a number of authors (cf. above Wallace and Atkins, and Romney and D'Andrade; also Frake, 1964) Burling holds that the rules he advocates predict behaviour in a wide sense including the use of terms, but he denies that cognition could be inferred from behaviour. This makes it futile to even seek what is inside people on linguistic grounds only: it is what he calls "the Whorfian fallacy" (1964 b: 120). Hammel shares this view:

"That internal essence which is 'inside people', in whatever form it may exist, is only knowable when it is outside them, and when it is outside them it takes a variety of forms which can, with effort and good fortune, only be summarized in externally valid and verifiable

ways. Like Burling, I am an 'outside' man and, like Fraake, I suspect that God's truth is is hocus-pocus" (Hammel, 1964: 1169).

Wallace, however, still refused to accept this view (Wallace, 1965). In his opinion a semantic analysis must give the meaning of terms to the native users, not to the analyst, or else the whole procedure is senseless and gratuitous for him. The analyst has to relate information about the structure of classification to nomenclature, by means of techniques independent of the mechanics of componential analysis itself, in order to establish the validity of his analysis. He reaffirms his distinction between structural and psychological reality; while purely formal rules will be sufficient to establish the structural reality and predict the use of terms with parsimony, elegance, and accuracy, more is required for establishing psychological reality, viz. both an accurate prediction and the criteria by which the native speaker arrives at his designation. This is the cognitive, truly semantic system, which requires additional techniques to identify dimensions of classification and logical operations which are demonstrably real to the native speaker, or on the other hand to demonstrate that certain dimensions or operations are the analyst's, not the native speaker's. Wallace is quite correct, I think, when he states that the mere fact of using a universal frame of reference does not give an automatic claim to psychological universality for the elements of that frame. Tests will have to identify the dimensions used by the native speakers: here he quotes with approval the tests by Romney and D'Andrade for measuring semantic saliency. Other tests will have to

be devised to determine how the native speaker 'reckons' his kinship relations, which criteria he uses, both with regard to dimensions and logical operations. Wallace accepts that it will be impossible to arrive at a unique solution: he does believe, however, that it should be possible to determine whether an analysis is psychologically real or only structurally real.

After this theoretical interlude we return to the practical work which has been done. This work continued what Wallace and Atkins and, more particularly Romney and D'Andrade had started.

P.J. Pelto (1966) has made a number of interesting observations while comparing these two analyses. Romney and D'Andrade claimed as their specific innovation and improvement of the Wallace and Atkins analysis their concept of reciprocity. Pelto points out that Ego does not figure in the Romney and D'Andrade analysis: Ego is, as it were, looking in from the outside. In Wallace and Atkins Ego is present in the middle, looking around (cf. above 4.3.2 for details of these analyses). This gives their solution an implicit, built-in factor of reciprocity. At any rate, terms which are clearly reciprocal (e.g. GrFa and GrSo) would be expected to show comparable salience to Ego; but Romney and D'Andrade's own facts contradict this. This flaw can be explained partly by the non-denotative character of saliency (and what is non-denotative is not accounted for by a componential analysis), partly by the composition of their sample of test-persons. Pelto has reservations about the possibility of completely preventing connotative meaning from having an influence on the tests in question. Similarly

he feels that the reduction of all range-sets to single expressions involves a loss of information in cases of a wide range of application, by ignoring the "closeness" to Ego of the various kin-types within the range. Pelto concludes that Romney and D'Andrade's analysis is not necessarily better: their great merit is to have proposed concrete ways of testing results. Pelto suspects that there may well not be one single American English kinship system cognitively speaking, but several systems, different possibly according to age-group, sex, etc. Society does not require that all values are commonly shared in order to be held together; it can function perfectly well as an organization of diversity, of different but congruent cognitive systems.

Romney and D'Andrade had limited their triad test for practical reasons (viz. the number of possible permutations) to eight kin terms, all for male relatives. They were satisfied that this would not affect the reliability of the test. Pelto claimed that leaving out such a natural dimension as sex was bound to affect adversely the value of the results. Nerlove and Burton (1972) applied different testing techniques to the data of Romney and D'Andrade and found confirmation both of the adequacy of their dimensions of collaterality, generation, reciprocity, and sex, and of the assumption that the results would be the same for the whole set which would include both male and female terms. Fillenbaum and Rapoport (1971), using a multidimensional scaling analysis, also come to the conclusion that Romney and D'Andrade's analysis fits the data quite well and can be said to be psychologically valid.

Peggy Sanday has examined the psychological reality of English kin terms by means of an information-processing approach (1968). This approach is intended to be complementary to componential analysis which is said to give a static view of the behaving organism. Her study looks for the format by which kinship terms are stored in memory (cognitive structure) and looks at the way in which these terms are interrelated and used (cognitive process). Two listing tasks are set: (1) "List all the names for kinds of relative and family members you can think of in English", and (2) "List all of your own relatives using the name you would call each one if you were talking about him (her)". The order of recall usually starts with a primary relative, but it is after this that it becomes interesting because it is assumed that the next terms are generated according to the format by which people store the terms in their memory. Then there are the operators and decision rules by means of which the subjects operate over the structure: an analysis of the predominant emphasis in the selection of these rules forms the basis for the classification of the cognitive structure. On the whole the subjects produced similar cognitive structures for both tests. The differences between individuals were hypothetically related to each individual's experiential knowledge of kinship. For this purpose data were collected for the relatives listed, viz. geographical location, age relative to ego, frequency of contact, intensity of ego's affect feeling, and whether the relative was alive or dead. The conclusion was that the psychological reality of kinship terms can be considered

to be the set of kin terms an individual remembers, i.e. the elementary discriminable units stored in the memory, plus the set of elementary operators and decision rules, also stored in the memory, which are the cognitive processes.

The interesting thing about Sanday's article would seem to be, first that she takes the matter clearly away from a static, metaphysical level at which no solution seems to be forthcoming anyhow, onto a practical level where psychological reality is defined from the results of the tests, rather than the tests being used for proving the psychological reality in a general sense. The second point of interest is her reference to operators and decision rules, a pointer towards a "generative" type of analysis (cf. below 4.4.).

The limitations of the componential analysis discussed above and the need for some other type of approach also appear clearly when the acquisition of kin terms is considered. Haviland and Clark (1974) noted that psychologists generally agree about the different stages in the development of children's definitions of kin. There is a pre-categorical stage where a question such as "What is an uncle?" would be answered either by giving a name, or by giving an irrelevant or wrong reply. The first or categorical stage consists of the most primitive definitions by means of perceived property features, e.g. a brother is simply a boy. In the second or concrete relational stage the components have no longer direct perceptual correlates; at this point children would see the need for the presence of more than one child in the family for the word 'brother'

to be of possible use, but the reciprocity of the relationship still escapes them. In the third or abstract relational stage the definitions are both relational and reciprocal: you have to have a brother or sister in order to be one yourself. The componential analyses of Wallace and Atkins and of Romney and D'Andrade just do not provide us with components which could be matched with the different stages discovered in this acquisition hypothesis. In stage one a child could be said to have acquired the sex and generation components, but after that it becomes impossible to establish a correlation. This does not mean that the componential analyses are wrong. Haviland and Clark accept that notably the Romney and D'Andrade analysis may have considerable psychological validity for adult speakers of English: for the acquisition of kin terms, however, these analyses are inadequate because - so it would seem - a componential system cannot cope with the relational and reciprocal aspects of the terms to the extent required by the data on the acquisition of kinship terms. A relational analysis would seem to be a good way to deal with the stages of acquisition of kin terms and even with the order in which different terms pass through these stages. This order is in fact linked with the semantic complexity of a term, i.e. what number of relational components there are in a term and, when several, whether these components (viz. PARENT OF and CHILD OF) occur alone or in combination, and with recursion or without it.

Haviland and Clark also tested the hypothesis that familiarity and experience with different family

members would affect the level of definition. This prediction, however, was not supported by their data (loc.cit.: 42-43), so that the complexity hypothesis remains the preferred explanation of the gradual acquisition of kin terms. Only as an explanation for asymmetries or irregularities within a given level of complexity could one think of the child's own roles as an influencing factor (ibid. 45).

Chambers and Tavuchis (1976) have studied children's understanding of American kin terms. They insisted less on the linguistic aspects or on relational thinking but simply wanted to ascertain how well young children perceive the characteristics which define various kinship terms. They remark that the centrality of personal experience makes it difficult at the cognitive level to handle certain relationships of which they have no personal experience or to recognize relationships, known to them from within their own generation, in a higher generation. Children are more easily struck by concrete traits; yet, there is a grasp of the "necessary and sufficient characteristics" of the terms, not just of ascribed traits. The frequency of contact with specific kin or the composition of the family are of little importance, but nuclear family terms prove less troublesome than extended family terms.

It would seem clear to me from this research that a psychological explanation of the meaning of kin terms from the point of view of the individual cannot be upheld: if an "extension of sentiment" approach would mean that, it would have to be rejected. If one does want to opt for a psychological explanation I would

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suggest it should be a social-psychological one based on the concept of role. A role is defined by the expectations of society, but at the same time we make a role our own. At one moment we are acting a role, the next moment we are in the ranks of the specators: it is this double identity which a child learns to discern gradually. Whether it happens with or through the increasing complexity of their language remains an open question for the moment.

When I mentioned Nogle's book on the semantics and cognition of kinship terminology (cf. above 4.1.2.) I referred to his contribution to the discussion and testing of psychological validity. First he showed the existence of cognitive non-sharing and redundancy (Nogle, 1974: 65 f.). This was done by presenting the informants with various presortings of a limited set of terms and asking them to identify the basis of this presorting. All informants agreed in having dimensions for sex, nuclear/non-nuclear, senior/junior, and generation. The presence of both senior/junior and generation points to redundancy. Their answers showed that they used the genealogical reckoning procedure of the relative product or folk definition (in which one traces a relationship until one reaches the designated kinsman), rather than componential definitions. This way of genealogical reckoning would seem to suggest that the genealogical model was real to the informants. The triad test led to the same conclusion. Next Nogle tried to correlate distinctive features of componential analysis with a set of features drawn from the field of ideal kinship behaviour, the assumption being that this correlation would bring out those features which would be most valid for a semantic-cognitive

analysis (loc.cit.: 68 ff.). The behavioural fact of primary legal responsibility, for example, appeared to correlate with the feature lineal/collateral. He came to the following list of valid features: kin/non kin, senior/junior, male/female, nuclear family/non-nuclear family, and lineal/collateral. As I mentioned, folk definitions were clearly psychologically real for his informants, more so than componential definitions; the psychological reality he speaks of is to be understood as meaning the natural way in which people talk about kin relationships rather than an abstract concept of logic of the mind. Folk definition is akin to generative analysis which is in a sense one possible formal way of representing folk definitions in a systematic manner. So here once again the search for psychological reality seems to point to a generative type of analysis rather than to componential analysis.

The conclusion from all that precedes can only be that the search for one psychologically real or valid model of the domain of kinship in the heads of English speakers has proved inconclusive. This definitely takes away something from the attraction of componential analysis. Componential analysis generalizes - over-generalizes, one could almost say - in order to get at the essentials: if all it achieves is to present one possible way of looking at things, one wonders whether the losses incurred by generalizing are justified. However, before concluding this section I must still refer to the work of Cecil H. Brown (1974, 1976) who has suggested a way of salvaging the linguistic premise he considers threatened by the outcome

of this discussion, viz. that the meanings of words are shared by the speakers of a given language. After reviewing the psychological aspects of componential and relational analyses (1974: 417 ff.) Brown confirms our conclusion that practically everybody accepts the multiplicity of possible solutions with all the implications this has for psychological reality. However, he claims that there is another way of looking at it. To this end he proposes a distinction between empirical facts which are contingently real, and logical facts which pertain to necessary reality (1974: 422 f.; cf. also Brown, 1976). A componential analysis deals with perceptual properties, with contingent facts, contingent upon decisions and interpretations by analysts and informants. Those facts can be interpreted differently by different analysts, hence the multiplicity of solutions. Kinship relational logic, however, does not relate to empirical reality but rather to relationships established solely in the human mind and agreed upon: relational logic is necessarily real, it does not involve individual interpretation and perception but a shared conception of what is logically and grammatically correct. Brown bases himself on Wittgenstein's view of language. According to Wittgenstein, he says, the meaning of a word is associated with the rule or rules for its ordinary use. Rules-of-usage or grammatical rules are shared, conventional part of the public domain; this grammar or ordinary usage alone is shared, this alone deserves the name of semantics for meaning is shared, communicated. The ordinary usage solution, he claims, will consequently have necessary psychological reality. The 'dimensions of

ordinary usage' are: use of terms with words for sex, use of terms with words for relative age, reciprocal usage, and bound form usage (cf. above 4.3.2. for paradigm). Brown has tested his theory on the fourth criterion of bound form usage (1976) by giving a group of subjects multiple choice questions consisting of a bound form followed by several alternative kin terms. The subjects had to fill in which term - if any - the bound form was used with most normally or naturally. The test is supposed to reveal the conceptual dimension of ordinary usage, i.e. how people think they ordinarily use kin terms and bound forms. The tests bear out the results of the ordinary usage hypothesis. Ordinary usage is general, is shared.

I accept that the link with grammatical rules makes Brown's solution perhaps a more explicitly linguistic one than some of the other solutions we have seen. But I am less convinced that a Wittgensteinian view of language is the best way to argue in defence of necessary psychological reality. Wittgenstein defines meaning as being constituted in use in different language-games: this surely makes meaning contingent, in the sense of depending on interpretation. If one would want to argue that this interpretation would somehow follow from the grammatical rules or rules-of-usage so that the solution would be necessarily real, I would want to make the point that to my mind the game we agree to play has other than grammatical rules. I think Brown's account of American kin term usage is valuable and interesting, but I cannot see it as the final answer to the problem of psychological validity. That question remains unsolved if not insoluble. Perhaps Stephen Tyler's view is the best one to take (Tyler,

1969: 77):

"Psychological reality is a limit theory that asserts [that?] the convergence of results from independent approaches (i.e., formal analysis and psychological tests) to a common problem provides a 'nearer approximation' to native cognition. Ethnographic predictability is a limit theory in which the imputed ability of the ethnographer to behave appropriately in some other culture is a 'nearer approximation' to the rules of appropriate behavior in that culture. The important difference between these two limit theories is that they call for 'nearer approximations' to quite dissimilar ends - unless we assume that there is an additional convergence of native rules and native cognition. In that case, both ethnographic predictability and psychological reality could be 'nearer approximations' to native cognition and the two limit theories simply different expressions of the same thing, albeit involving different methodologies".

Whether one doubts - as Tyler does - that native cognition will ever be knowable, or thinks that such a 'nearer approximation' to native cognition is possible, it can, in either case, be a valid and worthwhile working hypothesis.

4.4. Formal, Generative Approaches.

4.4.1. Introduction Remarks.

John H. Winkelman has discussed briefly four possible representations of kinship systems:

"The first representational system is an associative system, which treats the meaning of a word as the set (possibly ordered) of unlabeled associates to that word. The second is a semantic feature system, in which the meaning of a word is given by its values on a set of semantic dimensions. The third system is based on formal logic, involving the use of predicates, relations, quantifiers, and

logical connectives. The fourth is an algebraic system of set mappings in which complex kinship expressions are regarded as sequences of mappings" (Winkelman, 1975: 133 f.).

I am not quite sure which actual system Winkelman is referring to as 'associative': I think it must mean a system on a basis of folk definitions, where kin terms are linked with kinship expressions formulated as folk definitions.

The second system referred to is componential analysis. Winkelman considers this system to be inadequate as well because in none of its forms does it represent all kinship relations nor can it be extended to do so in any natural manner. His criticism is perhaps severer than it need be: componential analysis does succeed in reducing the original list of kin-types to a minimal number of symbols and rules. It defines terms through the joint intersection of the classes formed by the distinctive features. There was, as we have seen (cf. above 4.3.1.) a distinct preference for conjunctive or class-product definitions in semantic analysis. Not all classes, however, can be defined conjunctively so that disjunctive definitions, based on the union of classes, may seem inevitable. It was in this context that we were faced with the problem of homonymy and polysemy. D'Andrade (1970) mentions two ways of avoiding having to treat classes as completely disjunctive: one is the extensionist technique (cf. below 4.4.3.), the other is by formulating a relational concept, by finding a relation which holds between certain properties of each object in a class (see the next section, 4.4.2.).

Winkelman too comes to the conclusion that the

right kind of approach must be relational, based on predicates and relations. He himself prefers the fourth system which uses an algebraic notation rather than logical predicates, connectives, and quantifiers because in his opinion it copes better with complex relations (cf. Winkelman, 1975: 136). He uses as basic predicates and relations 'parent', 'spouse', and 'male'; Greenberg had developed a very similar system already in an early article on the use of formal logic in kinship analysis (Greenberg, 1949). Greenberg had even claimed to need only 'parent' and 'male', having reduced the marriage relation to 'parent of child (i.e. non-parent)'. He does this on practical grounds in order to achieve greater simplicity. I do not think it is a very fortunate solution; but since he is particularly interested in creating a descriptive meta-language for comparative purposes one can see some justification for such extreme simplification.

In a very different perspective M. Bierwisch (1970) also came to speak about the importance of relational components. He touched upon this matter when he was discussing semantics in general, the explanation of how sentences are interpreted and understood, and how they are related to states, processes, and objects in the universe. This involves the analysis of word meaning on the basis of a type of feature analysis, an analysis into components. Not all components represent properties, however: one also needs relational components, "semantic components representing relations between two and perhaps more terms" (Bierwisch, 1970: 172). This is illustrated by means of certain kinship terms: we shall return to this later.

The picture of the formal, generative approach which emerges from these introductory remarks is that of an account consisting of a set of rules which operate on logical predicates or on semantic components. I would like to make a threefold distinction in this approach. First there are the relational analyses which - as was suggested on several occasions in the previous section (4.3.3.2.) - would seem to be relevant more particularly with regard to the discussion of the psychological validity of kin analysis. Secondly we have the extensionist approach which has made a special contribution towards the study of polysemy and metaphor in kin terminology. Finally there is the linguistic approach, the study of kin terms as part of the lexicon in the grammar of a language, in a transformational-generative frame-work.

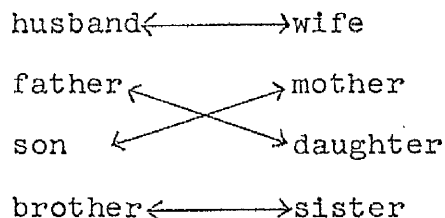
4.4.2. Relational Analyses.

Robbins Burling has presented an analysis of American kinship in a very clear and simple way which insists less on a complete treatment with parsimony and precision and more on the search for psychological reality (Burling, 1970 a). In this latter perspective one gives more attention to additional data, e.g. compatibility with children's usage.

The child first recognizes the categories of sex and age. This gives him four terms:

	Sex	
age	father (husband)	mother (wife)
	brother (son)	sister (daughter)

The subsequent recognition of the relational aspects of the terms, i.e. the fact that they relate to some Ego, refines this system. There now are eight terms, displayed by Burling in the following scheme:



In discovering the special status of husband and wife the child discovers consanguinity as a dimension. At the same time 'age' is replaced by the more abstract dimension of 'generation':

gene- ration	father	mother	husband wife sex
	brother	sister	
	son	daughter	
	sex		consan- guinity

After learning the cover terms parent and child it becomes possible to define further terms. Finally the child learns how these further terms can be modified by one or more operators. Only after having learned the kinship system will the child get to know the application of kin terms to non-kinship domains.

Burling claims that this simple approach is more easily brought in harmony with ontogenetic development than is componential analysis, and in its simplicity it seems more natural. Moreover it makes a case for the

existence of more uniform central parts of kin terminology with increasing variation when stretching out into the periphery. This characteristic cannot be shown in componential analysis which places all terms on the same level.

Burling's simple analysis makes a case for verbal definitions to be used next to the referential definitions which are formulated by means of feature analysis techniques. These verbal definitions, or more exactly the reckoning operations used both in overt utterances and in thinking in order to trace the connection between a relative and Ego, are the basis of A. Wallace's relational analysis (Wallace, 1970). Wallace considers a kinship system as a set of transformations on a basic sentence that states the relation between a specific X (the relative) and a specific Y (the Ego). This sentence is:

X is a | member of the family of | Y.

The formula "member of the family of" is chosen because it also covers adequately the spouse-relationship.

There are fourteen male/female pairs of terms in English which may be substituted for the core part of that sentence

Consanguineals:

cousin/cousin	brother/sister
uncle/aunt	father/mother
nephew/niece	son/daughter

Spouses:

husband/wife

Affinals:

son-in-law/daughter-in-law
brother-in-law/sister-in-law
uncle/aunt
father-in-law/mother-in-law

Steps:

step-brother/step-sister
step-father/step-mother
step-son/step-daughter

Each term can be replaced by a reckoning phrase made up of a subset of no more than three of the following relational phrases:

child of = a
sibling of = b
parent of = c
spouse of = d

with generation distance written as an exponent on the relation. Exponent value 1 is not expressed. This results in:

<u>Consanguineals:</u>	a^m	b	c^n 1
cousin	a^m	b	c^n
uncle/aunt		b	c^n
nephew/niece	a^m	b	
brother/sister	a	b	c
mother/father			c^n
son/daughter	a^m		

-
1. The top line after Consanguineals, Affinals, and Steps represents the general pattern of each of those types of relationship.

<u>Affinals:</u>	d	a	b	c ⁿ	d
son-in-law/daughter-in-law	d	a			
brother-in-law/sister-in-law	d		b		
			b		d
uncle/aunt	d		b	c ⁿ	
father-in-law/mother-in-law				c	d
<u>Spouses:</u>					
husband/wife	d				
<u>Steps:</u>	a	d	c		
step-brother/step-sister	a	d	c		
step-mother/step-father		d	c		
step-son/step-daughter	a	d			

Under "consanguineals" I would have expected the notation 'b' for brother/sister, or 'a c'. I can only guess that Wallace's notation is meant to stand for both possibilities though 'a c' would to my mind be more appropriate as the notation for "half-brother/sister".

As can be seen consanguineal reckoning from X to Y proceeds from X up a child-to-parent line, across a sibling link, then down a parent-to-child line to Y; affinal and step relations are simple transformations of this structure. Since the 'meaning' thus defined is "essentially grammatical", i.e. in the form of a basic sentence, it leads allegedly to a unique solution, the only relational solution possible for the usage of adult English speaking Americans: for relational logic does not depend on any facts extraneous to itself and would therefore be 'necessarily real' in Brown's (1974) terms (cf. above 4.3.3.2.). While the relational approach certainly has much to recommend itself, it is difficult to accept it

as the only 'natural' one for English speakers. One could agree that it is unlikely that e.g. uncle would be defined other than as parent's brother in the kinship reckoning of ordinary speakers of English since the distinction between paternal and maternal uncles is inoperative. An absolute claim, however, on psychological reality would seem to be too strong.

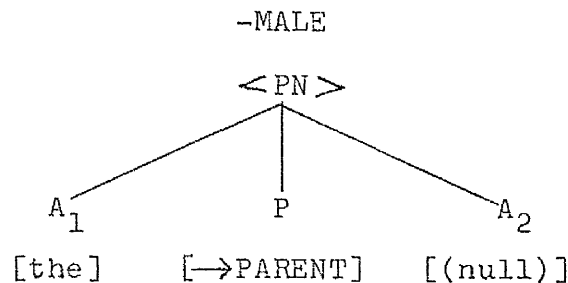
Wallace saw his system as a set of transformations on a basic sentence. The predication analysis of G. Leech seems to have a similar basis. A predication can be compared roughly to a sentence consisting of two arguments linked by a predicate. Each of these three units is a cluster of features. Predicational analysis does not replace componential analysis: it is complementary to it.

Leech's proposed predicational-componential analysis of English kinship semantics (Leech, 1974: 247 ff.) is worked out in much greater detail than Wallace's and has the added advantage of being part of a complete approach to linguistic semantics. Leech starts off simply by two oppositions:

sex $\begin{cases} +\text{MALE} & \text{'male'} \\ -\text{MALE} & \text{'female'} \end{cases}$, and parenthood $\begin{cases} \rightarrow \text{PARENT} & \text{'is parent of'} \\ \leftarrow \text{PARENT} & \text{'is child of'} \end{cases}$.

The second opposition is a relative one because of the converse relation between parent and child. Kinship relations are represented on the predicational level as downgraded predications, i.e. as features which have the structure of a predication, symbolized as <PN>.

Let us take, for example, the term mother:



or: -MALE <the. → PARENT. ∅ > "female (who is) parent of ...".

Since this downgrading structure is general to all kinship terms, the notation can be simplified to: -MALE. → PARENT. The application for lineal kinsmen is straightforward. Grandfather, e.g. will be: +MALE. → PARENT. → PARENT., where the two points '...' indicate an unspecified linking kinsman.

When we try to extend this notation to collateral kin we meet with a problem, as would any analysis using the features 'male' and 'parent' only. Defining brother as +MALE. ← PARENT. → PARENT., would make me my own brother, and by the same token my uncle and my father, and my cousin and myself would be identical. This can be avoided by the introduction of a further semantic primitive, the symmetric relation of siblinghood, ↔ SIBLING. Brother can now be redefined as +MALE. ↔ SIBLING. The derived nature of the siblinghood relation is captured in a 'Rule of implication' which defines it in terms of the basic relation of parent-hood:

"x. ← PARENT. → PARENT. y" entails "x. ↔ SIBLING. y" where $x \neq y$. This siblinghood relation appears in the analysis of all collateral terms. Uncle e.g. is defined as: +MALE. ↔ SIBLING. → PARENT. However, as this definition of uncle covers only FaBr and MoBr, Leech introduces an optional marriage link

for the kin-types FaSiHu and MoSiHu, where square brackets are used to indicate optionality. The full definition for uncle now reads: +MALE [..↔MARRY.].↔SIBLING..
 →PARENT. To account for the terms ancestor and descendant without having recourse to recursive listing and while maintaining the identity of meaning of, .e.g., descendant and grandchild, great-grandchild, etc., another Rule of implication is introduced which derives two new semantic oppositions from the relation of parenthood, viz. (→LINEAL/←LINEAL) and (1 GENERATION/2 GENERATION/...etc.). Lineality will also help provide a unitary definition for cousin. A third rule of implication will revise this definition so as to incorporate the distance-modifiers "once removed, etc...". A fourth rule of implication will finally define the notion of kinship itself. Rules of implication are substitutions of one semantic formula for another. Such rules are sometimes necessary when in an area of lexical meaning different conceptualizations are possible though they destroy the one-to-one relation between formula and meaning. Kinship semantics is such an area. Kinship relations can be seen in terms of the nuclear family relations of parents and siblings, or in a much wider perspective of a family tree, or as a combination of both. For example, nephew can be defined simply as +MALE. ←PARENT..<↔SIBLING., or one can bring in the question of ancestry and define 'nephew' as:

+MALE..<←LINEAL ..↔SIBLING.
 1 GENERATION

The simpler definitions are intuitively felt to be more appropriate and matching with psychological reality.

The advantages of predication analysis are said to be the correct representation of converse relations and symmetric relations while it shows more clearly, moreover, how the specification of a given meaning (e.g. BrDa, SiDa) is subsumed under a general definition (viz. niece). Leech sees the basic oppositions of 'sex' and 'parenthood' as biologically founded and universal, while the derived relations of siblinghood, ancestry, and cousinship, introduced by means of the rules of implication, can be defined in terms of rights, duties, and other social correlates and are culturally relative aspects of kinship semantics.

I mentioned in my introductory remarks to this section (4.4.1.) that Bierwisch had suggested one needed relational components as well as property features in a semantic description of certain parts of the vocabulary, notably for kin terms (Bierwisch, 1970: 172 f.). Haviland and Clark (1974: 29 ff.) have worked out his brief suggestions to show the kind of analysis which would, in their opinion, fit in best with the findings of psychology on the acquisition of relational concepts (cf. above 4.3.3.2). There is a basic relational component [X PARENT OF Y] with its inverse [Y CHILD OF X]. These combine with simple property features such as [MALE X] and [FEMALE X], to give simple definitions, e.g. for mother: [XPARENT OF Y][FEMALE X]. By redundancy rules we know that a more complete definition would contain the features <ADULT X> AND <ANIMATE X>, and that all components are joined by an 'and' connection.

Sometimes there will be a recursion of certain relational components in lexical entries which relate X and Y.

The introduction of dummy entities deals with this, as e.g. in grandmother: [X PARENT OF A][A PARENT OF Y][FEMALE X]. The entry for brother can be expanded by a redundancy rule to an underlying primitive entry containing a dummy: [X CHILD OF A][A PARENT OF Y][MALE X].

Since the sibling relationship is such an important one a simple notation is given for this redundancy rule, viz:

[X CHILD OF A][A PARENT OF Y][X ≠ Y] ⇔ [X SIB Y].

Together with the PARENT OF and CHILD OF relationships the SIB relationship will give the essential entries for the terms of the English kin system. A further relation SPOUSE OF will make it possible to write the entries for terms which may or may not involve a blood relationship, while a 'not' marker, ~, will open the way for the entries for half-brother and step-brother.

A very interesting suggestion is added at the end of this short description (Haviland and Clark, 1974: 32 f.): there probably are besides the defining features other, merely characteristic features which could well be culture-specific. An example would be the fact that an uncle will only be called an uncle if he is an adult. This could be added to the definition, using curved parentheses. We then have for uncle:

[X SIB A][A PARENT OF Y][MALE X] (ADULT X).

By similar devices one could account for the use of uncle in address towards a close friend of the family:

(ADR) : [X-R-A][A PARENT OF Y][MALE X][ADULT X] where [X-R-A] represents the relationship 'good friend of'. It must be noted that ADULT X in this last case is a defining, not a characteristic feature.

The analyses of both Leech and Haviland and Clark are interesting. I believe that a preference for either one or the other would not so much be a preference for a specific form of kin term analysis as a choice for a particular semantic system and theory. Their great attraction is that they both present a balanced mixture of universal and culture-specific elements.

Wallace's analysis is very brief, less an analysis than an illustration of a theoretical point, viz. his claim for a psychologically necessary solution. I remain convinced that no absolute claim on psychological reality can be justified for any analysis, at least at present. I believe, however, that both the section on psychological reality and this section on relational analysis have shown that we can establish a working hypothesis on the psychological implications of aspects of kin term analysis even if we may never be able to reach comprehensive and firm conclusions.

4.4.3. The extensionist approach.

At the end of his detailed analysis of Seneca kinship terminology by means of componential analysis Lounsbury discussed multiple denotation (1964 a: 207). He indicated two ways of dealing with this, either by total class definitions, or by basic member definitions and supplementary rules of extension. What was at first presented as an alternative way of handling a particular problem has since grown into a whole body of theory and application; it is of great importance from the semantic

point of view because of the light it has thrown on matters of polysemy, homonymy, and metaphor, and it is important for the study of kinship because of the explicit stand it has taken in defence of the biological genealogical foundation of kinship.

The semantic background to extensionism is identical with the one we discussed in connection with Lounsbury's work on componential analysis, viz. Morris's concept of semantics. There is a similar kind of strict delimitation here: one does not pretend to give a complete analysis of all the meanings of the kinship terms in a given language, the analysis is concerned solely with the system of classification as such (cf. Scheffler and Lounsbury, 1971: 2).

"The real problem is not what kinship terms mean but the nature of the relations among the genealogical designata and significata of certain words and between those designata and any other designata those words may have" (Scheffler, 1972: 311).

In other words: analysis concerns denotation, not connotation or status relationships (Scheffler and Lounsbury, 1971: 58; Scheffler, 1972: 316 f.). By equating social categories with connotation Scheffler and Lounsbury make it very clear what the position of extensionism is: kinship relations are, at base, relations of genealogical connection.

"In this view, kinship reckoning is based squarely on genealogical reckoning. Extensions are not seen as blanket labelings of 'social groups' characterized by lineage, locality, age-grade, etc., but rather as resulting from a series of derivations, reckoning genealogically from one individual to another in accordance with the principles

that are normal for any given system"
(Lounsbury, 1964 b: 244).

This brings us right into the heart of the discussion about the biological vs. the sociological interpretation of kinship, whether, in other words, kinship is a matter of genealogy, or of relative statuses (with correlated role expectations) between members of different social categories in whose definition the biological connections of individuals are not necessarily relevant (cf. Lounsbury, 1969: 17 ff.). The social interpretation takes its origin in certain anthropological data, e.g. the discrepancy between physical paternity and ascribed social fatherhood, the practice of adoption, etc., and is considered to receive confirmation from certain linguistic data, viz. the classificatory terms which are said to denote social groups. The social point of view was put very strongly by Radcliffe-Brown and has since been defended by all anthropologists who call themselves "social" anthropologists, as well as by Lévi-Strauss and other protagonists of various types of alliance theory (cf. Scheffler and Lounsbury, 1971: chapter 2).

Lounsbury and others reject this interpretation. In a general way it is considered by them to be an extreme form of cultural relativism which has eyes only for what is culture-specific, viz. social structure, at the expense of what is universal, viz. the facts of biology. More particularly, as far as kin terminology is concerned, it is felt that the social interpretation makes the mistake of considering the terms of classificatory systems to be monosemic: Lounsbury has called this "the total category approach" (1969: 21). In such an approach it is assumed

that the meaning of a word is the highest common factor (product of common features) in all of its denotations; and implied in this is the assumption that every denotatum of a term is on a par with every other. Extensionism challenges both assumptions: many words are polysemic, and they have a primary sense and various secondary, extended senses. Polysemy and the process of extension depend on the sharing of some of their distinctive features between words. Polysemy can be based on the narrowing of reference by imposition of additional constraints on the use of a word, or on the widening of reference by the suspension of certain restraints (cf. Scheffler, 1972: 313 f.). It is the sharing of distinctive features, not of features of connotative meaning which is being considered: the former are necessary and sufficient conditions for membership of a class, the latter only accidental conditions and as such unlinkable with the process of extension. If one or more defining features of the primary sense of a word is suspended and replaced by some feature of connotative meaning associated with the primary sense, we have an extension of a special kind, the metaphorical extension: the feature of connotative meaning becomes here a criterial attribute, a sufficient though not a necessary condition for membership (cf. Scheffler, 1972: 318 f.).

When one speaks of primary sense, the word 'primary' needs clarification. It does not refer to what children learn first, nor to the sense which is psychologically more salient or has historical priority (cf. Scheffler and Lounsbury, 1971: 66): one is looking for structural primacy, for the sense which is logically prior

(Scheffler, 1972: 314). This will in fact be one of the first steps in the analysis. One begins by listing the kin terms in a given language and after that one has to establish the focal referents of each term, i.e. the kin-types that fall within the ranges of the various terms when used in their primary senses. It is assumed that the focal types are the genealogically closest kin-type or types in each class (cf. Scheffler and Lounsbury, 1971: 86 ff.). In a next step one has to define componentially these primary ranges of denotations, and eventually their broadest ranges as well. All this leads to the first kind of rule required in the analysis: the defining rule. Each language has at least as many defining rules as it has terms, more if some of the terms are polysemous. In a second phase we have to formulate extension rules which specify the conceptual operations whereby the broad or extended sense (senses) of a term is (are) derived from its most narrow or primary sense. These rules are also called equivalence rules since they specify a structural equivalence of two or more kin-types, an equivalence which is purely terminological (ibid.: 106).

Contrary to what we have seen previously in the case of certain practitioners of componential analysis or relational analysis no claim is made here that this procedure for the reduction of kin-types is the procedure used by native speakers when they reckon the classificatory status of kinsmen of the same kin-type. The concern of extensionists is about the structure of a well-formed cognitive system, not about the structure or operation of the cognitive processes whereby that system is put to use (ibid.: 126).

This does not mean that Scheffler and Lounsbury would accept the distinction introduced by Wallace and Atkins between structural and psychological reality of analyses: on the contrary, they reject this distinction. They maintain that the multiplicity of possible analyses stems from a lack of clarity about the question whether kin terms are kin terms; they claim to eliminate this uncertainty in their approach which is based on the primary sense of genealogically defined focal referents with a range of extended senses. A native speaker does not need to know anything about componential definitions of terms in narrow and broad senses or about equivalence rules in order to use the terminology correctly. These definitional and equivalence rules are general structural principles underlying and relating a number of more specific relationships between terms and the kin classes they designate. If one wants to speak of a psychological or cognitive reality it must be of a type comparable to the (unconscious) reality of linguistic structures (ibid.: 136-150).

The assumption of the social meaning of kin terminological systems was precisely that the terms were socially relevant and that one had to look consequently for the social explanation of a particular type of classification. Extensionist theory does not attempt to explain by reference to social factors why a certain system of kin classification has the structure it has, for social and terminological systems do not always coincide. All extensionists will want to do is to try and show that the system of kin classification is at least

logically and functionally consistent with certain other aspects of a given social order (ibid.: 151 ff.). The extent of isomorphism will vary from society to society.

Extensionist theory does not only disagree with causal explanations on the basis of social factors but also with Schneider's cultural approach (cf. above 4.2). Now that we have discussed both approaches it is obvious which are the principal points of disagreement. Schneider does not work on the basis of the distinction 'denotative vs. connotative vs. metaphoric meaning' as understood by Scheffler and Lounsbury. Code-for-conduct is a distinctive feature of kinship and of the categories designated by American kinship terms in Schneider's view. He bases himself on cultural values, and these values are by definition all of the same kind. Each of these values can give a different meaning to a term, e.g. real father vs. step-father, father-in-law, etc.: in this sense there is polysemy, not in the sense of one meaning being primary and the others being derived. It always comes down to the alleged biological foundations of kinship and one's assumptions about multiple meaning: while I have to admit that Schneider does not always convince me on every point, I cannot see any conclusively convincing argument for either style of analysis. Which one to prefer will depend on what aspect one wants to study more particularly. Whether one or the other can or must be preferred in a linguistic study of kinship is a question I would like to leave open for the moment.

Scheffler has proposed an alternative interpretation of some of Schneider's most crucial arguments

(Scheffler, 1974: 33 ff.). Starting from the fact that in the American system a relative is a person who is related by blood or by marriage, Scheffler claims that this concept is genealogically based. When there is no such link of blood or marriage - the case of adoptive and foster "relatives" - we are dealing with a metaphoric extension of 'relative' since standards of interpersonal conduct are not distinctive features of that category. Step- and in-law relatives are also extensions of the category of blood relatives, but not metaphoric extensions. These are instances of extension by neutralization of a genealogical distinction, viz. the distinction between one's own parents and siblings and those of other people, rather than the introduction of a new criterial attribute. This type of extension is therefore genealogically based: consequently they are cases of polysemy, not of metaphor.

How the terms are used is a different matter from how to give an account of their meaning: sometimes the use of a given expression may be thought to be connotatively inappropriate in a given situation, but this does not affect the definitive meaning. Scheffler points out that in some cases kinship terms are extended metaphorically beyond the domain of relatives: not even the connotative feature of "diffuse and enduring solidarity" is present in such a case, e.g. in the case of a priest being called "father". Schneider calls this and similar usages "all members of the same set" (1968: 100), i.e. members of the set of relatives by law only; in his eyes this usage is therefore not a metaphorical extension. He holds that kinship terms are both terms for kinds of

relatives and terms for relationships; the term father used for a priest indicates such a social relationship, a relationship of the father-child kind.

At this point I shall limit myself to the following observations. Metaphor is a traditionally accepted concept in the study of meaning though the understanding of what a metaphor is exactly can differ considerably from one author to the next. Scheffler's use of this concept is determined by Morris's theory of signs with its denotation/connotation distinction (cf. Morris, 1946: 136 ff.): he is quite clear and explicit on this point. In Schneider's case, however, I cannot see how he would define a metaphor or whether perhaps he would reject the notion altogether. Presumably he does not reject it altogether: when he says that calling a priest "father" is not a case of metaphor, one must assume that he accepts that there are extensions of meaning in certain unspecified cases that would be metaphoric. But the lack of explication of the concept of metaphor diminishes the value of the negation of its occurrence in a specific case as, for instance, that of the priest/father.

As the example of the use of the term "father" for a priest plays such a crucial role in this debate it may be useful to summarize briefly Scheffler's interpretation of this usage and to add my own comments. Being a priest myself, addressed as "father" in my church and accustomed to addressing and referring to fellow priests by that same term, I may be able to throw some more light on this particular usage. Scheffler says that the use of

the term "father" for priests does not express the intention of asserting that priests are genitors or relatives: it only implies that, in some limited respect, the social relationships between priests and other church members should be like the social relationships between genitors and their offspring (Scheffler, 1974: 8 ff.). The similarity is limited: both genitors and priests are entitled to respect, both are expected to look after the welfare - physical and spiritual respectively - of their dependants. But the similarity does not cover the whole field of rights and duties of a genitor and his offspring. A priest is referred to as "father" as by a title. The criteria for belonging to this category are not that he would be acting in some limited way like a genitor should act; they are simply those required for a valid ordination. Scheffler sees this confirmed by the fact that a priest will be called "father" by all church members, not only by his own parishioners; he even receives the title when he does not have a parish at all. Another indication can be found in the practice of priests to refer to or address each other as "father" (loc.cit.: 10 and 17). Priests are metaphoric 'fathers' but they are not metaphoric 'relatives' (ibid.: 47).

I suppose it is correct to say that the dominant feature in the relationship of a church member towards a priest is one of respect and that this has no biological or legal basis but derives from the fact of ordination. This certainly is the way the members of a church would feel about it. However, one could perhaps argue that, on

a deeper level, there is more similarity between a priest and a genitor than the one outlined by Scheffler. It could perhaps be said that a priest is expected to act like a genitor in some limited way after all. The priest gives new, spiritual, life by conferring the rites of initiation. He is duty-bound to provide sustenance for this spiritual life by his teaching and by the administration of the sacred rites. He is entitled to do this by virtue of the powers which he has received through ordination. I would suggest, therefore, that there is a similarity not only on the behavioural level but also on a functional level of right and duty. Perhaps similarity is not the best word: one might prefer to speak of parallelism. For we are not dealing with the extended use of just one term, there is a group of terms involved: life, father, mother, son, daughter; brother, and sister, all interpretable in a physical, biological sense and in a spiritual sense. One could consider the possibility of setting up two separate systems, parallel to one another. I believe, however, that one would miss a possible generalization if they were seen as unrelated. It strikes me as interesting that the points mentioned by Scheffler to support his interpretation, viz. the fact that all priests are called "father" regardless of their work or position, and the fact that priests call each other "father", have their parallel in kin term usage. The first fact is a typical case of classificatory use where 'father' is the term used to refer to the whole category of 'potential fathers' (cf. above, 2.4.2); the second fact has its parallel in the custom of parents calling each other "father" and "mother", sometimes

even when there are no children present. But it must be admitted that the parallelism between the two systems is far from perfect: the most notable difference is that nothing in the spiritual system corresponds to the father/mother relationship. The two terms are used in very different contexts in their spiritual usage: 'father' defines a relation with the totality of the members of a church while 'mother' only refers to the head of a religious community of women. It would seem to depend on the scope of the analysis whether or not to deal with these uses as extensions of ordinary kinship terms or to take them as a special - albeit parallel - system.

After this general discussion of the extensionist theory I would like to give a brief account of two applications of this theory to the kinship terminology of American English. The two accounts appeared more or less simultaneously, in 1973.

R. Casson (1973) accepts the point of departure of Scheffler and Lounsbury (1971) concerning the polysemic nature of kin terms, their genealogical basis, the distinction between primary and secondary senses, and the use of equivalence rules to specify the relation between these various senses. His analysis is concerned with the polysemy of unitary lexemes, i.e. lexemes whose meaning cannot be deduced from their grammatical structure, and it deals with the relation between the focal and the extended members of a given category. He considers 'grandfather', 'grandmother', 'grandson', and 'granddaughter' to be unitary lexemes as well as all in-law terms. The analyses discussed previously took the same line with regard to 'grandfather'

etc., but not for in-law terms. Casson's reason for doing so is that the denotata of, e.g., father-in-law are exclusive of those of father; I do not consider this to be a compelling argument. He uses the kin-type notation of Romney and D'Andrade (see above 4.3.2.). He introduces an asterisk superscript to indicate provisional expansions in order to account for the phenomenon of fuzzy boundaries, i.e. for the fact that not all speakers agree on where to draw the line in reckoning people as relatives. For example, the term grandfather covers the kin-types PaFa, PaPaFa, PaMoHu, SpPaFa, and the extended range expression reads: $a(=a)(+a)^{0,1*}+a+(f=)m$, where the non-parenthesised symbols represent the focal kin-type.

In his equivalence or re-write rules he uses the following conventions:

- 'x' represents male or female (m or f is required, a is not allowed)
- a single dot indicates either the initial or the terminal element of the genealogical chain, i.e. either Ego or the designated kinsman
- three dots indicate either a non-initial or a non-terminal element of the genealogical chain.

Three unordered extension rules will generate all of the extended kin-types of American English. The rules are named after the modifiers they introduce (cf. loc.cit.: 195 ff.).

Rule I: Great (or Lineal) Merging Rule.

$+a... \longrightarrow +a+a...$

"Let any person's parent of either sex as a link to some other kinsman be regarded as equivalent to that person's parent's parent of either sex as a link to said kinsman".

Rule I: Reciprocal Corollary

$$\dots a- \longrightarrow \dots a-a-$$

"Let any linking kinsman's child of either sex be regarded as equivalent to that linking kinsman's child's child of either sex".

This first rule accounts for all extensions to secondary and more distant consanguineal kinsmen, i.e. for the parentheses which enclose plus signs (or minus signs for the reciprocals) in the kin-type notation, as shown in the extended range expression for grandfather.

Rule II: Step-kin Merging Rule.

$$\dots x. \longrightarrow \dots x_i = x_j$$

"Let any linked terminal relative (alter) of one sex or the other (m or f, but not a) be regarded as equivalent to that linked relative's spouse (naturally, of the opposite sex, $i \neq j$)".

Rule II: Reciprocal Corollary.

$$.a \dots \longrightarrow .a = a \dots$$

"Let any initial kinsman (ego) of either sex as a linking relative be regarded as equivalent to the spouse of that kinsman as a linking relative".

This rule is also known as the "married pair principle" or the equivalence of spouses. It accounts for all extensions of affinal kinsmen, indicated in the notation of the range expressions by parentheses containing the symbol for marriage, '='.

Rule III: Half-Sibling Merging Rule.

$$\dots 0 \longrightarrow \dots +a-$$

"Let one's sibling be regarded as equivalent to one's parent's child".

The rule is its own reciprocal corollary. It is a near universal rule which accounts for the fact that half-siblings are terminologically equated with full siblings.

Frank Wordick's analysis (Wordick, 1973)

differs from Lounsbury and Casson on a number of points though his rules are not really that much different from Casson's. Wordick uses transformation rules as well as expansion/reduction rules: these transformation rules do not reduce or expand kin-types but only rewrite them, and they are not recursive. Furthermore he distinguishes three different levels of classification: deep structure, surface structure, and superficial structure. The deep structure

"contains categories which are the direct output of the semantic rules. Stems of kinship terms (i.e., basic terms) label these superclasses".

The categories found at the level of surface structure

"represent the output of taxonomic rules. Kinship terms as they are used in everyday speech are labels for these classes". On the level of superficial structure

"taxonomic categories may be subdivided with even more precision. Modifying elements added to the kinship terms identify these narrowly defined subclasses" (loc. cit.: 1635).

Wordick uses the following operators in his rules (loc.cit.: 1635):

→: is structurally reduced to

>: is structurally transformed into

=: is called

∩: is the reciprocal of

/: in the environment of

To avoid having to reproduce Wordick's personal way of kin-type notation I have preferred to adapt his notation to the one I have been using.

On the level of deep structure there are thirteen categories (cf. loc.cit.: 1637 ff.), displayed componentially along three dimensions: type of relationship (lineal, collateral, affinal), generation removal (+1, 0, -1), and sex of referent:

Fa	Mo	MoBr	MoSi		
Br	Si	MoSiSo		Hu	Wi
So	Da	SiSo	SiDa		

The following ordered reduction rules are necessary and sufficient to derive these basic categories:

1.a. Half-Sibling Rule (Global)

- | | |
|---------------------------|----------------------------|
| i. FaSo \rightarrow Br | iii. FaDa \rightarrow Si |
| ii. MoSo \rightarrow Br | iv. MoDa \rightarrow Si |

b. Affinal Incorporation Rule (American)¹

- | | | | |
|------------|----------------------------|--------|-----------------------------|
| α { | i. HuCh \rightarrow Ch | \sim | vii. FaWi \rightarrow Mo |
| | ii. WiCh \rightarrow Ch | \sim | viii. MoHu \rightarrow Fa |
| β { | iii. HuSb \rightarrow Sb | \sim | ix. BrWi \rightarrow Si |
| | iv. WiSb \rightarrow Sb | \sim | x. SiHu \rightarrow Br |
| | v. HuPa \rightarrow Pa | \sim | xi. SoWi \rightarrow Da |
| | vi. WiPa \rightarrow Pa | \sim | xii. DaHu \rightarrow So |

2.a. Ascendant/Descendant Rule (American).

- | | | | | |
|---|---------------------------|--------|---|----------------------------|
| { | i. PaFa \rightarrow Fa | \sim | { | iii. ChSo \rightarrow So |
| | ii. PaMo \rightarrow Mo | | | iv. ChDa \rightarrow Da |

Rule 1.a corresponds with Casson's Rule III, which Casson called "near universal" and Wordick "global". Rule 1.b, corresponds with Casson's Rule II: its function is to make consanguines out of affines. Where Casson and Wordick differ is in the second part of Rule 1.b which

1. Wordick does not explain the status of the genealogical cover symbols Ch, Sb, and Pa in his rules. I presume they are used merely as notational devices for the sake of a simple, balanced presentation.

deals with in-law relatives whereas its first part dealt with step-kin. Casson considered terms for in-law relatives as unitary lexemes: this seems rather counter-intuitive and unconvincing to me. While Wordick keeps step-kin and in-laws separate he deals with them under one rule and, as he shows (loc.cit.: 1639), Rule 1.b i-vi can be conflated and reduced to two simple forms:

mWi... → m...

fHu... → f...

or: "Let a man's wife as linking relative be regarded as equivalent to that man as linking relative".

"Let a woman's husband as linking relative be regarded as equivalent to that woman as linking relative".

The fact that this generalization is possible shows to my mind that Wordick's solution concerning the position of in-laws is to be preferred. But both Casson and Wordick agree on the explanation of this affinal incorporation rule which is said to originate in the strength and importance of the bond between spouses within the nuclear family.

While Wordick's Rule 2.a is similar to Casson's Rule I it is different in covering also grandfather, considered by Casson to be a unitary lexeme. At first sight one would be inclined to agree with Casson that the equivalence rule only starts operating when the "great-" modifier is applied. Wordick's claim, however, is situated at another level, at deep structure level. His claim is that descent is irrelevant in the American social structure as a structuring principle. Rule 2.a reduces all relatives at the level of deep structure to immediate or primary relatives and it is applicable to both lineal and collateral ascendants/descendants.

At that level no kinsman can be more distant from ego than one generation level, nor can he be more than one degree of collaterality away. The fact that, for instance, father and grandfather are two completely different kinsmen in the way they are perceived by their relatives and in the way these relatives behave towards them is a fact of surface structure.

At this level of surface structure the analysis becomes taxonomic, i.e. establishes classes and attaches labels to them. But first Wordick introduces his transformation rules in order to simplify the taxonomic section. This simplification is achieved by ensuring that only single kernel referents exist for all kinship terms (loc. cit.: 1641 ff.).

3.a Bilateral Symmetry Rule

$$\left\{ \begin{array}{l} \text{i. FaBr} > \text{MoBr} \\ \text{ii. FaSi} > \text{MoSi} \end{array} \right\} \sim \left\{ \begin{array}{l} \text{iii. BrSo} > \text{SiSo} \\ \text{iv. BrDa} > \text{SiDa} \end{array} \right\}$$

3.b Sex-Neutralization Rule

$$\text{MoSiDa} > \text{MoSiSo}$$

Rule 3.a expresses the existing situation in which consanguineally related males and females of otherwise similar status are considered to be equal for purposes of computing relationship. Wordick's choice of transforming paternal relatives into maternal ones rather than the other way round is based on psychological tests which show that most people feel closer to maternal relatives than to paternal ones. Rule 3.b avoids having up to eight different referents for the term cousin. Having thus established the thirteen kernel kin-types one can now label them. If a term labels a class the basic term will be the correct

kinship term; if it labels a superclass a taxonomic rule must be located which corresponds to the reduction rule used in the reduction of the genealogical string to the kernel form. This taxonomic rule will lead to the correct affix to be attached to the basic term. A few selected examples from Wordick's list (loc.cit.: 1644) will serve to illustrate the method:

<u>Kernel Kin-Type</u>	<u>Associated Terminology</u>
Fa superclass	father
via 2.a	grand.....
via 1.b β-in-law
via 1.b α	step-.....
MoBr superclass	uncle
via 2.a	great <u>or</u> grand
Si superclass	sister
via 1.b β-in-law
via 1.b α	step-
via 1.a	half-
MoSiSo class	cousin

Wordick has introduced a third level, that of the superficial structure. At this level excessive information and overspecification are provided through the use of linguistic modifiers (cf. loc.cit.: 1643 ff.). Some of these means of subclassification are quite simple, e.g. "by blood", "by marriage", "paternal", "maternal". Wordick deemed it sufficient to provide rules for the two more complicated cases, grandrelatives and cousins.

The solution for grandrelatives is the simpler of the two. One applies at the end of the process of reduction the following equation:

$$\gamma = \# 2.a-1$$

or: "the number of times the modifying element 'great' appears before the grandrelative term in question is equal to one less than the number of times Rule 2.a was applied in the reduction of the kin-type to which the term refers".

Wordick gives as example how to determine the proper term to be used for one's FaMoFaFaSoWi:

FaMoFaFa(SoWi) → FaMoFaFa(Da)	1.b.xi
FaMoFa(FaDa) → FaMoFa(Si)	1.a.iii
Fa(MoFa)Si → Fa(Fa)Si	2.a.i
(FaFa)Si → (Fa)Si	2.a.i
(FaSi) > (MoSi)	3.a.ii
MoSi = <u>grandaunt</u>	taxonomy

$$\gamma = \# 2.a-1$$

$$\gamma = 2-1$$

$$\gamma = 1$$

Consequently the proper term for one's FaMoFaFaSoWi is great-grandaunt.

The subdivisions within the ccusin class are more complicated; we need two equations in fact, one to compute the degree of collaterality, and one to compute the distance in generation removal. The two equations are respectively: $K = (\# 2.a.i + \# 2.a.ii) + 1$, and

$$C = |(\# 2.a.i + \# 2.a.ii) - (\# 2.a.iii + \# 2.a.iv)|.$$

When applied after the reduction of MoFaMoSiSoDaDaSo the appropriate term proves to be "third cousin once removed".

Finally Wordick deals with variety in usage, alternative terms, and with what Schneider called 'fuzzy boundary' (loc.cit.: 1645 ff.). When faced with the question whether a particular genealogical type is or is not a relative and, if a relative, what term to use for him,

he proposes to approach the problem as transformational generative grammar handles the question of deciding on the grammaticality of doubtfully grammatical sentences. The logic of the system itself, established on the basis of data which are clear and unambiguous, will tell us what the decision must be in each case. Wordick states in fact explicitly (loc.cit.: 1652, note 4) that he wants to give an account of the ideal American kinship system, he wants to describe a competence model, not a performance model. This explains what he means when he speaks of

"boundaries on the extensiveness of their [i.e. the speakers'] kindreds which are narrower than those placed on them by the rules of the system" (loc.cit.: 1647).

Elsewhere (ibid.: 1651) he speaks of "sheer arbitrariness in certain cases":

"It is evident that such 'soft' or 'optional' boundaries, which are not defined by the kinship rules, are not really a part of the kinship system, but are imposed on it".

He suspects that at the level of deep structure American kinship is a fairly monolithic institution, though there can be a great deal of variation on a more superficial level (cf. loc.cit.: 1650).

Wordick's analogy with grammaticalness calls for a short comment. He says that he feels sure that the native speakers will readily accept the results of his reductions and term assignment in problematic cases. This suggests to me, however, that he is thinking of acceptability rather than of 'grammaticalness': and acceptability belongs to the realm of performance, not of

competence. The notion of grammaticalness is a much more theoretical concept and is not based on experience. Moreover, there are degrees of grammaticalness: it is overoptimistic to expect that a clear-cut answer can be found for all doubtful cases. It is not impossible that his analogy with competence really is a way of making a claim in an implicit and indirect manner for the absolute value of his solution. All in all the analogy does not seem very helpful or enlightening. As for a possible claim to absolute value I would like to say that the differences which exist - notwithstanding all the similarities - between Casson's and his analyses, have strengthened my conviction that at present no such claim can be upheld for total systems. All one can say about alternative solutions is that they may possibly highlight better one or other aspect of kinship or kinship terminology, or that they reflect a particular field of interest. When David Kronenfeld tested the extensionist hypothesis by means of a computer analysis (Kronenfeld, 1976) he seemed to express a similar point of view in speaking of "the proliferation of alternative analyses of particular systems (especially of English)":

"The proliferation has forced considerations of alternative goals to which different semantic analyses may be directed, and of the possibility that somewhat different analyses may be required for different goals" (Kronenfeld, 1976: 916).

4.4.4. The Transformational-Generative Approach.

At the end of the previous section we saw that Wordick borrowed certain concepts from transformational generative grammar. He distinguished deep and surface structures, introduced transformation rules, spoke of competence vs. performance, and drew a parallel with grammaticalness and acceptability in assessing the result of his reduction rules. But basically he intended his analysis to be of the same type as Lounsbury's, albeit an improved version of that type.

In this section I intend to deal with two things. First there are a few contributions which have borrowed substantially from the notions and notation used in transformational grammar, even applying the name "transformational-generative" in some cases. These studies try to show what contribution a general transformational generative framework can make to analyses of kinship terminology.

Secondly I shall deal with the studies which are concerned with the light issues of kinship terminology can shed on problems in syntactic-semantic theory within a transformational generative framework.

D. Schneider (1965: 289) remarked already that the domain of kinship is in principle infinite though in practice it is kept finite by what he called "fade-out". It was this characteristic of open-endedness which made analysts look for inspiration towards transformational grammar since it appeared to provide a formal, rule-based model which - while being finite itself - was devised to account for an infinite number of sentences. The format

of this model seemed to be an excellent way of generating the set of kinship terms - both differentiating them from each other and at the same time showing the inter-relationships between the terms.

M. Durbin and M. Saltarelli were the first scholars to borrow various notions from transformational grammar in their article "Patterns in Kinship" (1967). They advocate a formal, i.e. rule-based, account and they refer to the potentially infinite number of terms; they speak, furthermore, of competence vs. performance. The use of a set of kinship terms in any one particular culture and the system that can be induced from those terms is analogous to performance. The study of the universal, non culture-bound maze of relations among the members of a potentially non-finite set of kin-types is analogous to competence. This non-finite set serves as input to the formal mechanism of the kinship rules. These rules are, according to the authors, neither rewrite rules, nor expansion rules, transformation rules, or realizational rules; they call them "identify rules": "an element x is identified as of the category A in the environment $-W-$ ". Their first rule, for instance, is: $x \rightarrow P / \underline{\bar{x}} /$, or:

"an element x is identified as of the kinship category parent (P) in a context relation $/ \quad /$ with \bar{x} (which is the opposite value of x)" (loc.cit.: 10).

Other rules identify the categories Offspring and Spouse while a fourth rule is an indexing rule, increasing the index of any category by one. The rules are iterative as indeed is the entire set of rules. I fail to see what these rules achieve that cannot be achieved more simply by

a genealogical tree, except stress the potentially non-finite number of kin-types. But as the number of actually recognised kin-types is always finite this is not much of a gain. Moreover, the notion of "identify" rule is somewhat obscure to me. Since the notion is hardly explained I cannot really make out what this kind of rule is supposed to do.

After having given these identify rules Durbin and Saltarelli complete their method with a lexical component, a set of unordered rules which map the finite number of kinship terms of a given language onto the correct kin-types generated by the kinship rules. This lexicon provides for a pairwise distinct subclassification of kin-types. The subcategories used in the (American) English system are: male vs. female, Offspring vs. Parent, and Offspring vs. Sibling. Again I am not quite sure how to interpret these categories. It would seem they are rather like logical relations than like distinctive features. I would prefer in any case other proposals, e.g. Wallace's relational phrases (cf. 4.4.2), or the notation used by Kay (see below in this section), or any of the forms of componential or feature analysis, provided they do not split up the category "cousin" in such an arbitrary way as Durbin and Saltarelli would seem to do, viz. 2nd, 4th, etc. Cousin vs. 1st, 3rd, 5th, etc. Cousin; moreover, for some unknown reason they place their second group of cousins in the same set as the in-laws (cf. loc. cit.: 8). Unless I have completely missed the point of this article I do not think it is particularly revealing or helpful towards a better understanding of kinship and

kinship terminology in general, or English kinship terminology in particular.

Others who have been inspired by transformational grammar without actually working within a generative framework are Philip K. Bock, "Some Generative Rules for American Kinship Terminology" (1968), and H. Stephen Straight, "The Transformational-Generative Grammar of American English Kinship Terminology: A Revision of Bock's 1968 Analysis" (1976). The title of Straight's article is interesting: kinship terminology is not studied as part of the lexicon or of the semantic component of the grammar of a given language. Kinship terminology is itself seen as a language, and the analysis of that terminology as a grammar. In Bock's words, this grammar

"will automatically generate (as 'terminal strings') all American kinship terms and only terms which are acceptable parts of the American system of terminology" (Bock, 1968: 1).

The result is said to account

"for certain intuitions which most English speakers have about their kinship terminology (especially the ambiguity of several terms)" (ibid.).

The rules "can generate an infinite number of kinship terms.... They also assign a structural description to each term" (ibid.: 4). These few quotations from Bock's article read almost like a short characteristic of transformational grammar but transposed onto kin terminology. Straight subscribes completely to Bock's objectives (Straight, 1976: 157 and 163); he merely wants to introduce some refinements and improvements.

I shall now sketch briefly how the analysis

operates and I shall follow Straight's version for this purpose. The differences between his analysis and Bock's come down to two things mainly. Bock gave separate derivation rules for step-, half-, and -in-law terms without reference to the rules which generated the corresponding consanguineal terms. Straight makes this connection quite explicit. He thus makes it clear that we are not dealing with homophones while, moreover, the extra step now required in the derivation of the compound terms makes it possible to read off more accurately from the number of rules used the difference in 'distance' between, e.g. father and father-in-law (Straight, 1976: 163). Secondly Straight proceeds in a more systematic manner, giving first the taxonomic expansion rules, then the transformations, and finally the lexical-insertion rules. In Bock's analysis all the rules were ordered but not separated by kind. Straight's method makes it possible to assess more easily the role of the transformational component: this is where the special character of any given kinship system allegedly shows itself (loc.cit.: 164).

Straight uses the following symbols and operators (loc.cit.: 159):

R	relative
Af	affine ('relative by marriage')
Con	consanguine ('blood relative')
m	Male
f	female
Sp	Spouse
RSp	relative through (marriage to) spouse
RP	relative through ancestor('s sibling or through parent)

RSb	relative through sibling (or sibling's child)	
RCh	relative through child	
P	ancestor	& 'who is'
Sb	sibling	- ' 's ' ,
C	descendant	∅ (null symbol)
Pa	parent	X (variable)
Ch	child	() optional symbol
		→rewrite
		⇒transformation
		⇒lexical insertion

A selection from each of the three sets of rules will illustrate the method of analysis (cf. loc.cit.: 159 ff.):

- Symbol-expansion Rules

1. $R \rightarrow \begin{Bmatrix} Af \\ Con \end{Bmatrix} \& \begin{Bmatrix} m \\ f \end{Bmatrix}$
2. $Af \rightarrow \begin{Bmatrix} Sp \\ RSp \\ RP \\ RSb \\ RCh \end{Bmatrix}$
4. $RP \rightarrow \begin{Bmatrix} Pa + Sp (+ Ch) \\ P + Sb + Sp \end{Bmatrix}$
6. $RCh \rightarrow Ch + Sp$
7. $Con \rightarrow \begin{Bmatrix} (P) (+ Sb) (+ C) \\ Pa + Ch \end{Bmatrix}$
8. $P \rightarrow Pa (+ P)$

- Transformations

11. $(Ch)_1 (+) Sp_2 (+) \begin{Bmatrix} Pa \\ Sb \end{Bmatrix}_3) \& \begin{Bmatrix} m \\ f \end{Bmatrix}_4 \Rightarrow 1+3+4+INL$ (unless 1 and 3 are both null)
13. $Pa + Ch \Rightarrow HLF + Sb$

- Lexical-insertion Rules

20. $INL \Rightarrow \underline{-in-law}$
21. $HLF \Rightarrow \underline{half-}$

26. (Sp &) Pa & Sb (& Sp) & $\left\{ \begin{array}{l} m \Rightarrow \underline{\text{uncle}} \\ f \Rightarrow \underline{\text{aunt}} \end{array} \right\}$
28. Ch & $\left\{ \begin{array}{l} m \Rightarrow \underline{\text{son}} \\ f \Rightarrow \underline{\text{daughter}} \end{array} \right\}$
29. Sb & $\left\{ \begin{array}{l} m \Rightarrow \underline{\text{brother}} \\ f \Rightarrow \underline{\text{sister}} \end{array} \right\}$

Applying some of these rules we can produce the following sample derivations (loc.cit.: 161 ff.):

'half-sister'

R

1 \longrightarrow Con & f

7 \longrightarrow Pa + Ch & f

13 \Rightarrow HLF & Sb & f

21 \Rightarrow half- & Sb & f

29 \Rightarrow half- & sister

'uncle'

MoBr, FaBr

R

1 \longrightarrow Con & m

7 \longrightarrow P + Sb & m

8 \longrightarrow Pa + Sb & m

26 \Rightarrow uncle

MoSiHu, FaSiHu

R

1 \longrightarrow Af & m

2 \longrightarrow RP & m

4 \longrightarrow P + Sb + Sp & m

8 \longrightarrow Pa + Sb + Sp & m

26 \Rightarrow uncle

'son-in-law'

R

1 \longrightarrow Af & m

2 \longrightarrow RCh & m

6 \longrightarrow Ch + Sp & m

11 \Rightarrow Ch & m & INL

20 \Rightarrow Ch & m & -in-law

28 \Rightarrow son & -in-law

The two derivations of 'uncle' show how this analysis deals with ambiguity of terms. Variance in usage could be handled by means of optional rules.

The rules given by Bock and Straight are not complete, they do not cover all possible cases of extended usage, as Straight admits. E.g. as the rules stand they would not generate the terms nephew or niece for the child of a half-sibling (loc.cit.: 165). There is no reason to doubt, however, that the rules could be developed to cover such usage. There are other restrictions to which he admits which are of more interest. The rules as given are insufficient to explain syntactic facts. He gives the example of "wife's brother" vs. "brother-in-law", seemingly identical expressions. Yet, one can say: "My wife's brother gave her a present", intending her to refer back to wife. This reference is excluded in the paraphrase: "My brother-in-law gave her a present". This would seem to be an example of the phenomenon of "anaphoric islands" one of the problems which generative semantics was intended to deal with (cf., e.g. Leech, 1974: 338). But it is not so clear how one could block the choice of this type of alternative designation within the framework of the present analysis. It is just one more manifestation of the limitations of what I have called "minimal meaning", i.e. definitions on a genealogical basis only (see above 4.3.1). Such kin-type definitions make it difficult to distinguish between "wife's brother" and "brother-in-law". Both Bock (loc.cit.: 5) and Straight (loc.cit.: 165) - the latter in stronger terms than the former - indicate that a genealogical model will never be sufficient to

describe adequately actual usage of kin terms. They are thinking at this point less of syntactic problems such as pronominal reference than of the question of connotation.

Expressive and pragmatic connotations often take precedence over genealogical significations. Such connotations, too, constitute a kind of meaning which must find a place in a semantic description. Relegating non-genealogical usage to an area of associated meaning is, in Straight's view, rather uninformative.

The transformational-generative analysis proposed by Bock and Straight would seem to be a valid alternative to other rule-based types of analysis, nothing more, nothing less. Each type has its own special interest: relational analysis approaches the subject from the way we reckon kinship, equivalence analysis insists on the hierarchical structure of the domain and the relations obtaining between primary and secondary senses of kin-types, while transformational-generative analysis concentrates chiefly on the potentially infinite number of relationships, the open-endedness of the field of kin-types, and relates this to the limited number of kin terms available. Transformational analysis achieves its aim by treating kinship structure as a grammar with non-terminal elements, taxonomic rules, transformations, and lexical-insertion rules. We should not, however, draw too close a parallel with the original meaning of these notions and their mutual order and relations. For where lexical insertion takes place, what the transformations operate upon, and whether there is a taxonomic base component are highly

sensitive and crucial questions in the present-day debates among transformational grammarians. The discussion of kin terms within the generative framework is very much linked with these discussions. The last developments in the study of English kinship terminology take us right into the heart of this discussion.

The discussion of the place and function of semantics in a transformational grammar has crystallized around the notions of interpretive and generative semantics. Interpretive semantics recognizes a base component which has the deep structure as output. This deep structure is semantically interpreted by the semantic component. The lexical items inserted in the deep structure are in the form of sets of distinctive features. Only after the lexical insertion do transformations apply, though in the Extended Standard Theory some aspects of the surface structure, notably word order, can also serve as input into the semantic component: semantic interpretation thus can take place at more than one stage. The semantic component translates deep structures into symbolic representations of the meaning of sentences, into semantic representations.

In generative semantics the structures generated in the base component are semantic representations, not deep structures. The claim is that not only sentences with the same deep structure have the same meaning but that the reverse is equally true: that sentences which have the same meaning share the same underlying structure. This underlying structure must be understood as more abstract in nature than the deep structure of the Standard Theory.

Generative semanticists claim to be able to establish wider generalizations. In the Standard Theory transformations were used to account for non-lexical synonymy; in generative semantics the same means are also used to describe lexical synonymy. Internal semantic structure is, in other words, no different from sentence structure generally. Consequently this semantic structure can be represented in the form of tree structures instead of sets of distinctive features. It is inappropriate here to examine all details of this discussion and the various arguments used. Whatever either side may claim, it would seem to me that the matter is still wide open. Moreover, though the cover terms 'interpretive' and 'generative' semantics have their use, it is not always easy to characterize the position of any single author in such a clear-cut manner, nor do all scholars who defend either interpretive or generative semantics agree in every detail on the theory they support.

All those who have discussed how to account for English kinship terminology within a generative framework would seem to favour on the whole the position of generative semantics. But before I come to those accounts it might be useful and interesting to refer briefly to a study by Chomsky in which he uses kinship terms as illustration of a particular point, and to a reaction by P. Seuren on Chomsky's understanding of the meaning of kin terms.

In order to prove that semantic representation must be different from the deep structure of sentences Chomsky develops an argument in which he gives the following three expressions (Chomsky, 1972: 85 f.):

- (1) John's uncle
- (2) the person who is the brother of John's mother or father or the husband of the sister of John's mother or father
- (3) the person who is the son of one of John's grandparents or the husband of a daughter of one of John's grandparents, but not his father

Generative semantics would allegedly claim that these three expressions must have the same semantic representation. But when each of these expressions is inserted into the following context:

(4) Bill realized that the bank robber was _____
 the resulting sentences will not be paraphrases; given the right conditions each might be true and the other two false. Consequently these three sentences have different semantic representations. Standard Theory accounts for this fact by deriving (1)-(3) from three different deep structures. It further assigns such intrinsic lexical semantic properties to the verb realize that the meaning of sentences with this verb (and other similar verbs) of the form "NP realizes that p" depends not only on the semantic interpretation of p but also on the deep structure of p. Seuren (1975: 113 ff.) does not think that these objections by Chomsky against generative semantics can be upheld. He claims that Chomsky's reasoning is based on two misunderstandings. Firstly, there is a confusion between the meaning of p and what p entails. The truth of a proposition p can contain the truth of a complicated disjunction as the truth of (1) entails the

the lengthy disjunction of (2) and (3). But it would be absurd to require that the complete disjunction be included in the semantic representation of a sentence. There is no reason why somebody who "realizes that p" should also be aware of all the propositions that follow logically from p: meaning and entailment must be kept separate. Secondly, the or disjunction in (2) and (3) is not necessarily part of the semantic representation of uncle, it can very well be considered to be part of the meta-language of the grammar in the sense that the grammar would specify that the term uncle can be inserted into the phrase-marker to replace either the subphrase-marker "brother of x's mother" or the subphrase-marker "brother of x's father" or ... etc. Or does not normally form part of the semantic specification of lexical elements except perhaps in the case of homonyms. Seuren does not believe that uncle is homonymic in any of our West European languages: it forms one coherent notion. He sees no problem in explaining how the sentences resulting from the insertion of (1), (2), or (3) respectively into (4) differ in meaning. Only in the case of (1) can the normal semantic realization for uncle be used. In the other two cases, what Bill realized was the truth of a rather complicated disjunction, a sentence in other words with as highest predicate the semantic verb or, and not 'uncle'.

Chomsky has one further reference to kinship terms in the book quoted (1972: 143, note 20):

"If a child were born to a brother-sister marriage, the father would not be its uncle. Laws against incest is [sic] no part of the meaning of uncle. Therefore Q [i.e., the subphrase-marker generating uncle] must contain the information that an uncle is

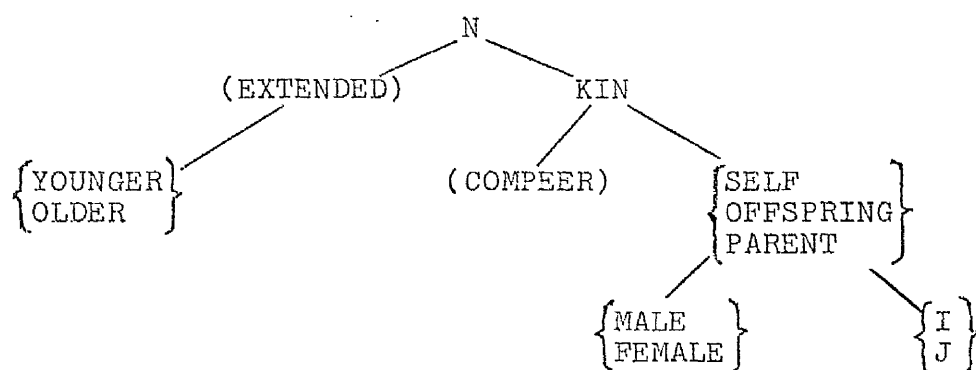
distinct from the father, a fact that requires still more elaborate collapsing rules".

It would seem that the information that an uncle is distinct from the father will have to be part of the semantic description of 'uncle' anyhow and this is bound to bring about some extra complication in any system. But more interesting is Seuren's comment (loc. cit.: 116) that Chomsky's observation only goes to prove that objective reality is less relevant for the way we speak than the way our cognitive system is organised. In the cognitive world of the child of a brother-sister marriage the father is not an uncle. This is not a linguistic fact but a fact of cognitive reality. It is only this cognitive reality, at work in our language, which can explain changes in the range of application of certain terms. Consequently, this cognitive reality will have to be taken into account in a semantic study of kinship.

One of the first linguists to feel dissatisfied with the semantic approach within the Standard Theory was Jeffrey S. Gruber (cf. Gruber, 1965). Gruber began to give the semantic-syntactic information in the form of a tree structure which consisted of syntactic categories as well as feature-like subcategorizations. Lexical elements are not all inserted at the same point in the derivation and not necessarily before the transformations are applied. The lexical attachment is polycategorial, i.e. a lexical item may be attached to more than one terminal node at the same time. In later developments of generative semantics lexicalization will take place whenever a suitable

configuration occurs in the normal course of the transformation processes: the separate lexical component will then have disappeared altogether.

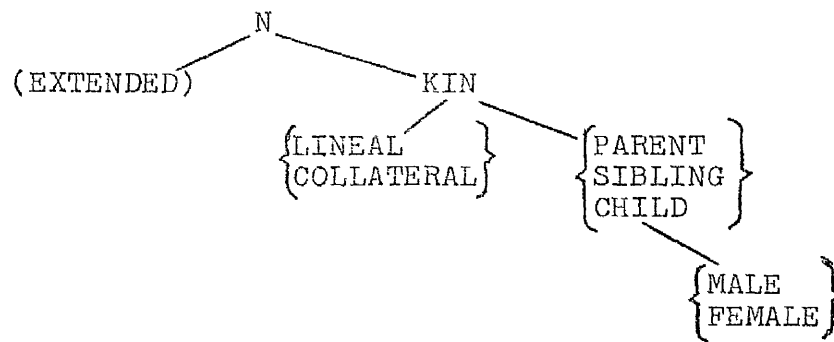
In his transformational analysis of #Hõã Kinship Terms Gruber draws up the following base structure containing the categories with which categorical meaning assignment takes place (Gruber, 1973: 440):



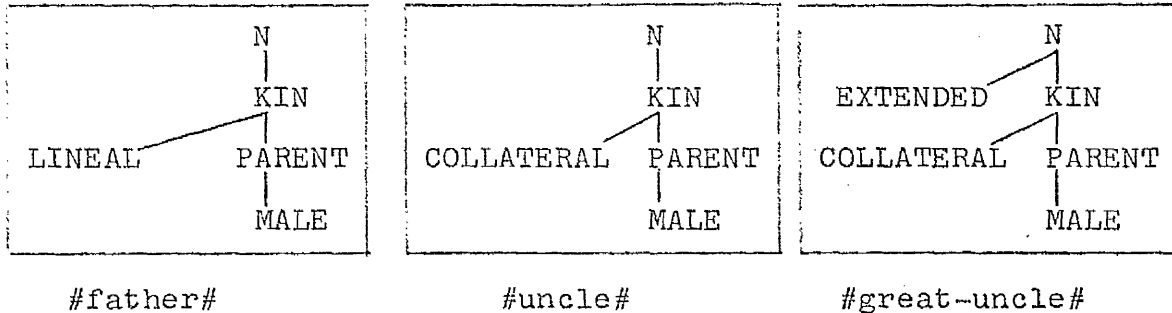
where COMPEER stands for "kin on the same relative age level (as a given kin)" (loc.cit.: 443), and I and J respectively for "on father's side" and "on mother's side" (loc.cit.: 435).

"The categorial specifications of meaning are to be regarded as the environments (with a constituent structure as yet undescribed) in which the respective kinship terms are attached to the base, consisting of categorial constituent structures generated by a system of rewrite rules. The lexicon consists of the set of lexical entries, each of which is a term or item paired with its environment for attachment" (loc.cit.: 438).

Casson has tried to show that - though the basic orientation is very different - the approaches of transformational linguists and of anthropologists have much in common fundamentally and are complementary in a way (Casson, 1975). He has adapted Gruber's base structure for English kin terminology:



and given the following samples of lexical entries:



(cf. Casson, 1975: 324 f.).

Comparing this Gruber-type of analysis with anthropological studies Casson points out first of all the similarity between the semantic categories of the lexical entries in the transformational analysis and the semantic components of componential analysis. Gruber himself calls the semantic categories "specificatory categories rather than distinctive features" (1973: 438) since it is their presence or absence which specifies meaning, not a particular value of an obligatorily present feature. Though the exact status of semantic features or components will probably differ slightly from author to author there can be little doubt that there is considerable similarity. Consequently Casson would seem to be right in saying that anthropologically inspired componential analyses are useful and relevant to linguistics. The second aspect on which he comments, viz. the optional inclusion of such categories as EXTENDED, brings him to a

similar conclusion: categories of this kind deal in a succinct way with the facts accounted for in detail by the rules of equivalence analysis and the extensionist theory.

The relevance of the anthropological or ethno-linguistic analyses for linguistics may seem clear. Their position vis-à-vis linguistics, however, is less clear. R.W. Langacker, for example, agrees - as we shall see shortly - that equivalence rules of some kind are necessary but he considers them to be extralinguistic conventions (1969: 848). However, I would first like to present another attempt at bridging the gap between anthropological and generative approaches made by Paul Kay (Kay, 1974). Kay explicitly sides with the generative semanticists: he does not think that sets of features are powerful enough to account for the structure of the meanings of English kinship terms (loc.cit.: 120).

An analysis which would capture what is known about English kinship semantics from existing analyses and bridge the gap between anthropological and generative approaches will according to Kay have to satisfy four conditions. First it must have the capacity to generate an indefinitely large number of semantic representations, it must have the recursive capacity which, for example, componential analysis misses. Next it must make explicit all instances of co-variation between sound and meaning, i.e. it must specify the meaning of the composing parts of compound kin terms and show the meaning of the compounds to be composed of the meaning of the parts. Furthermore an analysis should account for the minor variations between

speakers and even between varying usages of a single speaker by showing how meaning may be extended, or not extended, in varying ways. Finally an analysis must provide an account of the relations of paraphrase, entailment, contradiction, sequitur, etc., that obtain between the sentences which contain kin terms (cf. loc. cit.: 121).

Conceptually English kinship has two primitive terms, "offspring of" and "spouse of", plus the predicates "male" and "female". The notion of parenthood which is presupposed by this and is used in genealogical reckoning is not a simple but a complex notion, contained in what he calls the "Rule of true and legitimate parenthood" (loc. cit.: 123 f.). This rule expresses the fact that our normal genealogical reckoning is based on the assumption that all parents are true and legitimate parents except when preceded by the rule suspenders "step-" and "half-". It is Kay's way of restating what many scholars before him have said, viz. that kinship reckoning is basically concerned with normal genealogical relations, with biology rather than with sociology so to speak. The notation Kay uses was developed by John Atkins. Any blood relationship is analysed as a sequence of "child-of" relations (Q) followed by a sequence of "parent-of" relations (P) and is established by counting the ascending and descending steps to and from the common ancestor (cf. loc. cit.: 124). The general form for any consanguineal relation is $Q^i P^j$. A restriction \leq must be added to rule out the form $Q^1 P^0$. The notation is not a relative product: it may not be so to exclude the possibility of doubling back to an individual. E.g. $Q^1 P^1$

must be sibling and cannot be self.

Kay's analysis proceeds in three steps: basic predicates, immediate derivatives, and predicates derived via functors (loc.cit.: 125 ff.).

Basic Predicates

Class	Notational expression	Primitive Predicate of English
O	$Q^0 P^0$	SELF
I	$Q^0 P^1$	PARENT-CHILD
II	$Q^1 P^1$	SIBLING
III	$Q^1 P^2$	NUNCLE-NIBLING ¹
IV	$Q^2 P^2$	COUSIN

Immediate derivatives

Class I	$P(\text{ARENT}) (x,y) = \text{PARENT-CHILD} (x,y)$ $\text{CHILD} (y,x)$ just if $P (x,y)$ $\text{MOTHER} (x,y)$ just if $P (x,y)$ and $\text{FEMALE} (x)$ etc. for FATHER , SON , and DAUGHTER .
Class II	$\text{BROTHER} (x,y)$ just if $\text{SIBLING} (x,y)$ and $\text{MALE} (x)$ $\text{SISTER} (x,y)$ just if $\text{SIBLING} (x,y)$ and $\text{FEMALE}(x)$
Class III	$\text{NUNCLE} (x,y) = \text{NUNCLE-NIBLING} (x,y)$ $\text{NIBLING} (y,x)$ just if $\text{NUNCLE} (x,y)$ $\text{AUNT} (x,y)$ just if $\text{NUNCLE} (x,y)$ and $\text{FEMALE} (x)$ etc. for UNCLE , NIECE , and NEPHEW .

Predicates derived via functors.

Rule 1:	$\text{GREAT} (Q^i P^j)$ just if $Q^i P^{j+1}$	$i \leq 1$
	or phrased differently:	
	$X - Q^i P^j - Y$ $X - \text{GREAT} - Q^i P^{j-1} - Y$	$i \leq 1$ $j > i+1$

-
1. NUNCLE is the cover term for uncle and aunt, NIBLING for nephew and niece.

As long as $j-1$ is still greater than $i+1$ the rule is reapplied, yielding $\text{GREAT} - \text{GREAT} - Q^1 P^{j-2}$, etc., till the exponent on P equals $i+1$. Then the rule is blocked.

Similar rules deal successively with STEP- , HALF- , IN-LAW , BY-MARRIAGE , -TH , and TIMES REMOVED .

These rules account for the underlying semantic facts regardless of the variation in surface lexical items as used by different speakers. It is by these rules that the first three conditions laid down by Kay at the beginning of his article are met while the kind of notation adopted automatically achieves an explicit statement of relations of paraphrase and the like, his fourth condition. As he admits, however (*loc.cit.*: 130), a very important thing remains to be done, in fact an essential point I would have thought if one really wants to claim to have bridged the gap between anthropologists (or linguists with an ethnological interest for that matter) and transformationalists in their approaches to kinship terminology. One will have to show how to connect these semantic representations directly to what we know of the structure of English sentences containing kinship expressions, be it via projection rules, meaning postulates, equivalence rules, or ordinary transformational rules. However, because the semantic section of transformational grammar is at present in a state of uncertainty and upheaval, any proposal for a solution to a particular question is almost bound to become an issue in the methodological debate. In those circumstances it may take quite some time before this final condition is met to everybody's satisfaction.

Ronald W. Langacker's interest in English kinship terms is linked with his examination of lexical evidence for the mirror image convention (Langacker, 1969). Though his concern was explicitly with a type of analysis which formed part of a transformational grammar, he recognized that his rules and the feature definitions of previous approaches both intended to capture the same generalizations and that, consequently, the two types of approach were not as different as they might appear at first sight (loc.cit.: 849). He was critical of componential analysis on some points, though, first because it took the molecular rather than the atomic approach, a point which we shall come to shortly; secondly because the features it used were perhaps not unanalysable and because it did not show that some kinship terms are more closely related than others. This last weakness is really a consequence of the minimal-meaning approach of componential analysis (loc.cit.: 850 f.; cf. also above 4.3.1). Langacker holds that semantic representations should not be conceived of as bundles of semantic features but rather as structures with sub-parts ordered and connected in a specific way (loc.cit.: 845). His views on the nature of the lexicon of a language place him squarely in the camp of the generative semanticists as he states explicitly:

"Lexical items are then conceived as transformational rules that associate phonological matrices (together with grammatical and other information) with sub-trees of the input semantic structure" (loc.cit.: 847).

Assuming that the basic semantic elements are PARENT, CHILD, SIBLING, SPOUSE, MALE, and FEMALE one can think of two possible ways of representing those structures.

One can set up another basic element, 'POSS' (possessive), that links the elementary semantic units in an ordered structure; or alternatively one can treat the elements PARENT, SIBLING, etc., not as noun-like entities but as relations on the semantic level (cf. loc.cit.: 845 f.). E.g. "Bill's cousin" would be either:

(1) Bill 'POSS' PARENT 'POSS' SIBLING 'POSS' CHILD
or: (2) w such that w CHILD x; x SIBLING y; y PARENT z;
and z = Bill.

Both structures are ordered structures, both use the same semantic units, both have the same element as the 'head' of the structure, viz. CHILD.

In the framework of generative semantics one can then expect lexical transformations of the kind which follows (loc.cit.: 747):

(T 1) X, PARENT + 'POSS' + SIBLING + 'POSS' + CHILD, Y \Rightarrow 1, cousin, 3
1 2 3

(T 2) X, $\begin{bmatrix} \text{MALE} \\ \text{PARENT} \end{bmatrix}$, Y \Rightarrow 1, father, 3
1 2 3

Langacker also discusses the question of equivalence rules and extensions (loc.cit.: 847 f.). He admits that these equivalence rules are absolutely necessary in order to account for certain types of kin classification. He is less certain, however, about the precise status of these rules. He doubts whether they can be interpreted as syntactic transformations and - at least for the moment - prefers to look upon them as "extra-linguistic conventions that define the classes of kin relations considered equivalent in a culture". I hope to show below (cf. especially 6.1.3 and 6.2.3) how equivalence in kin type classification

can in fact be given a linguistic interpretation by linking it to the deictic nature of this part of language.

The extensions by means of compounding of nouns make Langacker opt for atomic rather than molecular lexical insertion rules, if only because the compounding process is in principle unlimited in languages like English. This is why componential analysis was criticised on this point: pushed to its logical conclusion the molecular approach is impossible in principle and it does not capture possible generalizations which transformational rules do capture.

A particularly powerful way of explicating generalizations is the mirror image convention which

"allows two rules to be collapsed into a single rule when they are mirror images of one another - i.e., when the terms of their structure indices and output sequences are identical, but occur in exactly reverse order" (loc.cit.: 844).

The mirror image rules will be marked here with an asterisk.

The clearest example of a mirror image rule in English kinship terminology is the rule which inserts -in-law (loc.cit.: 851 ff.). In order to be able to deal with this instance we must first return to our four basic semantic elements, PARENT, CHILD, SIBLING, and SPOUSE. Of these four PARENT and CHILD are more similar to one another than to either of the other two and are clearly connected in some way. If we define PARENT as [+ lineal, + parent] and CHILD as [+ lineal, - parent] we can indicate this similarity to one another and difference from both the other elements by [+ lineal]. This will be the feature we shall use in the formulation of the rules whenever we want to specify PARENT or CHILD as opposed to all other kin relations.

The term brother-in-law is ambiguous: it can either be a spouse's male sibling or a sister's male spouse, with their respective underlying structures:

(3) SPOUSE 'POSS' $\left[\begin{array}{c} \text{MALE} \\ \text{SIBLING} \end{array} \right]$

(4) SIBLING 'POSS' $\left[\begin{array}{c} \text{MALE} \\ \text{SPOUSE} \end{array} \right]$

Other possible meanings of brother-in-law (WiSiHu, HuSiHu, BrWiHu, etc.) are disregarded for the moment but it would be possible to integrate them into these rules. The source of brother in the underlying structures is given in the underlined capitals: the placing of the feature MALE does not cause a problem as in English the surface head noun of a kinship expression always inherits the gender of the rightmost element of the underlying semantic structure. The non-underlined portions must, as a consequence, be the source of -in-law.

The following transformation rules will insert -in-law into (3) and (4):

(T 3) X, SPOUSE, 'POSS', SIBLING, Y \Rightarrow 1, \emptyset , \emptyset , 4-in-law, 5
 1 2 3 4 5

(T 4) Y, SIBLING, 'POSS', SPOUSE, X \Rightarrow 5, 4-in-law, \emptyset , \emptyset , 1
 5 4 3 2 1

These two T-rules can be collapsed by mirror image convention:

(T 5)*X, SPOUSE, 'POSS', SIBLING, Y \Rightarrow 1, \emptyset , \emptyset , 4-in-law, 5
 1 2 3 4 5

Disregarding the features [+ parent] and [- parent] and just using the feature [+ lineal] as explained above, we can draw up a similar mirror image rule deriving father-in-law and son-in-law:

(T 6)*X, SPOUSE, 'POSS', [+lineal], $Y \Rightarrow 1, \emptyset, \emptyset, 4\text{-in-law}, 5$

1 2 3 4 5

Finally we can collapse (T 5) and (T 6):

(T 7)*X, SPOUSE, 'POSS', $\left\{ \begin{array}{l} \text{SIBLING} \\ [+lineal] \end{array} \right\}, Y \Rightarrow 1, \emptyset, \emptyset, 4\text{-in-law}, 5$

1 2 3 4 5

In a similar way one could analyse the special relationship which is felt intuitively to exist between uncle, aunt, nephew, and niece, a relationship which Romney and D'Andrade characterized by the feature [collateral] (cf. Langacker, 1969: 853 ff.). A simple way of showing this relationship is by drawing up lexical transformation rules for these four terms: all four rules will use the elements [+ parent], 'POSS' SIBLING, and MALE/FEMALE in different order. Here too it is possible to propose a mirror image rule. 'POSS' and SIBLING are the common elements which set the group of four apart from all others. To distinguish the four from one another one can write a mirror image rule which will mark the particular underlying structure by a new element to be introduced, viz. [+ oblique]. Together with [+lineal] and MALE/FEMALE this rule will derive the four terms, e.g.

(5) SIBLING 'POSS' $\left[\begin{array}{c} \text{FEMALE} \\ \text{CHILD} \end{array} \right] \Rightarrow$ (T rule "oblique")

$\left[\begin{array}{c} \text{FEMALE} \\ + \text{ oblique} \\ \text{CHILD} \end{array} \right] \Rightarrow \underline{\text{niece}}$

Another instance of mirror image would be the rules of grand- insertion. One simple transformation rule will account for the derivation of both grandfather and grandson, by using the alpha notation in [α parent]. Two

more rules will give granduncle and grandnephew. Using again the feature [oblique] and the alpha notation these two rules can be collapsed into a mirror image rule, and eventually there is a way of collapsing this mirror image rule with the earlier rule for grandfather/grandson.

There is no doubt whatsoever that allowing transformation rules to apply to lexical elements results in a clear insight into certain aspects of this part of the vocabulary of English. This type of rule is different from the equivalence rules as proposed by Lounsbury: yet, I think it is going too far to call Lounsbury's rules extra- or pre-linguistic. It is true that Lounsbury's position has much to do with what is an anthropological debate about the genealogical or social basis of kinship reckoning. Yet, his rules also deal - and deal adequately - with linguistic elements. Certainly, his rules are not an integral part of a linguistic theory in the way and to the extent that Langacker's rules are part of his brand of transformational grammar. The significance of this difference, however, is greatly diminished by the uncertainty about the shape transformational grammar might take: at present it does not seem possible to predict whether transformational grammar will develop more along the lines of the Extended Standard Theory or Generative Semantics and for that matter whether transformational grammar will be the dominant theory.

As for Lounsbury's anthropological interest I can only say that I, too, believe that anthropological factors are important for the understanding of kinship terminology. To be aware of the anthropological implications and to make

one's position in this respect known and explicit is a positive value whatever option one takes. Kay did make his option clear: a genealogical point of view starting from the nuclear family. Seuren and Langacker unfortunately did not make clear in an explicit way where they stand in this matter though, as we saw, they did make some fairly important observations - on homonymy and equivalence rules respectively - which would have gained by showing an awareness of the related anthropological problems and implications and by making clear which position they take on these matters.

5. Evaluation: Criticisms, Problems, and Questions.

5.1. Introductory Remarks.

C. Hockett once remarked: "Ethnography without linguistics is blind. Linguistics without ethnography is sterile" (cf. Hoijer, ed., 1954: 225). This remark, made in a discussion about Whorf's theory of linguistic relativity, was meant to express an intuitively felt conviction that the relativity hypothesis was worth pursuing notwithstanding all the theoretical problems as well as the practical dangers and pitfalls. As a matter of fact this idea was explored long before Whorf, e.g. by J. de Josselin de Jong (1913, e.g. page 212) in a study which he entitled "ethno-psychological". He later adopted the term ethnolinguistics which he defined as follows:

"De studie van de functionele samenhang
- zowel synchronisch als diachronisch -
van de linguistische en niet-linguistische
aspecten der cultuur" (de Josselin de Jong,
1951: 161),

'the study of the functional connection - both synchronic and diachronic - between the linguistic and non-linguistic aspects of culture'. De Josselin de Jong's 1951 article is still tentative after a life-time of high quality scholarly work. When one looks back over more than a century of research into kinship and kinship terminology, of ethnolinguistic research, any conclusion seems bound to be equally tentative.

Some authors take a very gloomy view of what has been achieved. R. Keesing, for example, said at a special occasion marking the Morgan Centennial Year:

"Had W.H.R. Rivers addressed a similar gathering fifty years ago, he could have argued convincingly that Morgan's vision of kinship terminology as an anthropological Rosetta Stone was on the verge of fulfilment. Yet after a second half century of concerted effort, we apparently have not yet learned how a people's kinship terminology enciphers the structure of their social universe. The code is not yet broken, the mapping of the social order not yet understood" (Keesing, 1972: 17).

This opinion is an intentional exaggeration towards the pessimistic side. It must be quite clear, however, from all that precedes that it is not possible to select one solution as the definitive one, as totally satisfactory in every respect. I have already quoted Kronenfeld's opinion (cf. above end of 4.4.3) that the facts seem to force us into accepting that alternative forms of analysis may be directed towards different goals: a different goal may well require a somewhat different analysis. Jean Jackson seems to take a similar view in her analysis of Bará kinship terminology (1977). She examines this terminology by means of three different approaches to kinship semantics, the genealogical approach, the alliance theory approach, and the sociogeographical approach which is primarily concerned with the local distinction between "own group" and "other group":

"These analytic approaches are a first attempt at specifying three postulated semantic domains in the Bará language. Although the approaches and the domains they represent are analytically contrasted with one another in this paper, I do not feel that one of them must eventually be chosen as absolutely correct" (loc.cit.: 83).

Scheffler takes a different view (1972: 323 ff.). He argues that there is such a variety of alternative models that one can be excused for wondering whether to take any

of it seriously. There is, however, in his opinion a reason for this:

"It has not often been made clear why a particular method has been chosen, or why it should be chosen, or how it relates to a conception of the nature of the object under analysis. In other words, too many studies of kinship semantics have been based on ad hoc, unsystematic, and largely implicit semantic theory or - what is worse - on no clearly semantic conceptions at all" (loc.cit.: 324).

He goes on to argue that if one accepts that his basic assumptions about polysemy, primary meaning, and extension of meaning are correct, it becomes possible to judge the relative validity of the different methods.

I would agree with Scheffler about the importance of one's basic assumptions. This is, as a matter of fact, exactly what I have been trying to do: to unravel not only the techniques developed for analysing kinship systems but also the background to each particular form of analysis. As Scheffler points out, and as I have shown in detail, the semantic theory was mostly implicit in some cases or simply deficient in others. My aim has been to let each method speak for itself since I believe that the questions a given form of analysis tries to answer are of equal if not greater interest than the solutions proposed.

After having studied the main representatives of the study of kinship and kinship terminology since Morgan I have come to the conclusion that particular answers are arrived at because of the kind of questions which were being asked; and on the whole these answers are valid and reasonably good answers to those questions even though I may have been able, with the benefit of hindsight,

to discover certain omissions, inconsistencies, or weaknesses. I have, therefore, no difficulty in agreeing with Kronenfeld and Jackson that there probably are no absolutely correct answers. There are correct answers to specific questions. Scheffler spoke about the relative validity of the different methods in the light of the criteria of the extensionist theory; I would rather speak of the relative validity of all solutions.

I would now like to examine a bit more closely some of the fundamental questions connected with the study of kinship terminology and try to establish some of the remaining areas of uncertainty. This may lead us eventually to consider kinship terminology in a different way, to restate the question of the relation between language and culture, exemplified by kinship terminology, in a new way.

5.2. Some Problems and Questions arising from the history of the study of kinship terminology.

We are assuming that the theories discussed in Chapters 2, 3, and 4 have, on the whole, proposed valid answers to questions asked in a particular way about the connection between language and culture. Since the questions were determined by a specific interest the answers were bound to be partial, and at times they were not quite perfect even within their self-imposed limitations. If one were to judge the achievements of one century of kinship studies in absolute terms the verdict could be rather negative. No single unequivocal answer has come out of it, indeed there is not even agreement on whether

one should expect a causal connection between kinship terminology and social structure (and if so which would be cause, and which effect), or whether one should be content to establish co-variation. Some theories are so wide-ranging or so abstract that there is little hope of ever testing and proving them in a comprehensive way. Other approaches have strictly limited objectives; at times, however, their preoccupation with exact detail becomes so great that the wider view, necessary for an integrated language and culture study, is no longer considered. It will be clear that the first group of theories forms part of the work done in a perspective of causality, be it historical or structural-functional, while the second group consists mainly of types of formal analyses.

5.2.1. Causal historical explanations.

Scheffler (1972: 312 f.) considered one of the major weaknesses of the historical approach to be an implicit assumption of monosemy. Though he does not give much attention in his review of kinship semantics to the historical approach and his choice of the word 'monosemy' is clearly inspired by his personal view on the importance of polysemy in the study of kinship terminology, there is an element of truth in this. All too often scholars were led by the assumption "one label - one thing", the two being isomorphic at least at the outset.

When we are talking about evolution we must be very careful in establishing a connection between terms and social categories or concepts. One may well be justified

in seeing social development in terms of a hypothesis of a very simple beginning with a gradual increase in complexity and diversity.

There are at least some indications in the historical facts at our disposal that development of social life does imply a growth in complexity. The distinction simple vs. complex society may therefore be acceptable in social science. As far as language is concerned we have no indication whatsoever for degrees of structural complexity so that a simple parallel development becomes hard to imagine let alone to reconstruct. Indeed, when we mention the word "reconstruction" it must be remembered that reconstructed forms do not represent a previously existing reality: they are merely a theoretical statement of an alleged relationship between historically attested forms. This is why evolutionary solutions are unacceptable even without taking into account unacceptable prejudices about e.g. original promiscuity (cf. above 2.1.1 and 2).

Even if one does not pretend to go right back to the origins but simply wants to explain the structure of a terminology in relation to the social structure at a given moment in history it is not always easy to establish the presence of the hypothesised correlation. It is in connection with this difficulty that we find the explanation of cultural lag:

"A change in social structure which significantly alters the social equalizers and differentials affecting particular kin-types will be followed by adaptive changes in the pertinent kinship terms only after a lapse of time" (Murdock, 1949: 137).

Paul Friedrich (1967) has made a study of the change in Russian kinship and kin terminology from the Tsarist to the Soviet period. His assumption is that the change in social system "precedes and predetermines change in the corresponding semantic systems" (loc.cit.: 31). The fact that there is no perfect fit at a given moment can be explained as having three causes: the inadequacy of the symbolic resources to express all nuances; the fact that language forces us to overrepresent or underdifferentiate reality; and finally the fact that "the linguistic system always lags behind the evolving natural and cultural environments to which it is meant to relate" (ibid.: 32). He goes on to say that "fundamental changes in any basic social institution should be reflected within a generation or two by correspondingly fundamental changes in the semantic system". An example of such a change would be the disappearance of the dimension of the sex of the linking relative with regard to collateral and affinal kinsmen in the Russian kinship system, due to the diminishing importance of the extended family in favour of the nuclear family. This trend is well attested in Western Europe as a whole: the four Latin terms avunculus, patruus, amita, and matertera are now reduced to only two in the vocabulary of many European languages, e.g. English uncle and aunt.

I do not want to challenge these facts, on the contrary, I think they are important. But it is not always the case that changes in the social system are followed by this kind of changes in the kinship nomenclature. E.Dozier has discussed an interesting case which illustrates this

point, the case of the Arizona Tewa (Dozier, 1955). They used to have a bilateral, generational kinship system which the related Tewa from New Mexico still have. The Arizona Tewa, however, moved away from New Mexico around the year 1696 to Arizona where they settled down and developed, under the influence of their new neighbours, the Hopi Indians, a matrilineal structure: their kinship terms which were formerly adjusted to a bilateral system were now made to fit into a lineal system and changed in meaning as a result.¹ We do know in this case how to explain the discrepancy between social system and kin terminology: my point is that if the one can change without the other, there can be no certainty about a necessary causal link existing between social system and kin terms. Our certainty does not go beyond the observed co-variation in specific, documented cases.

Another reason for being extremely prudent is the uncertainty about the duration of this lag. Friedrich speaks of "a generation or two". Two comments come to mind here: is there ever any real inadequacy experienced by the people in their use of kin terms, and secondly, is either the social or the terminological system ever completely stable and is it therefore realistic to imagine the process as a development in leaps with the terminology following the social changes and catching up? As far as I can see the answer is negative on both counts. I do not want to deny the value of historical explanations for

1. e.g., ko'o: New Mexico Tewa: mother's or father's younger sister. Arizona Tewa: mother's younger sister (loc.cit.: 252).

precise, limited questions but I doubt if it is meaningful or wise to ask a historical question in order to try and find a global solution.

There is another example which illustrates my point about limited questions. It concerns single factor explanations, i.e. explanations of particular ways of classifying relatives by reference to one single social practice or fact. This type of explanation is found in structural-functional accounts as well: my reflections concerning their use in a historical approach would be equally valid for those. An example would, for example, be Sapir's use of the levirate as the factor which explains the terminological equation of step-father with uncle, step-mother with aunt, and step-child with nephew or niece in certain American Indian languages (Sapir, 1916; cf. above 2.4). These explanations, though interesting as hypotheses, are very limited since they concern themselves with a few terms only. One is not really discussing the social relevance of a given kinship terminology. Even if one disregards the ethnocentric prejudice which sees a need for explanation only for those terms which are unusual by our standards, and if one assumes that some parts of kinship terminologies are fairly similar across many cultures, it would still require some sort of explanation in what way and to what extent kinship terminology, and not merely certain kinship terms, can be said to be socially relevant.

5.2.2. Causal structural-functional explanations.

Many of the theories we discussed in Chapter 3

were primarily anthropological theories. Their chief interest was culture, culture in the sense of 'the man-made part of the environment' as opposed to 'nature'. Language was considered to be a part of culture. When anthropologists discovered exotic systems of social organization and faced the problem of trying to understand their structure they would look at the whole of a given culture to try and gather whatever information might be helpful to them in this analysis. Language - and in the case of social structure, kinship terminology - was for them a valuable tool to help discover at least some of the basic principles which were supposed to govern the native ways of classification. Since, therefore, their interest was mainly directed towards social organization one would expect that the social structure would, more often than not, be seen as the cause and a given type of terminology as the effect, the result. Murdock, for example, wrote about various forms of social structure that they "tend to produce different types of kinship terminology" (1949: 180). When one takes into account the anthropologists' rather broad view of culture it is understandable that they should want to speak of "types", i.e. fairly broad generalizations which are not necessarily very specific as to details. As, moreover, language was studied as a tool one would not expect anthropologists to maintain their interest in it when conflicting detail would make it more of a troublesome burden than a useful tool. Anyhow, exceptions are accepted as inevitable in cross-cultural studies. Köbben made this point very strongly in a major study on this question: when there is

a rule in anthropology there are bound to be exceptions (Köbben, 1967: 4).

All this does not mean that no claims are ever made for a perfect correlation between social structure and kinship terminology. In a fairly recent article about the kinship system of the Piramalai Kallars, from the Madurai district in Tamil Nadu in India, we read:

"The kinship terminology is a salient aspect of the kinship system of the Piramalai Kallars. The study of the kinship terminology itself will sufficiently throw light on how the kinship system is structured in the Piramalai Kallar community. Kinship terminology is so structured that it is in perfect harmony with the kinship rules and customs. As a matter of fact, it is the kinship rules and customs which determine the terminology" (Natarajan, 1976: 147).

Without going into detail, the Piramalai Kallar kinship system is basically characterized by the dichotomization of the relatives into parallel and cross kin (cf. loc.cit.: 151); this principle works its way through all aspects of that society including its kinship terminology. Three generations are affected, as the following tables show:

Generation	Parallel kin		Cross kin	
	m	f	m	f
Same generation as Ego	annan tampī	akka: tankacci	macca:n maccunān	matini koluṇṭiya:l
Senior generation	appa: (periya-/-nalla-)	a:tta: (periya-/-nalla-)	ma:man	aṭṭai
Junior generation	makan	makal	marumakan	marumakal

In general category terms this means:

Generation	Parallel kin category	Cross kin category
Same generation	Siblings	Siblings-in-law
Senior generation	Parents	Parents-in-law
Junior generation	Children	Children-in-law

The kin-types of the various terms are as follows¹:

annan	eB, FBS(e), MZS(e)
tampi	yB, FBS(y), MZS(y)
akka:	eZ, FBD(e), MZD(e)
takacci	yZ, FBD(y), MZD(y)
macca:n	MBS(e), FZS(e), eZH, WeB
maccunan	MBS(y), FZS(y), yZH, WyB
matini	MBD(e), FZD(e), eBW, WeZ
koluntiya:l	MBD(y), FZD(y), yBW, WyZ
appa:	F, FB, MZH
(periya-/-nalla-)	e/y (obligatorily used with "appa:")
a:tta:	M, MZ, FBW
(periya-/-nalla-)	e/y (obligatorily used with "a:tta:")
ma:man	MB, FZH, (H/W)F
attai	FZ, MBW, (H/W)M
makan	S, BS, WZS
makal	D, BD, WZD
marumakan	ZS, (MBS/FZS)DH, WBS
marumakal	ZD, (MBS/FZS)SW, WBD

(cf. Natarajan, 1976: 148, 153).

1. In this kin-type notation Z stands for sister, S for son, e for elder and y for younger.

Assuming that we have here an example of perfect correlation, the fact still remains that imperfect correlations do occur. Whenever these are found to exist different explanations are given for this lack of harmony. As we saw before, the traditional explanation was to put it down to social change; this kind of solution is still proposed occasionally. For example, when Kay (1974: 123) discussed his basic assumption of true and legitimate parenthood (cf. 4.4.4 above) he noticed deviations from this principle and contradictions, the indeterminacy of the data to which Schneider has drawn our attention, and he remarked:

"It should be recognized that such indeterminacy resides not in the language or culture per se, which is internally consistent, but from the fact (sic) that we have to make do with this language-culture in a world of social arrangements for which it was never made".

Another explanation is based on the non-necessary character of the correlation between terminology and social system, first hinted at by Lévi-Strauss and developed further by R. Needham and R. McKinley. As we have seen (cf. above 3.5.2) Lévi-Strauss distinguished "système des appellations" and "système des attitudes" (1958: 45 f.): the two systems are said to be interdependent in particular by providing the possibility of overcoming problems and tensions which may exist in one system with the help of the other one, not necessarily in the sense that one reflects the other.

R. Needham (1966/1967), too, has argued that we really have two objects of study: categories, and social

groups and action. Both can be analysed independently, so that there is no need to regard either as peripheral to the other. Ideally one would probably want to subsume both analyses under one formulation. As things are it would seem to be a matter of temperament whether one thinks that the more fundamental insight can be expected from the study of social organization or from that of the organization of categories. But Needham's empirical tests show clearly that a given type of relationship terminology is not necessarily correlated with any type of social grouping.

It may be worthwhile to continue a line of thought of Needham's - though not necessarily in the direction he would have pursued it. Given the limited success of attempts to establish a systematic correlation between kinship terms and social structure, would one not be justified, indeed well advised, to approach the matter from another angle and ask: how do people actually integrate the two? Robert McKinley (1971 a) has been thinking along similar lines. His starting point was the Crow/Omaha terminology systems. These systems are characterized by three basic features: cross-cousins are distinct from siblings and parallel-cousins; patrilineal cross-cousins are distinguished from matrilineal cross-cousins; and - most typically - the cross-cousins on one side are classed with relatives of an ascending generation while those on the other side are classed with relatives of a descending generation. Allegedly the general assumption has usually been reflectionist, i.e. an assumption that terminologies 'reflect' specific attributes of social organization; the

terminologies were considered to be purely passive in these cases. In the Crow/Omaha cases the specific attribute in question was taken to be the presence of unilineal descent: several generations of unilineally linked relatives are included under a single term. While this seems a reasonable explanation one is faced with difficulties when inverting the pattern of investigation and starting from the presence of a unilineal descent system. It appears that in the majority of the societies with such a system we do not find a Crow or Omaha type of kin terminology. There is, therefore, no necessity for a given type of terminology to be the result of a specific form of social organization. The reflectionist assumption has clearly taken too simple a view of what are very complex and subtle relationships of interaction between terminology and social structure. One question which is never asked would be what the significance of non-reflection might be in a given case. It is obvious, then, that the whole problem is far from solved. The answers given so far are too easy, too mechanistic, definitely incomplete and perhaps slightly superficial. The fact remains, however, that kinship nomenclatures are embedded in social life: that is sufficient reason for continuing to examine in what way kin terminological usage may be said to be socially determined or relevant.

Another difficult case which has been reported by Maurice Bloch concerns kinship terms on Madagascar (Bloch, 1971). The kinship terminology is practically uniform throughout the island, but the social organization and the kinship systems which use these terms vary to an

extreme degree: this would seem to deny all connection between terminology and social organization. Lévi-Strauss's and Needham's proposals for solving such difficulties involved a distinction between two systems, a system of behaviour and a system of (terminological) categories. Bloch proposes a different distinction. He speaks of two ways of considering kinship terms, viz. from the point of view of the place they hold in the system of values, and from the point of view of which tactical uses they can serve. This would clearly take the study of meaning of kin terms beyond the 'denotation of genealogical positions': it would involve strategic use as well as denotation of already existing roles, and kinship terms, when considered as value-judgements, may well contain elements which need a much wider framework than merely the realm of kinship in order to be defined properly (cf. below 5.2.3.3). There is no doubt that this proposal is at variance with Lounsbury's hypothesis concerning the extension of meaning: I shall return to this aspect later (cf. below 5.2.3). At this moment I would like to draw attention to the possibility of solving an apparent contradiction between linguistic and social facts by asking a different question. Instead of asking how to correlate kin terms and social structure, one tries to establish what the moral scheme of a particular language is, its value-judgements. One then places terms into this scheme and finally considers their use as strategies.

There is another factor I must discuss in this section. As a rule descriptions of kinship systems tend to display a homogeneous set of kinship terms, though

Schneider's reference to alternative usages is one of the exceptions. I am thinking less, however, of variation in individual usage than of regional variations of which a few examples have been recorded. One such case has been studied by Stanley and Ruth Freed (1970), viz. the regional variation in Navajo kinship terminology. The Navajo Indians use Iroquois cousin terminology, i.e. they extend sibling terms to parallel cousins but have separate terms for cross-cousins, the same ones for cross-cousins from either side (bilateral). In some localities, however, this terminology has taken on features of a unilineal type, viz. the matrilineal Crow-type with its shift across generation boundaries. Though more research is required, the first indications are that the explanation for this variation is to be found in the influence of the nearby matrilineal Hopi who - as we have seen - completely changed the social system of the Tewa who settled in their vicinity. This case confirms our earlier observations about the difficulty of establishing a firm link between kin terms and social structure. If accidental circumstances can destroy similarity of structure between the two, there is no guarantee that in other cases the presence of similarity could not equally be accidental. With so little research done on regional variation (or even individual variation, especially in simple societies), it is difficult to assess the importance of this factor.

At this point I would like to consider what, in my opinion, is one of the most awkward but also one of the most challenging facts concerning the hypothesis of

the social relevance of systems of kin terminology, viz. the question whether this hypothesis can be stated with any confidence with regard to the terminologies of our complex, industrialized societies. From a linguistic point of view one could argue that there is no parallel in the field of languages to the simple or primitive vs. complex or developed distinction which social scientists make. Kinship terminology as a linguistic phenomenon is, therefore, an object of study in its own right whatever language one selects. If, however, one takes an ethno-linguistic point of view and is interested consequently in the area where language and society meet and interact, it becomes more difficult to keep linguistics and social science strictly separated. As a result the ethnolinguistic study of kin terms has to deal with more than strictly linguistic differences. These differences, however, cannot be all that great. After all, anthropology is trying to understand man, not merely man in a certain type of society. Lévi-Strauss asked the following question in this connection:

"Should we try to include modern societies in our sphere of investigation, and may we do so by applying the same conceptual framework which has proved so valuable for the study of simpler societies?"

His answer is unequivocal:

"It is useless to attempt to find out what kinship really is before we succeed in ascertaining whether its field of operation covers all types of human society, and in this case to what extent, and in which way its modes of operation change as it passes from simple societies to complex ones" (Lévi-Strauss, 1965: 18).

This is not easy, however. Haudricourt and Granai, in their critical discussion of Lévi-Strauss's early work,

mentioned several reasons why, in their opinion, there is a considerable difference. Their remarks are valuable even if one would like to take a more hopeful view of a possible solution:

"L'ethnologue travaille traditionnellement sur des sociétés différentes de la sienne propre; la langue est, pour lui, un aspect, difficilement dissociable, de la société envisagée, - et un aspect privilégié puisqu'il lui donne accès aux autres phénomènes sociaux. Le sociologue, au contraire, qui traditionnellement analyse sa propre société tend à oublier le caractère social de la langue qu'il emploie spontanément. D'autre part, les sociétés auxquelles l'ethnologue s'intéresse (sociétés archaïques) sont généralement des sociétés globales très limitées (ou qui paraissent telles) qui coïncident bien souvent en extension avec la langue. En d'autres termes, système de la langue et système de la société apparaissent comme homogènes et semblent se correspondre 'membre à membre'. ... Enfin ... l'importance des structures et des comportements axiologiques dans les sociétés traditionnelles ... conduit aisément l'observateur à l'idée de la société comme système symbolique de comportements qui a son homologue dans le système de la langue" (Haudricourt et Granai, 1955: 128).

The authors believe that it is this special relationship between the anthropologist and his object of study, and the conditions of his work, not the fact that language and society are truly co-extensive and homologous, which lie at the root of theories of the kind advocated by Levi-Strauss.

It certainly is the case that the analysis of the kinship terminology of a complex society seems much more convincing and satisfactory when there is a point of comparison, either diachronic or synchronic. Weisgerber took the view that comparison is a necessary condition for an analysis which is to reveal "das sprachliche Weltbild"

(1964: 97, cf. above 3.6.3). But when we try to assess what all the various forms of analysis of English kin terms have shown us about the corresponding social structure we have cause to feel disappointed. To a large extent psychological interest gradually took the place of the social orientation of most structuralists and functionalists (cf. 4.3.1). Parsons gave some indications as to the way in which American English kinship terminology could be said to reflect the actual institutional structure of kinship (1943: 27); as I have noted above (cf. 4.2), his list of characteristics is entirely negative and derived from comparison with structures prevalent among non-literate peoples. It is to be expected that the relative importance and the degree of integration of the elements which are common to all cultures (e.g. economics, religion, language, social organization) vary considerably: but the very idea of culture supposes some form of integration of those elements and, though it may be difficult to analyse, there is no reason why a positive outcome to a search for the nature of this integration would be impossible. The only thing which could frustrate such efforts would be a deep crisis within a given culture which would destroy - at least temporarily - the proper integration of the various constituent elements. Whether or not our society is passing through such a crisis is in itself an interesting question but one beyond the scope of this study. Even if there were some measure of a crisis, our society does not seem to be affected to the point of total disintegration of the fabric of its social structure.

We can, however, observe clearly certain tensions connected with change. P. Willmott has given an interesting example of this in an article which examined how the changes in social legislation in Britain after World War II compared with people's intuitive feelings about kinship (Willmott, 1958). Statutory liability is imposed by the National Assistance Act 1948, only upon husband and wife for each other and for their children under the age of sixteen. This Act reduced the range of responsibility for one's relatives which had been much wider under the three centuries old Poor Law; it was seen simply as an official and belated legal recognition of a sociological change which had started with the industrial revolution. One might think that, having released people from the duty of maintaining their impoverished relatives, the State would now be no longer under any obligation to assist those who still are in fact supporting their relatives. Social legislation, however, still takes some account of the kinship obligations that are voluntarily accepted; the State now recognizes two degrees of kinship obligations - those which are legally enforced and others, towards a wider range of relatives, which may be voluntarily undertaken. The acceptance by the State of these two degrees must be attributed to the strength of feeling among the public to continue considering relatives as different from other people; the State gave way to this pressure even though the distinction did not always fit in easily into the original concept of the 1948 Act. One can then take a list of terms for relatives or kin-types and see how this

compares with these developments. The resulting picture lacks consistency due to the way legislation came into being but it is quite obvious that there is still a good deal of kinship recognition beyond the immediate, nuclear family. If one looks at the legislation on national insurance benefit, war pensions, legal aid assessment, income tax relief, and inheritance on intestacy an almost universal recognition appears to have been given to spouse, child, grandchild, parent, step-parent, grandparent, sibling, half-sibling, and step-sibling. The list of relatives who are mentioned in connection with certain aspects of the Act contain, for example, foster parents, siblings and half-siblings of one's parents, cousins, nephews and nieces, the parents and possibly further relatives of one's spouse, etc. One wonders whether all this shows anything more than incidental, haphazard facts of association of responsibility/ rights and certain kin-types: very little systematic structure would seem to emerge from the data.

Regardless of the value or importance of these particular facts I would suggest that we have here an indication that it may well be necessary or at least profitable to look at the matter from a different angle. At some later stage I shall introduce the concepts of 'nearness' and 'distance': I am hoping that these may prove to be suitable notions for discussing kinship terminology and its function in society, be it a simple or a complex type of society. It would certainly seem indicated to rethink this question of the relevance of ways of classifying kinsmen. In the past there were good legal

reasons why one should distinguish between paternal and maternal relatives: terminologically this was reflected in, e.g., the terms for 'uncle' and 'aunt' as shown in the Latin avunculus (MoBr) - patruus (FaBr) and amita (FaSi) - matertera (MoSi). When the reasons for the legal distinction ceased to exist the terminological distinction disappeared as well in many cases, but not in all: Swedish, for instance, continues to distinguish between FaBr, farbror, and MoBr, morbror, though the reasons for the distinction in terms of legal implications (basically the question of responsibility for one's sibling's children and the matter of inheritance) have ceased to exist in Sweden as they have in the rest of Western Europe (cf. Malmberg, 1966: 192 f.). Another puzzling case for which as yet no satisfactory explanation has been given is the development in some Germanic languages of the word of which the modern English form is nephew. In Middle High German the term neve can be defined as follows:

"neffe wie oheim, besonders: schwester-
sohn und mutterbruder ... geschwister-
kinder von der mutterseite ... verwandter
uberhaupt, besonders in der anrede"
(quoted in Bjerke, 1969: 30-32).

In MHG the following kin-types are indicated by the term neve according to Narro11 (1958: 753 f.): PaPaSb, GrNe, Un, Ne, Co(m). Perhaps one would hardly have expected this vagueness of reference to non-linear kin well before the nuclear family had achieved the exclusive position it holds at present. I find it even more puzzling, in the light of this commonly proposed theory concerning the growing importance of the nuclear family, that the

terminology on this point; rather than becoming less specific, has in fact become more precise, though not so much in Dutch where neef still means both Ne and Co(m) though no longer Un. I may as a native speaker of Dutch confuse occasionally the words nephew and cousin in English: I fail to see, however, in what way Dutch and English social organization or individual feelings might be significantly different with regard to the kin-types concerned. If both the sociological and the psychological explanations fail to give satisfaction, we must conclude once again that existing theories cannot be accepted as final and that, at least in order to deal with certain questions, it may be advisable to try and approach the problem from a different angle, to formulate our question differently.

5.2.3. Formal Approaches.

Our remarks and observations concerning the various formal approaches can be fairly short. Since our historical account of these studies followed fairly closely the discussion in this field, we have touched already upon a good number of issues. The present assessment will complement earlier remarks. I will consider three issues: the basic assumption of most types of formal analysis, aspects of componential analysis, and the extensionist theory. Other matters, not formal in themselves but which have come up in connection with formal studies, will also be mentioned briefly: they are matters of usage, of how language is used creatively. I shall discuss in particular the notions of metaphor and role.

5.2.3.1. The Basic Assumption.

When I discussed briefly Buchler and Selby's book on kinship and social organization (cf. above 4.1.2) I mentioned among other things the balanced view they took concerning the discussion of the biological vs. social conception of kinship relations. They did not, in fact, choose between the two methods but saw them as complementary: the genealogical method, especially when using some kind of formal analysis as a discovery device, will display more clearly the internal consistency of a set of linguistic data by mapping them onto biological kin-types, while the sociological method is more satisfactory from the anthropologist's point of view because it shows kinship terms to be category words by means of which an individual is taught to recognize the significant groupings of the social structure into which he is born. They reject any absolute claims for either solution and prefer a middle position:

"Biological kin type is a misnomer, and it would be more proper (if more awkward) to call these notational symbols [i.e. the symbols used to refer to kin types], sociological constructs, based in part upon biological considerations"
(Buchler and Selby, 1968: 35).

This issue has been under discussion from the very beginning of the study of kinship. Morgan related kin terms to genealogical positions; McLennan denied that they represented a system of blood-ties, he saw merely a social side to kinship nomenclature, viz. "a code of courtesies and ceremonial addresses in social intercourse" (cf. above 2.2.1). The social point of view became predominant from Rivers onwards, not in such an extreme

form as advocated by McLennan, but in a more moderate way, similar to the position of Buchler and Selby. The preference for the social interpretation became more marked in the period of structuralism and functionalism. As Buchler and Selby intimated, the opposite point of view tended to be held by those who advocated various kinds of formal analysis. I did not come across any explicit mention of this matter in Kroeber who is, in a way, the fore-runner of present-day feature analysis. He did, however, reject all social explanations of kinship terminology and many of the categories of analysis he proposed were in fact biological in nature. Davis and Warner (cf. above 3.3.2) spoke very explicitly of the biological foundation of kinship structure; theirs was, in fact, the most formalized analysis of the structuralist period. In more recent times the association between formalism and the genealogical/biological view can be in no doubt. It is in particular an integral part of the extensionist approach to kinship terms developed by Lounsbury; in fact, the whole of the second chapter of Scheffler and Lounsbury's study of Siriono kinship semantics is taken up by this argument. I shall return shortly to the extensionists' point of view; meanwhile, the general argument can be illustrated by reference to an exchange of articles initiated by Ernest Gellner's "Ideal Language and Kinship Structure" (1957) which provoked a forceful reaction from R. Needham (1960), followed by a rejoinder from Gellner (1960). J. Barnes (1961) joined in the debate to state clearly an anthropologist's view on physical and social kinship which made Gellner restate

once again, and very explicitly, his position with regard to kinship (1963). His original concern had been to develop an ideal language in reaction to "ordinary language" philosophy. He had chosen to try it out in the domain of kinship structure because he considered this to be an important and well developed part of social anthropology, a reasonably tangible aspect of society which could be stated with a fair degree of accuracy and would lend itself to comparison between societies (cf. Gellner, 1957: 235). He saw kinship as a sub-group of the relationships which make up society, and he observed:

"On what principle is this sub-group isolated from the total mass of relationships? Answer: by selecting those relationships which systematically overlap, in the anthropologist's view, (without being identical with) physical kinship. It is for this reason that he translates the indigenous words, which he finds used to denote those relationships, as 'father', 'brother' and so forth. What other principle could he conceivably employ for selecting 'kinship' relations?" (Gellner, 1963: 237).

An answer to this rhetorical question was given by John Beattie (1964); the following quotation from his article sums up well the opposite point of view:

"Kinship as it is studied by social anthropologists is not a set of genealogical relationships. This is so even though these relationships may (or may not) be denoted by terms having a genealogical reference, and even though they may (or may not) overlap with 'real' genealogical links between the parties concerned. Kinship can no more be reduced to a set of statements about genealogical connexion than, say, an enquiry into the social significance of funerary feasting can be reduced to a set of statements about the physiology of human metabolism. People eat, and people copulate and have children; the social scientist does

not doubt these facts, but in the context of his own enquiries he takes them for granted: they form no part of his explanatory apparatus" (loc. cit.: 101).

"The reason why an 'ideal language' based on the kinds of real genealogical connexion found in human (as in other animal) societies, can never have any relevance to the socio-anthropological study of kinship, is simply that the categories of kinship, as social anthropologists study them, are social and cultural categories, not biological ones. And there is no one-to-one correlation between the categories of kinship, and the kinds or degrees, or even the presence, of genealogical connexion. Entirely different levels of investigation and analysis are involved" (ibid.: 103).

In Beattie's view people use the language of kinship to talk about social relationships. This idiom is sometimes expressed in terms of genealogical connection, but it is not these biological connections the anthropologist is interested in. He points out that some kinship terms contain no reference at all to genealogical links in themselves:

"For example, the term mwihwa (or one like it), found in many Bantu languages, is usually translated as 'sister's child'. But it does not mean 'sister's child', and in fact a man applies it to a large number of persons besides the children of his sister. And the term nyinarumi, usually translated 'mother's brother', does not mean this; literally it means 'male mother', or 'mother man'. ... Kinship terms are not the names of genealogical connexion, even though they may be associated with such connexions; they are the names of categories, sometimes groups, of people, socially defined" (loc.cit.: 101).

I do not consider the actual examples convincing. First of all, if one takes a sociological point of view one cannot refer to 'Bantu' as to one group: with regard to culture and social structure there are quite important

differences within the linguistically defined Bantu group. Secondly, of the terms quoted, mwihwa is, by the author's admission, applied to the sister's child even if also to other individuals and could be said to be genealogically defined for that reason, while the example of nyinarumi is said to have the literal meaning 'male mother': one would like to know, however, if this is really the meaning as understood by the ordinary native speaker or merely an etymological analysis which is interesting for its own sake but irrelevant to present understanding of the term.

There are, however, more precise data available for one particular Bantu language, viz. Luganda, which can serve to illustrate the dilemma of the biological vs. social meaning of kin terms. M. Southwold (1971) distinguishes four categories of Ganda kin terms: those corresponding with straightforward English kinship terms (e.g. nnyina = mother), terms formed on similar principles to regular English kinship terms though not found in normal English usage (e.g. muganda = sibling of the same sex), terms which may be considered to combine English principles of formation albeit with unusual modifiers (e.g. nnyina omuto = little mother), and terms formed on principles quite alien to English social usage (e.g. mujiwa = child of female lineage member; kojja = male of mother's lineage). It is this last group which causes difficulty, for in Ganda culture 'lineage' is not a descent group, not a genealogical notion in other words: it indicates a group one can become a member of in various ways, though admittedly mainly by birth. It remains a fact, however, that these 'lineages' are more like

corporations, alliances, with social and political rather than genealogical overtones. I intend to return below (5.2.3.3) to the extensionists' claim to have found a way of dealing with facts such as these. Here I have developed this argument merely to show the basic difficulty of the notion of kinship and the delimitation of the kinship domain. The terms mujjwa and kojja are part of an intuitively recognised lexical domain, but at the same time it would seem that they do not refer to typically genealogical categories. If one argues that, since the other terms of the domain are genealogically based kinship terms, the whole domain must somehow be genealogically based, one is clearly courting circularity. If on the other hand one accepts that the two terms under discussion are not genealogical, the whole set would change in character, or if one continues to speak of the set of 'kinship terms', much of the theory on kinship would risk becoming meaningless.

It is not easy to see a way out of this dilemma, if there is one at all. It may well be a matter of temperament, or of the kind of question one is interested in, which of the two options one prefers. It remains, however, a basic option, a choice one must face up to. Consequently one would expect any study on kinship to be explicit on this point and as clear as possible considering the difficulty of the problem. My own position on this point is the following. It would seem to be fairly general practice that in determining the lexical domain of kinship scholars rely on meaning rather than on form: this meaning can be controlled, e.g. by the question "Who calls whom what?".

It has traditionally been accepted that there is a genealogical, biological foundation to the kinship domain. I accept that it would be unrealistic not to take into consideration this genealogical, physical aspect. I feel at the same time, however, that - though this genealogical side is part of the concept of kinship - it is not the decisive factor. The decisive factor is the social decision which gives the physical facts their meaning. I base this opinion on the following argument. I consider kinship to be a special type of relationship which obtains between limited numbers of individuals. If, however, we accept the physical unity of the whole of mankind (and for the sake of this argument we can take a monogenetic point of view in order not to complicate matters unnecessarily) while the notion of kinship is always applied to a very restricted group of people, we must conclude that this decision to recognize socially a bond of a special kind between such a restricted number of individuals only, is of paramount, indeed crucial importance. I call it a social decision because it is not forced upon us by physiological facts. It may well be that the whole issue is too complicated to be resolved either way by a decisive argument, especially such a brief one as I have just given. But I believe that my reasons carry a certain weight; for me they carry more weight than the arguments in favour of the genealogical position, and I see confirmation for my point of view in certain conclusions reached by anthropological theory. For example, Barnes (1961) has made the point that it is not sufficient to make a distinction (as was customarily done) between pater and genitor, i.e. between social father and

physical father, since even genitor merely indicates 'he who is considered by the community to be the physical father'. Whether he really is what Barnes calls the "genetic father" is not at issue.

It may seem that my argument about the domain of kinship remains solely in the realm of anthropology and does not concern us from a linguistic point of view; I believe it does. I believe that a distinction similar to the genealogical/social one can be made concerning terminology: kinship terminology is not identical with genealogical nomenclature¹. This point is not always clearly made. Weisgerber, as we have seen (cf. above 3.6.3), distinguished between "sprachliche Zwischenwelt" and "Aussenwelt", and he stated clearly that they should not be confused or simply treated as equivalent. Yet, at one point he seems to make an exception for kinship terms when he says:

"Es gibt gewiss Fälle, in denen Wortfeld und Sachbereich sich so nahe stehen, dass man den Sachbereich zum Ausgang der sprachlichen Betrachtung nehmen kann (etwa im Fall der Verwandtschaftswörter)" (1962c: 151).

"Sachbereich" refers - as is clear from his writings, e.g. 1951: 35 - to the 'objective kinship system' or the genealogical framework. I consider that this view is begging the question and see no reason for making an exception in the case of kinship. The only exception which might be acceptable would be - as Porzig (1950: 118) suggests - the special field of man-made objects with

1. I shall return to this distinction in my discussion of the deictic interpretation of kin terms (cf. below 6.2.3).

man-given names as long as and to the extent to which these names are technical terms.

Perhaps, however, my perspective is different from Weisgerber's. I would say that in an ethnolinguistic perspective I examine the way in which I verbalize the reality as I see and live it 'hic et nunc'. This introduces an element of subjectivity into objective reality, an element of relativism into what might seem natural and universal. Relativism - though not extreme or absolute relativism - is part of my basic assumption. Again it would seem that the findings of anthropology confirm this view. Adrienne Lehrer gave a number of these findings when she discussed the problem of semantic universals in connection with kinship (Lehrer, 1974: 168 ff.). She pointed out that Greenberg had suggested that three of Kroeber's eight categories were universal: generation (parent of, offspring of), consanguineal vs. affinal (relative by blood or marriage), and sex of relative (male/female) (Greenberg, 1966: 86 f.). While this is true in almost all cases, there are cases where these features are not valid or at least take on a very special value. The institution of parenthood, for example, among the Nayar of southwestern India who practise polyandry, is irreducibly different from ours; the Ijo in Nigeria have two kinds of marriages, a big payment marriage which gives complete control to the man over his wife, and a small payment marriage which establishes a relationship that may be only temporary, a kind of marriage institution which does not seem to fit easily into a universally valid definition. Lehrer summarizes her examination of semantic universals and the dilemma these

pose, as follows:

"If a semantic feature is to be universally valid, it must be so vague that it has to be defined for each language or group of languages. This makes it of limited value as a prime. On the other hand, if a feature is given a clear and unambiguous explication, it ceases to be universally valid, although its application may be widespread (op. cit.: 170).

Though I am inclined towards a moderately relativist position I also feel that the own world view of each culture and the way it is worded can only be fully appreciated and understood in the light of cross-cultural comparison. The same ethnolinguistic interest which led me to relativism now makes me look for some sort of universal frame of reference. How I view this universal perspective I hope to discuss presently (cf. below 5.3). How I conceive of the mixture of subjectivity and objectivity in the verbalization of kinship relations will be among the points to be discussed in chapter 6.

5.2.3.2 Componential Analysis.

The remarks I would like to make with regard to componential analysis can be brief. A certain number of reflections have been made in the section dealing with this analytical technique (above 4.3), while certain developments in the study of kinship terminology, in particular relational analysis and extensionism, came about because of dissatisfaction with the limitations of the simple componential approach. Moreover, what I have called the "basic assumption" in the preceding section is of immediate relevance to this and the next

sections. The reason why I return briefly and more specifically to this aspect is that I believe it will help us to take a different look at kinship terms which is complementary to the one presented by componential analysis. This alternative approach will, in fact, be suggested by the various criticisms and remarks which all seem to point in a similar direction.

The points I am going to make have to be understood with reference to the study of kinship terminology, not to semantic feature analysis in a wide sense nor in relation to some syntactic framework. I simply want to consider the adequacy of componential analysis as it has been used in connection with kinship terms, whether or not - or better perhaps: to what extent - it is a satisfactory way of studying the meaning of a socially important set of lexical items. My specific purpose puts limitations on the attention I give to (and the use I make of) formalism. Rather than leave my attitude towards formal analysis vague and in the dark, it would only seem correct to state that I feel a measure of sympathy with those anthropologists who are wary of too much formal analysis for fear of overlooking "what is important in the search for what is verifiable" (cf. Berreman, 1966). I have no real objections to the use of formal methods and techniques: on the other hand, however, I am quite prepared to forego some of the rigour and precision of formalism in order to get an answer to the question which seems important to me, viz. the search for an understanding of man through the study of language. I do not think we would stand to gain much from a discussion of the choice between what one might call 'formalistic' and

'humanistic' approaches since it is most probably a matter of difference of emphasis rather than of principle. As the value of a work, however, depends largely on whether it achieves what it sets out to do I thought it right to add this 'caveat' about my goal to my remarks about the formal technique of componential analysis.

Linguists who do not necessarily share my views on formalism often seem to recognize the limitations and as yet unclear aspects of componential analysis. All forms of feature analysis are still grappling with the concept of feature itself: what is the status of features, are they universal, do they have to be binary, etc.? Some factors are difficult to reduce to components, e.g. relational opposites: this requires that the components be made directional in some way. The hierarchy of hyponymy is a difficulty which has been met by the introduction of redundancy rules. It is sometimes doubted whether the exercise is worthwhile. If the components are lexical items it may be impossible to decompose them all and the definitions could be circular; if, on the other hand, they are, e.g., concepts it will be difficult to justify their choice. F. Palmer goes even further: he argues that the method is unworkable because we are faced with a potentially infinite set of components since in principle any piece of information may be used to disambiguate a sentence; and disambiguation of sentences is precisely the purpose and defining character of features in his view (cf. Palmer, 1976: 90 f.). I would not go as far as calling componential analysis unworkable; I would rather speak of its limitations. I have discussed this before (cf. 4.3.1) in terms of

'minimal meaning', i.e.: componential analysis of kinship terms tends to be restricted in extent by the delimitation of the lexical domain, e.g. by means of a restricting control question; and it tends to give only a single kind of meaning, viz. denotation, ignoring all other aspects and kinds of meaning. Both the question of the delimitation of the domain and the exclusive concentration on some kind of objective referent are linked with the basic assumption that kinship is connected with 'biology' (cf. Leaf, 1971, especially p. 545 f.). Roger Keesing called this restriction a 'conjuring trick' to make disappear a number of important problems of semantics: the trick consists in 'holding the context constant':

"What better way to make semantics look easy than to study kinship terms in their genealogical sense? Componential analyses, reduction rules, and many other solutions are possible; they are tidy, formally equivalent, and formally trivial. By taking only the genealogical senses of kinship terms, we define a frame and, at the same time, acquire a logically handy metalanguage for mapping them onto their denotata" (Keesing, 1972: 18 f.).

One of the problems componential analysis did not do justice to was polysemy: Keesing recognizes that Lounsbury's work on extension rules has come to grips with this aspect though he stresses the fact that the simplification of context is still found there (ibid.: 27 f.). He insists on the fact that possible meanings are sorted out precisely by context:

"To specify formally the spectrum of meaning of a kin term as it moves through different contextual frames is another matter [i.e.: from considering the genealogical sense only]. We lose both the

controlled constant frame and the metalanguage for mapping words onto the world. Nor can we specify the range of possible contextual frames and how they are interpreted. Consider English 'father' - simple enough in its genealogical sense. In its non-genealogical senses ('He was a father to me', 'Our other Father will take confession', 'Don't father me!', 'Our Father Who art ...'), we are often hard-pressed to analyze the meanings, how they are related, or what contextual information enables us to understand them. What defines 'father-ness' in these senses is not a common feature or a common behavioral role but a relational pattern of which each sense is a transform" (ibid.: 19).

Others have claimed as well that contextual features must have a place in a semantic description (cf. e.g. Michelle Z. Rosaldo, 1972); Tyler has actually enumerated a number of contextual factors to be taken into consideration:

"social setting, audience composition, sex and age of speaker/hearer, linguistic repertoires of speaker/hearer, and - most difficult of all - something that might be called the speaker's intention" (Tyler, 1972: 268).

It is far from clear yet how these factors actually shape the use and the significance of terms. It would seem, however, that it will only be by asking this kind of question however difficult to answer, that we can hope to gain really new insights into kinship terminology.

Removing artificial restrictions will also require that the matter of other aspects of meaning than denotation be included in our considerations. Words often signify a good deal more than the objects designated as their referents:

"Formal analyses remain inconclusive at the ethnographic level to the degree that they fail to exhaust the semantic 'value' of the elements they analyse" (Fox, 1971: 219).

In a general way this shift of attention towards context and towards kinds and aspects of meaning other than denotation will also mean that more attention will be given to the actual use of words. Fillmore has expressed the opinion that a number of difficulties encountered in semantics could be traced back to the wrong question being asked. The question generally asked is: What is the meaning of this form? It should be: What do I need to know in order to use this form appropriately and to understand other people when they use it? (Fillmore, 1971 a: 274). It is this question of the relationship between the meaning of a word and its use, between the form of the definition of a word and the conditions under which that word can be used, which is not touched upon in componential analysis (cf. Mounin, 1972: 127). The importance of this point lies in the fact that in real life there are no perfect systems which can be analysed in a clear-cut manner: there are potential conflicts, departures from the strict logic of the system. M. Hammer in discussing this point referred to E. Leach's description of the Kachin marriage system which appears to be governed by the following characteristics (cf. Hammer, 1966: 362 f.):

- (a) Sub-groups are hierarchically ordered; if A is higher than B, and B is higher than C, then A is higher than C.
- (b) A man marries a woman of a higher-status subgroup.
- (c) There is no set of people who cannot marry.

It is clearly impossible for all the three rules to be true if they were to be pushed to perfect consistency of application. In fact, the practice is flexible enough either because theoretical distinctions are being blurred

or because substitute values are accepted, e.g. geographical distance instead of hierarchy. It would seem to me to be worthwhile investigating whether the way we use a system (be it a social or a linguistic one) can help us to understand better the nature of the system itself. Some of the work done on fictive kinship¹ (e.g. Norbeck and Befu, 1958) would seem to go in the same direction. Eventually I would expect this line of research to link up with the discussion around linguistic pragmatics. The outcome of this discussion is, however, far from clear at present (cf. e.g. Schlieben-Lange, 1975) and there is a long way to go yet. But I believe that this avenue must be explored though I am well aware that the danger of oversimplification and of vacuous generalities is, if anything, much greater still in pragmatics than it is in semantics.

5.2.3.3. The Extensionist Theory.

Keesing, in his provocatively critical discussion of kinship studies praises Lounsbury's and Scheffler's work on the hypothesis of the extension of meaning in kinship terms as a major contribution to the understanding of polysemy (1972: 27). Though he continues to disagree with their basic choice of the genealogical approach, he recognizes the great advance this work has brought about compared with the achievements of simple feature analysis: for the neglect of the polysemic nature of kinship terms

1. Fictive kinship: a socially defined equivalent of affinal or consanguine ties.

was his second major objection to componential analysis (ibid.: 18; cf. 5.2.3.2).

The question of polysemy is a difficult one which normally arises in contrast with homonymy, whether, for example, the different meanings of flight are unrelated and therefore several homonymic words or whether they are somehow related meanings of one word. In the study of kinship terms 'polysemic' is rather used in contrast with 'monosemic', a term not very widely used in linguistics, presumably because - apart from strictly defined technical terms perhaps - words tend not to be unambiguously monosemic.

When we are dealing with a polysemic word we will want to consider the relations between the various meanings. We distinguish a central or primary meaning and derived or secondary meanings. We may find, furthermore, instances of metaphoric use. With regard to primary and secondary meanings we will have to determine which kind of criterion to use in establishing this distinction: historical, psychological, etc. In the case of metaphors we have to distinguish between living and dead metaphors: again this distinction implies certain assumptions about the criteria we are using in making it.

It is a simple matter of fact that the extensionist theory (possibly out of a universalist bias) has firmly linked together its treatment of primary and secondary meanings of kin terms with the assumption that kinship is genealogically, biologically based. I do not see any compelling reason why the genealogical assumption and the concept of primary meaning should be linked together, but I am left with the impression that some of the principal

protagonists seem to see it as an either/or choice. Scheffler has attacked J. Lave for holding that both genealogically based terms and non genealogical ones were equally fundamental or basic to the structure of the alleged 'kinship' domain: instead he claims that the system of "culturally posited relations of genealogical connection" forms a different system from other systems of classification of interpersonal relationships and must therefore be analysed independently (Scheffler, 1976: 338). I would have thought that, at least logically, there should be room for the possibility of the various meanings not being equally fundamental but being grouped and ordered with reference to some criterion other than genealogy. Whether this would be workable in practice, or at least be easy enough to make it worthwhile, would be a totally different matter. As it is, however, the alternative to a genealogically based polysemy is monosemy in its anthropologically inspired sense.

The argument in defence of the case for a monosemic interpretation of kin terms seems sound in itself. It was clearly formulated forty years ago by A.M. Hocart and, though he wrote from a historical linguistic point of view, his reasoning is equally valid in a synchronic perspective. He claimed that the extensionist point of view originated in cultural prejudice. When people discovered that the Fijian word tama was used to refer to a person's father it was labelled 'father'. When later on it was discovered to be applicable to other men besides the father, these uses were considered to be extensions of the original meaning. This, in Hocart's view, was a fallacy: one should have given another

label to express the essence of tama-ship instead (Hocart, 1937). Scheffler (1972: 314 f.) rejects this argument, mainly because in defining the category 'tama' Hocart referred to Ego's own father, i.e. its genealogically primary meaning. Hocart's definition is: "tama = all males of the previous generation on the father's side"; I am not convinced that Scheffler's reasoning holds good: I feel he makes rather too much of the reference to Ego's father in this definition. Equally the fact that tama can mean my father, real father, does not cause unsurmountable difficulties: if one holds a monosemic position one will simply claim that use and context specify the general meaning of tama. One may not find this approach very attractive or not very promising: this would be a good enough subjective reason not to pursue it. It would seem unwise, however, to ignore the challenge of a theory which appears to be coherent and internally consistent.

Lounsbury admitted in 1969 that the debate was undecided though he did not have any doubt in his own mind as to the validity of the extensionist hypothesis (1969: 27): I do not know of any argument advanced since then which could be considered to have settled the issue. There has, however, been an attempt at reconciling the two points of view by Maurice Bloch (1971). I have referred to his article before (cf. 5.2.2) in connection with the problem of the apparent lack of correlation between the Malagasy kin terminological system and the different forms of social organization found on the island. The special approach to kinship terminology he develops is also meant to reconcile the social category approach and the extensionist hypothesis.

Bloch states that

"certain words, including many kinship terms, are best looked at: 1) in terms of the place they hold in the system of values; and 2) from the point of view of which tactical use they can serve" (loc.cit.: 80).

This means that in Bloch's view both schools of thought were wrong in concentrating exclusively on denotation of already existing roles and ignoring strategic uses.

"Kinship terms do not denote kinship roles; rather they are part of the process of defining a role relation between speaker and hearer and they must also imply much more than the mere establishing of a kinship-line of communication" (ibid.).

One of the examples he gives is the term havana, used by the Merina of central Madagascar (loc. cit.: 81 ff.).

The term havana can be translated as 'kinsman', but in practice the word is used for anybody with whom one is claiming an obligatory relationship which is not based on contract. This might suggest to some that the primary meaning of the term is not genealogical. However, people speak of tena havana, i.e. 'true kinsmen', in contexts which unmistakably refer to procreation and genealogical links. The difficulty for an extensionist interpretation arises from the data concerning address rather than reference: one would not normally address genealogical kinsmen as havana, indeed the term is used for non-kinsmen on purpose, precisely because of the absence of genealogical links. Bloch's proposed solution consists in distinguishing the concept itself expressed by the term, and the tactics of the use of that term in given social situations. It does not make sense to try and list the

possible referents of a term like havana; but it is possible to list the tactics it can perform, viz. it can

- "1) exclude non-genealogical kinsmen;
 - 2) include non-genealogical kinsmen;
 - 3) exclude genealogical kinsmen; and
 - 4) include genealogical kinsmen"
- (loc.cit.: 82).

The tactics depend on the context while the concept is constant. This concept is - in Bloch's view - a moral concept, a judgement on people rather than a label.

"This means that as value judgements, kinship terms may contain elements of great significance which have nothing to do with what we normally think of as kinship and need to be defined in a much wider framework" (loc.cit.: 80).

It is claimed that this distinction between moral and tactical meaning makes the problem of extensions disappear:

"No particular use of a term need be more or less problematical since, given the concept, the use is seen always as a particular attempt to use a tool for transforming a social situation. The notion of extension at its most useful implies manipulation of a new social situation by using a term which has been given meaning in a previous context. In the perspective that I have stressed here all uses are, in the terms of the extensionists, 'secondary' and the meaning as a moral concept is an aspect of a culture's moral scheme" (loc.cit.: 86).

There would seem to be an added advantage in relating meaning of kin terms to a culture's moral scheme. Nogle (1974: 21) had pointed out that the extensionist hypothesis was very much linked with kinship analysis and dependent upon the particular characteristics of that domain. If it were true that this hypothesis (at least in its value for semantics generally) had a restricted applic-

ability its value would diminish somewhat. Bloch's approach would seem to be valid for all category terms referring to people, all terms which qualify people in various ways, for all such terms are moral terms in the sense defined. His proposals may have originated in his anthropological work: I believe, however, that they converge with linguistic ideas about meaning and use and about context, and deserve to be taken into consideration in linguistic studies about context and use.

5.2.3.4. Metaphor.

We discussed at some length earlier on (cf. 4.4.3) the question of metaphor with reference to the debate between Scheffler and Schneider on the use of the term "father" for a priest. After having assessed the discussion of the notion of extension in the previous section I thought it useful to add some separate comments on the subject of metaphor. It would have been possible, of course, to include metaphor in the section on extension: after all, metaphor is a form of extension of meaning. However, since the extensionist hypothesis in its actual form is so closely linked with a genealogical framework and with denotative meaning it seemed preferable to deal with metaphor separately because of what Scheffler called "the distinction between simple extension within the domain of kin classification and metaphoric extension" (1972: 318).

The concept of metaphor has been studied for centuries but this is not the place to go into the history of this subject. In any case, many of the studies were carried out from a literary point of view, with special

reference to poetry and to stylistics, or from a philosophical point of view concerning problems of reference and meaning. I would simply want to consider briefly the question of metaphor in relation with formal analysis or feature analysis, the subject I am concerned with in this part of my evaluation. Scheffler defined metaphor with reference to semantic features: in a metaphor one or more of the defining features (criterial attributes) of the primary sense of a word are suspended while some feature of connotative meaning is substituted in its place; this feature then becomes criterial for the use of the term (cf. 1972: 319). The idea that a metaphor can be analysed semantically in terms of features obviously appealed to scholars working on formal grammars and the concept has been studied in relation to the notions of deviance, anomaly, selection restrictions, etc.

From 1969 to 1975 the concept of metaphor was the subject of a series of articles along these lines in Foundations of Language (cf. Bickerton, 1969; R. Matthews, 1971; Price, 1974; Loewenberg, 1975). Without going into detail I would like to point to some interesting aspects of the debate. Bickerton presented metaphor as a process of invention. This idea combined well with his assumption that meaning does not exist in language: if it exists anywhere, then it is only in the relationship speaker-language-hearer. For him, therefore, meaning is somehow and to some extent linked with use. He goes on to develop the theory that certain lexemes are marked as potential metaphors: when these lexemes are used in a way which is deviant because of rule-violation we shall have metaphors.

Not all rule-violations result in metaphor, however. Matthews objected to this theory; moreover he levelled the general criticism at Bickerton that his theory was one of performance, not of competence. He saw metaphor as a selectional restriction violation as well, but not in connection with individual words: he considered sentences to be metaphoric. However, when he distinguished between simple deviance and metaphor he invoked the intention, on the part of the speaker, to be metaphorical: I would interpret this as a form of use-theory, and I do not think that the speaker's intention would fit into a competence model. This was exactly Price's objection to Matthews' article: since a metaphor cannot be interpreted without an appeal to its use and the intention of its user it must be characterized partly with reference to performance. Metaphor is an expression of the creativity of language, not the rule-governed creativity of linguistic competence but rule-changing creativity. Once a metaphor can be characterized with reference to a competence model only, it is a dead metaphor. Loewenberg discussed at length all preceding contributions to the debate as well as the work of Katz and Fodor and of Weinreich. At the end of it all she concentrated on the crucial point of how to identify utterances as metaphors. Inevitably the intentions of the speaker play an essential role and also the fact that, though the utterances in question are incorrect, they are not mere false statements. They fail as assertions, yet there is a presumption of truth and a willingness on the part of the hearer to interpret them in the context. A special speech-act is thought to be involved:

when a hearer identifies an utterance as metaphorical he does not understand it as making a truth claim about certain referents but as a proposal to view and understand those referents in a particular way. This speech act of 'making a proposal' gives an utterance a special heuristic value.

Max Black's philosophical reflections on metaphor would also seem to be relevant to our discussion (cf. Black, 1962). He distinguishes three views of metaphor: the substitution view (i.e. a metaphorical expression is used in place of some equivalent literal expression), the comparison view (i.e. a metaphor consists in the presentation of an underlying analogy or similarity, a condensed or elliptic simile), and the interaction view of metaphor. Interaction means that in a given context the focal word of the metaphor obtains a new meaning which is not quite its meaning in literal uses: the new context imposes an extension of meaning upon the focal word. The hearer is forced to connect two ideas and "in this connection resides the secret and the mystery of metaphor" (loc.cit.: 39). Black gives as example "Man is a wolf". We know the meaning of 'wolf', i.e. we know a system of associated commonplaces, standard beliefs about wolfs. This system is evoked by calling man metaphorically a wolf. The use of this metaphor will suppress some details of the commonplaces normally implied by the literal use of 'man', and emphasize others. What is happening in fact is that the wolf-metaphor is organizing our view of man (cf. loc.cit.: 40 f.). While the other two types of metaphor can be replaced by literal translations with little more than some stylistic or poetic loss, the interaction type of metaphor has a cognitive

content of its own (ibid.: 45 f.).

The conclusion of these linguistic and philosophical reflections seems to be that an adequate treatment of metaphor involves more than mere feature analysis: it involves notions of use, context, and speaker's intention. And if the metaphor has a cognitive value of its own it must be studied on a par with the semantics of primary or focal meanings of lexical items, not as an afterthought. The combination of the creative intention of the speaker and the cognitive value of the metaphor could well make this study of great relevance to ethnolinguistic studies, as indeed J. Fox has suggested:

"If one accepts as an ethnographic goal some attempt to represent the social classification of a people, ... it is precisely these metaphoric usages that appear to be indicators of significant forms of native semantic patterns" (1971: 220).

Perhaps we could say with Tyler:

"Variants are not mere deviations from some assumed basic organization; with their rules of occurrence they are the organization" (Tyler, ed., 1969: 5).

Michael Reddy in his study of metaphor (1969) has also argued that we should not set apart metaphor too much. It is not the case in his opinion, that language has a literal meaning which describes external events, the world outside: speakers of a language speak about their experience of the world, they construct their world, albeit functionally and according to rules. There is, in other words, a subjective element to any form of language usage. It is not pure idealism he advocates but a moderate form of realism, a relativist position in fact. Once again we

discover the importance of one's basic assumptions.

5.2.3.5. Role.

It will surprise no one that we find the concept of role used particularly by linguists who are interested in factors of context or use in language. The idea of role was developed, for example, by J.R. Firth (cf. Firth, 1935: 28 ff.). I have used the notion of role earlier on (cf. above 4.3.3.2) with regard to the discovery by children of the relational character of kinship terms. My use of the term 'role' is rather loose, more in line with the metaphor of the stage with all its connotations of free acceptance of a role, and of creative play and interpretation. In sociology the general tendency would rather be to understand the concept in terms of social determinism: a role is a bundle of norms for behaviour, of social expectations. Sociolinguists use this deterministic concept, e.g. Bernstein when he speaks of a person learning "to subordinate his behaviour to a linguistic code which is the expression of the role" (1967: 127), though it is never denied that there will always be an idiosyncratic element in speech. In anthropology and anthropological linguistics the concept of role has been used in a less strictly defined manner. I would like to discuss two uses which have been made of this notion.

It has been suggested that an exploration of role elements in human behaviour might provide us with a means to bridge the gap between kinship rules and actual behaviour (Keesing, 1972: 22 ff.). In an over-simple, idealized model of kinship any discrepancy causes difficulties.

Given, for example, the hypothesis that in a particular society relatives on the father's side have an obligation to support one another in difficult times - a social fact which would be reflected in special kin terms for this group of relatives - , we face problems as soon as we come across counter-examples, especially if these are fairly frequent and cannot therefore all be put down to ill will. An approach in terms of roles could possibly solve this problem. We could say that each member of society enacts many roles, an increasingly large number as society becomes more complex. One never enacts all these roles simultaneously; though some role-obligations, notably kinship roles, may count heavily in life, there will be times when, by a personal decision, one will let obligations from other roles outweigh the kinship obligations. Rather than treat this as an embarrassing or annoying breach of the rules we should consider the context in which such a decision was reached, in order to detect the underlying strategy. In this way the concept of a social identity as a cluster of different roles proves its usefulness. We can go further and pursue this same idea on a smaller scale and say that a specific role, e.g. the role of the father, is not a simple role but has a composite character and contains several role-elements. We could also see it perhaps in terms of a focal point towards which several roles converge. The fact that not necessarily all these roles or role-elements are enacted by all the referents of a term labelled "father", as was the case for the Fijian tama discussed above (5.2.3.3), does not present any problem once we take the context and the situation of each individual

into account. Some usages may be less typical but, unless they surprise the native speakers, they must be part of the system and enter into any explanation given.

A second use which could be made of the role concept would be more directly in connection with kinship terminology, viz. by examining what Tyler has called "the role specificity for variations in terminology" (1972: 254). It would consist in an effort to determine which role-functions or role-elements correspond with certain variations in terminology. This approach would make it possible to discuss a kinship terminology more fully, taking into account alternative forms and paying attention to both denotative and connotative aspects (cf. e.g. Yassin, 1977).

Sociologically relevant behaviour always has a purpose. Role systems such as kinship are being used by society for organizational ends (cf. Banton, 1968: 12): social relationships are made the basis for social organization. It would seem that if we want to examine the practical usefulness of the role concept and other related ideas we will have to look at the question of the purpose and function of relationships. I shall turn to this matter now by way of conclusion to this chapter of evaluation and assessment.

5.3. Conclusion.

I have tried in this chapter to explain and justify the background to my approach to kinship terminology. My position is that of a relativist. I do not deny the reality

of the world around us, but what matters in the study of man, of which the study of language is a part, is what is real to man, or better perhaps, what is the objective reality in as far as and in the way man perceives it: that is his reality. It is in this sense that I accept that we create our own world, socially and linguistically; and this means that in a limited sense each cultural group has its own world and world-view. I am inspired by the ideas of W. von Humboldt without necessarily following the neo-Humboldtians in every aspect or detail. Yet I recognize at the same time the need for universalism. On a general, philosophical level this idea imposes itself for the simple reason that I accept the unity of mankind: whatever differences there are can never touch the core of our being human. On a mere practical level I see a need for universalism precisely because I hold a moderately relativist position: it only makes sense to hold such a position if things are relative in mutual comparison and with reference to some standard. I do not look, however, in searching for this universal standard towards an objective outside reality seen as the 'substance' which underlies the 'accidents' of the different cultural forms, according to the Aristotelian model. I see this universality as one of purpose rather than of substance. I favour, in other words, a teleological explanation.

I must acknowledge here my second source of inspiration, the work of C. Lévi-Strauss (cf. above 3.5). His theory was teleological (cf. Maranda, 1964: 524) which means that in order to explain a given phenomenon he indicated its purpose, its aim. Concretely in the field of kinship this meant that Lévi-Strauss explained the

structure of kinship by stating its function, viz. to assure communication. This one function operated both on the level of kinship behaviour and on that of the language of kinship. These two, as he saw it, were not causally linked; they were complementary in achieving one and the same aim, viz. integration of social relationships within society (cf. Lévi-Strauss, 1958: 47):

"exogamie et langage ont la même fonction fondamentale: la communication avec autrui, et l'intégration du groupe" (1949: 612). From the philosophical point of view the teleological method of explanation may be fraught with difficulties (cf. Bocheński, 1954: 115), but it is an accepted method and I see no objection to using it provided it is done with great caution. I feel all the more justified to use this method of explanation since the causal explanations - in principle a much more powerful and reliable type of explanation - have not really been able to produce general and comprehensive answers to the problem of the social relevance of kin terms.

To say that I share Lévi-Strauss's teleological orientation does not mean that I am convinced that the precise function of communication is a good explanation of our problem. I have expressed my reservations on this point above (cf. 3.5.3). I would like to propose another function, not 'communication' but 'situating people, defining their place'. On the social level this would refer to the creation of social relationships "which make people socially dependent upon one another and build up sentiments of solidarity" (cf. Banton, 1968: 10). Bloch's work (Bloch, 1971), referred to in this chapter, on the

strategies of use of kinship categories would fit in well with this approach, as would e.g. Titiev's view of kinship as "an effective mechanism for disseminating important sociocultural values" (Titiev, 1967: 48) or many other studies on use, context, and variation in kinship systems. Each society may have its own particular social structure, but all societies have the same aim: to situate people in a network of relationships so as to achieve integration.

On the linguistic level we can equally find a mechanism which has as its aim to situate people - with linguistic means - in space and in time: this is achieved through the deictic mechanisms of a language. Jespersen speaks of "shifters", "a class of words ... whose meaning differs according to the situation" (1922: 123). I would like to suggest that this deictic character is not only used to situate speakers/hearers of a language spatially and temporally, but that there also is a social deixis which allows speakers to situate themselves socially through language. It is this suggestion I would like to develop in my final chapter.

6. Kinship Terminology As A Deictic System.

6.0. Introduction.

In suggesting that considering kinship terminology as a deictic system may lead to a new, complementary, and possibly better insight into the nature of this part of language, one must obviously explain how exactly 'deixis' and 'deictic system' are to be understood. When linguists use the term deixis they normally refer to certain elements in language, e.g., tense, pronouns, etc., which relate an utterance to the situation in which it is produced.

Weinreich, for example, mentions in this respect the time of discourse, its place, and the participants in the speech act (cf. Weinreich, 1963; 154). The anthropologist Malinowski, on the other hand, used the concept of deixis in a very different way, referring to the non-verbal activities which accompany the act of speech, viz. "deictic gesture as an integral part of language" (1935, vol. II: 30). What is common to both ways of using it is the fact that some kind of extra-linguistic point of reference is deemed to be necessary for interpreting correctly linguistic utterances. Neither point of view, however, would seem to be adequate for use in connection with kinship terminology. The first thing to be done, therefore, will be to re-examine the notion of deixis with a view to establishing its scope as well as determining a concrete, practical way of using it in an analysis.

After that I would like to present the terminological material on which I intend to test the usefulness of this deictic view. I have decided to present my personal use of

Dutch kinship terms for this purpose, for a number of reasons. I have quoted above (5.2.2) Haudricourt and Granai who warned that ethnologists studying cultures and social structures which are totally strange and unfamiliar to them can easily be misled in their evaluation of what is crucial in them: they may end up describing and analysing what is difficult to understand from their own point of view rather than what is of central importance to the society under examination. By opting for my own native tongue I hope to avoid this trap. Moreover, when dealing with one's first language one has an inside knowledge of the system which goes well beyond a mere scheme of classification and allows one to use this system for various social or personal purposes, to play games with it so to speak. Though I am reasonably fluent in both English and French I have never felt quite confident that I could do the same in these languages. I believe that one of the main reasons for this may well be that I have not acquired kinship terms as part of a personally lived experience in any other language than Dutch. When I say that I feel totally confident about the use of kin terms in Dutch I do not want to suggest that there are no areas of uncertainty in this usage. It means rather that I know that when there is uncertainty this is more likely not the result of an insufficient knowledge of the language but of a complex situation where the difficulty of linguistic expression merely reflects unclarity of personal feeling or of social position: such uncertainty can itself sometimes be very revealing. Finally I preferred to choose the terminology of a modern complex type of society rather than

one of a so-called simple society. The great importance of kinship in the latter type of society may, by the sheer abundance of its forms and its ramifications, make it more difficult to discern what is basic and essential from a universal and comparative point of view. Perhaps we will reach those basic elements more quickly when studying a type of society where the importance of kinship is less overwhelming.

One could possibly object to this concentration upon personal use that it may prove to be too idiosyncratic, too individual to be of general interest. The whole point, however, of this approach is to show that there is more to kinship terminology than its being a rather impersonal, general classificatory system: there is, in Bloch's words, the use of kin terms as tools for "transforming a social situation" and "the manipulation of a new social situation" (1971: 86; cf. 5.2.3.3).

In presenting the Dutch kinship terms I shall follow at first the pattern of the standard ethnolinguistic procedure of listing the terms and of analysing them by means of a distinctive feature technique. C.J. Fillmore has pointed out (1971 b: 220) that an examination of deictic elements is often rendered difficult by the fact that the data are incomplete and inconclusive at crucial moments because they were not collected with a view to a deictic analysis. For this reason I shall add extensive observations to this first analysis about actual usage of kin terms not normally given in traditional, genealogically based descriptions. In conclusion we shall then try to indicate how all these data may be interpreted deictically.

6.1. The Notion of Deixis.

6.1.1. General Remarks about Deixis.

If I had to choose between the various definitions proposed for the notion of deixis I would perhaps prefer the following definition by Charles J. Fillmore:

"Deixis is the name given to those formal properties of utterances which are determined by, and which are interpreted by knowing, certain aspects of the communication act in which the utterances in question have a role" (1971 b: 219).

This definition offers scope for a wider interpretation of deixis than a mere way of dealing with the spatial, temporal, and (inter)personal coordinates of an act of speech.

This wish for widening the notion of deixis is not merely an ad hoc device which I am trying to introduce. I believe it to be part of a whole set of trends in linguistics, some more recent than others, which would all seem to have one thing in common, viz. a growing realization that - though linguistics has been right in emphasising the study of language for its own sake and in its own right - it would be wrong to study language completely and exclusively "in vacuo" so to speak. There is, for instance, the need for considering context, linguistic as well as extra-linguistic. Furthermore the way we use language is important. The growing interest in pragmatics reflects this concern, while the study of speech acts tries to discover not merely the structure of utterances but also what one can do with them. Presuppositions and the extent to which they are shared are points which retain

the attention of scholars, as do the various levels of speech and so-called social dialects. Robin Lakoff has written:

"What I am saying, then, is that the pragmatic content of a speech act should be taken into account in determining its acceptability just as its syntactic material has been, and its semantic material recently has been" (R. Lakoff, 1973: 293).

She outlines three areas of pragmatic behaviour: the speaker's assumptions about his relations with his addressee, his real-world situation in which he is communicating (viz. the importance of the communication, the seriousness of his effort to communicate, the formality of the speech-act situation), and his decisions based on the two previous points as to the effect he wishes to achieve through his communicative act (ibid.: 293, 296).

D. Wunderlich has put forward a similar view:

"Es kann keine Frage sein, dass die Erforschung der pragmatischen Aspekte der Sprache die zukünftigen Aktivitäten im Bereich der Linguistik wesentlich bestimmen wird" (1970: 8).

Among the linguistic elements

"die sich nur dann adäquat behandeln lassen, wenn die Theorie der sprachlichen Kompetenz auch die Elemente von möglichen Sprechsituationen einschliesst"

he mentions "deiktische Äusserungen" (1971: 156). F. Antinucci, too, links 'context' and 'deixis': deixis is defined with reference to the notion of context or "la situazione in cui viene prodotto un atto linguistico" (1974: 225).

Wolfgang P. Schmid has claimed that the foundations for a pragmatic component in grammar were laid on the one hand

by the work of certain philosophers, notably C.S. Peirce and J. Bar-Hillel, and on the other hand by linguistic work on deixis:

"In den Blickpunkt der Sprachwissenschaft wird das, was wir mit pragmatischer Komponente bezeichnet haben, erstmals von K. Bühler gerückt. Er entwickelt - gestützt auf Vorarbeiten von Ph. Wegener und K. Brugmann über die idg. Demonstrativpronomina - eine Zweifelderlehre für die menschliche Sprache und analysiert ihre Sätze in ein Zeigfeld und ein Symbolfeld. Im Mittelpunkt des Zeigfeldes stehen die drei Zeigwörter ich, hier und jetzt" (1972: 7).

Of the philosophers mentioned Bar-Hillel discussed the case for pragmatics in his article "Indexical Expressions" (1954), referring both to indexical sentences and indexical non-sentential expressions. The term 'indexical' was taken from Peirce's classification of signs according to their way of signifying, viz. symbolically, indexically, or iconically. A sign represents its object to its interpretant indexically "by being in existential relation with its object (as in the case of the act of pointing)" (Burks, 1949: 674). Unfortunately this term has been understood and defined in many slightly differing ways (cf. Lyons, 1977: 105 ff.). However, this is not the moment to enter into a philosophical debate and I therefore prefer to concentrate on the linguistic leads which the quotation from Schmid's paper has given us.

Brugmann, in his comparative study of Indo-European demonstrative pronouns (1904), tried to establish in parallel with the "Aktionsarten" of the verbs the "Demonstrationsarten" or deictic functions of the pronouns (op.cit.: 9). He considers pronouns as merely standing "pro nomine", instead of a noun, and their use as an

ellipsis (ibid.: 4 f.). Situational features or gestures ensure that adequate communication is achieved; historically the phenomenon of deixis is thought to be linked with the gesture of pointing (ibid.: 7 f.). He distinguishes four "modes of pointing" (ibid.: 9 ff.):

- Dér-Deixis: this is the basic deictic function of pointing towards an object without specification of distance
- Ich-Deixis: the speaker directs the attention of his interlocutor onto himself or his own sphere of interest
- Du-Deixis: this is the Dér-Deixis in so far as it has a special link with the interlocutor
- Jener-Deixis: these pronouns point towards what is distant either in space or in time, or to what is on the other side.

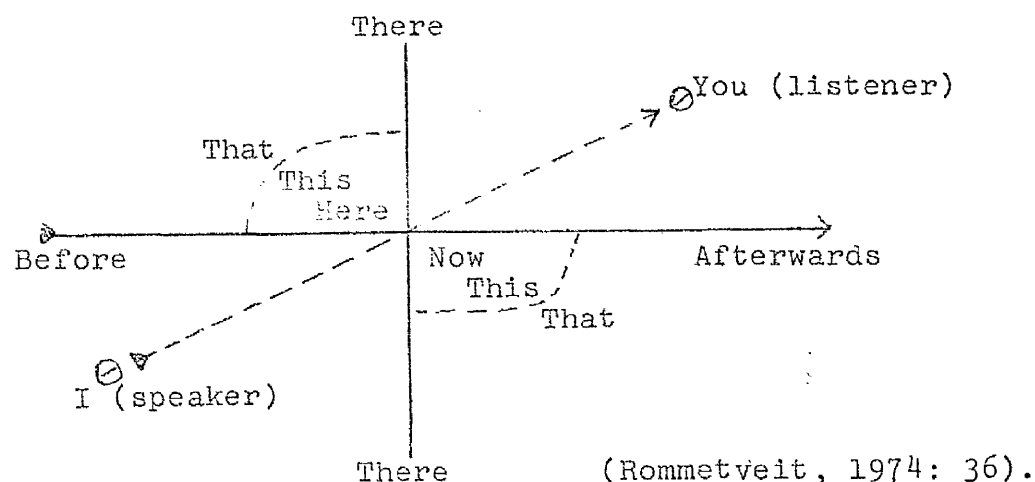
This four-way division is not as clear as one might perhaps have wished in strict logic. K. Bühler mentioned already proposals for changing Ich-Deixis and Du-Deixis to hic-Deixis and istic-Deixis since it is allegedly not the person but the place where a person finds himself which is pointed at (Bühler, 1934: 83). This reflects the idea that the spatial location is the most fundamental category in linguistic expression. E. Cassirer has claimed that languages as a rule specify place, situation, and distance obligatorily and meticulously:

"All other specifications are thrust into the background by this spatial characterization, or are represented only indirectly through it" (1953, vol. I: 200).

A similar idea is reflected in a proposal by J.A.F. Wils

(1938) that the whole pronominal system has developed as a deictic system starting from the demonstrative pronouns. In this process, it is suggested, the spatial dimensions come to be used for other forms of personal and emotional distance.

Earlier theories may, in fact, have stressed the importance in deixis of the pointing gesture accompanied by, or possibly replaced altogether by, a vocal gesture of deixis. Gradually, however, it was seen in a more systematic and abstract way as the basic structure of a speech situation centered around a speaking subject, the "I". There was a certain shift of emphasis from a purely localistic point of view to a more psychological one. Bühler distinguished the "Symbolfeld" of the "Nennwörter" from the "Zeigfeld" of the deictic elements; central to this 'Zeigfeld' are the words hier, jetzt, and ich. Another psychologist writing about deixis in language, Ragnar Rommetveit, has presented the same idea in the following more elaborate diagram:



The fact that "I" is more basic than "here" and "now" is not

only demonstrated by the fact that "here" and "now" are defined with reference to "I" but also by the development of communicative skill in children. A young child tends to be "egocentric" in his deixis, i.e. he assumes "that the world is unequivocal and monistic: his immediate here-and-now constitutes the centre of whatever world he can share with others" (Rommetveit, 1974: 42). At this stage a child is unable to adopt the perspective of the other person: only gradually his capacity for decentration, and with it his mastery of deixis, develops (ibid.: 43). Fillmore refers to the person who produces a linguistic expression as "the center of the associated communication act" (1971 b: 222).

Wunderlich has claimed that nine aspects must be considered in the study of a speech act: the speaker, the hearer, the time of speaking, the place, the phonological and syntactic properties of the utterance, its cognitive content, various preconditions (about what the speaker knows and assumes the hearer to know, about their social relationship, etc.), the intention of the speaker, and the relation which is established between speaker and hearer as a result of the utterance (1971: 178). Without commenting on the details of this proposed list, it must be clear that so many things are now considered as being part of what was traditionally known as deixis that some subdivision of this concept seems indicated. Fillmore has proposed a five-way distinction:

"One speaks of person deixis (references to the speaker and the addressee), place deixis (references to the locations of the speaker and the addressee), time deixis (references to the time of the speech act),

as well as references to portions of the utterance itself (discourse deixis), and references to the relative social statuses of the speech act participants (honorific systems, etc.)" (1972: 18).

Once we enlarge the scope of deixis we must also reconsider the conditions for deictic elements to be effective in communication. These elements have a wide domain of potential reference in the case of pronouns; the same is true for other words but in a different way. It remains true in fact for words which have not only a literal meaning but also - depending on the social situation - various extended meanings. Which precise reference is going to be realised in an utterance will depend on what is cognitively salient at a given moment in a particular situation. Rommetveit says:

"The message is thus mediated by words. Its specific content, however, stems exclusively from my temporary cognitive representation of a domain of particular persons and places" (1968: 188).

The words which are used are not the whole message, there is a non-linguistic constituent established by a particular convergence of cognitive orientations on the part of the participants of the speech act: "There is thus a peculiar dynamic complementarity between linguistic and non-linguistic constituents" (ibid.: 189).

Brugmann, as we saw, spoke of 'ellipsis' in connection with the use of deictics which replace nouns or names. Rommetveit reverses the situation: "Ellipsis", he claims,

"appears to be the prototype of verbal communication under ideal conditions of complete complementarity in an intersubjectively established, temporarily shared social world" (1974: 29).

One might not want to reverse the problem simply in order to establish once again an order of preference where, in fact, we have an interaction of elements which are blended according to the requirements of the situation in such a way that communication is ensured. I think it is wiser to follow Bühler (1934: 154) and Wunderlich (1970: 8) in replacing "elliptisch" by "empraktisch"¹. In any case, the shared social world is essential; in this world a sentence - or word - is assumed to be used correctly when it is used

"appropriately with respect to the speaker's intentions and with respect to the norms of interpretation for the language community in which the communicating event takes place" (Fillmore, 1973: 100).

The fact that utterances are embedded in speech act situations may seem to set us a hopeless task since particular situations are unpredictable and cannot be listed. It would be a wrong approach, however, if one were to try and start from single situations. What matters is discovering the mechanism which makes it possible for a language to be effective. This is what gives unity to the variety of speech act situations. The dimension along which this mechanism operates with regard to place, time, and person is that of distance:

"Categories of person, place and time deixis can be distinguished according to the opposition proximal and distal" (Fillmore, 1971 b: 222).

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1. Bühler speaks of "empraktische Rede" in the case of "Einbau des Sprechens in anderes sinnvolles Verhalten" (1934: 52).

Obviously, if need be, this distinction can be refined as Fillmore has done, for instance, for place deixis where he proposed a three-way distinction into proximal, closer distal; and farther distal (ibid.: 225), or proximal, medial, and distal (1966: 221). These dimensions are a means of locating the participants in place and in time, a way of determining their position with regard to the coordinates of the speech act situation. It can be claimed, however, that there are other dimensions involved than mere physical distance. R. Lakoff has examined the demonstrative pronouns this and that, and she has claimed that there is a case for distinguishing spatio-temporal deixis, discourse deixis, or anaphora, and emotional deixis in the use of these pronouns. This, for example, can serve to convey greater vividness, or familiarity, or even contempt:

"He kissed her with this unbelievable passion"

"This Fred Snooks turns out to have 24 cats".

(Lakoff, 1974: 347 f.).

She concludes:

"I have shown that there is a clear linguistic link between emotional and spatial 'closeness' and 'distance': these are not mere accidental metaphors. And the rules that correctly predict the spatial uses of this and that should somehow also serve to account for their discourse and emotional uses. How this is to be done remains mysterious" (loc. cit.: 355).

My proposal to consider kinship terminology as a deictic system is similar in inspiration to Mrs. Lakoff's discussion of demonstrative pronouns. As I intend to go beyond the field of pronominal deixis, however, it seems desirable to proceed in two stages. Firstly I shall consider

the case of pronominal deixis to see if I can throw any further light on the "mysterious" question of integrating the "spatial" and "emotional" parameters of the categories of closeness and distance. I have chosen to examine deixis in the personal pronouns for the simple reason that, in my opinion, every speech act is centered round the "I" of the speaker and must be interpreted with reference to the person of the speaker. L.L. Becker and I Gusti Ngurah Oka summarize their study on person in Kawi as follows:

"A central thread - perhaps the central thread in the semantic structure of all languages is the cline of person, an ordering of linguistic forms according to their distance from the speaker. Between the subjective, pointed, specific pronominal "I" and the objective, generic common noun, between these poles the words of all languages - words for people, animals, food, time, space, indeed words for everything - are ordered and categorized according to their distance - spatial, temporal, social, biological, and metaphorical - from the first person, the speaker. The cline of person also underlies most linguistic systems as well as words, systems of deixis, number, definiteness, tense, and nominal classification among others" (Becker and I Gusti, 1974: 229).

One may feel that this is too sweeping a statement; yet I remain convinced that the personal pronouns are the most basic, the most widespread, and the most efficient means of identifying the participants in a discourse (cf. Krupa, 1976: 150), not only with regard to the roles of speaker and hearer but also to their relative social status (ibid.: 154).

This last aspect leads me on to the second stage in which I shall argue the case for the possibility of extending the notion of deixis beyond pronominal systems to

social categories and consider some of the implications of such an extension.

6.1.2. Person Deixis.

6.1.2.1. General discussion.

When we analyse the personal pronouns in English we only need the features 'person', 'number', and, to a lesser extent, 'gender' to account adequately for the members of this set. However, when one considers other, especially non-European, languages it is obvious that such an analysis is far from universally valid, and one might argue that an analysis in terms of universally valid features is preferable, not merely when wanting to compare systems but under all circumstances. Ira R. Buchler and R. Freeze have attempted to summarize the findings of analyses of a wider range of pronominal systems and have deduced from them a number of distinctive features with a view to establishing a typology of this class of words (Buchler and Freeze, 1966). The notion of universally valid distinctive features is taken from Jakobson and Halle's study of phonology (1956). The distinction these authors had made in the case of inherent features between sonority and tonality features is said by Buchler and Freeze to correspond, in the field of pronouns, with on the one hand formal features, on the other hand social and cultural features. It is not my intention to examine the Buchler and Freeze article, the claims they make, and the analyses they propose in every detail. I would simply like to present the features they have selected with a few words of

explanation, and then look at the way they present the Dutch pronominal system in terms of this typology. I shall then try to present a 'deictic' analysis of that same system, taking into account as well the way the system is used.

The first of the formal features is symbolized as $M : \bar{M}$, i.e. minimal/non-minimal membership. This corresponds with singular/non-singular (i.e. dual ... plural). The second feature is $S : \bar{S}$, i.e. inclusion/exclusion of speaker; the third one is $H : \bar{H}$ or inclusion/exclusion of hearer. These two features serve to distinguish first, second, and third person pronouns from one another. They are also instrumental in characterizing the distinction between inclusive and exclusive first person plural pronouns which a number of languages make. Gilyak, for example, distinguishes ñin = we, exclusive, i.e. I and they (not-you), or $S \bar{H} \bar{M}$, from miñn = we, inclusive, i.e. I and you, or $S H \bar{M}$ (cf. Austerlitz, 1959: 102). The last formal feature which Buchler and Freeze propose is $MM : \bar{M}\bar{M}$, i.e. maximal membership/non-maximal membership. They introduce this feature to account for pronouns which, as far as number is concerned, mean "two or more" and "three or more", as a way of avoiding a three-way division $M1$, $M2$, and $M3$ for the feature of minimal membership. One of their examples is taken from Totonac (Mexico) where we find:

<u>ama</u>	he, she, they	
<u>huix</u>	you (one only)	
<u>huixin</u>	you (two or more)	(loc.cit.: 95).

Ama is a cumulative term which can denote "one only", "two

only", and "more than two"; hence it has the feature 'maximal membership range'. I have no doubt that some feature of this kind is required for facts of this nature. I find it difficult, however, to decide whether the proposed analysis is satisfactory as some obvious questions remain unanswered and unanswerable from the data published, e.g. what is one to make of the overlap between "two or more" and "three or more" in their Tzeltal data from Mexico. Since this feature does not play a role in the Dutch personal pronouns, and since I intend to analyse these pronouns primarily with a view to demonstrating the possibility and likely shape of a deictic analysis, not to contributing directly to the typology of pronominal systems, I feel fully justified in suspending judgement on this matter.

The four social and cultural features are SL:SL̄, i.e. solidary/non-solidary, ML:ML̄, i.e. male/non-male, P:P̄, i.e. person/non-person, and PR:P̄R̄, i.e. proximate/non-proximate.

There is no explanation why the feature male/non-male is treated as a social and cultural feature. When Paul Friedrich discussed the ten components of Russian pronominal usage he spoke about "discriminations that are 'biological' in a sense, although defined in terms of the culture" (1966: 229). Among these he counted relative sex because it could decide usage

"in the sense that two speakers of the same sex were normally more prone to use familiar terms, whereas speakers of opposite sexes would exercise greater restraint" (ibid.: 230).

It is possible that Buchler and Freeze had something similar in mind, although Friedrich is dealing with the usage of personal pronouns in mutual address and the role of relative sex in that event, while Buchler and Freeze seem more interested in the analysis of the terms as an abstract system of reference. I find the classification of the person/non-person feature as social and cultural equally difficult to understand. However, I will return to these questions directly or indirectly when I propose my deictic analysis of the Dutch personal pronouns. From that analysis it will appear that in Dutch the issue of person/non-person is chiefly a matter of anaphora and gender rather than of person deixis.

The feature of solidary/non-solidary derives its name from the solidarity semantic discussed by R. Brown and A. Gilman in their article "The Pronouns of Power and Solidarity" (1960). In it they examined the covariation between the pronoun used in address and the objective relationship existing between speaker and addressee. If the relationship is asymmetrical with one person having power over another they speak of a power semantic: the superior says T and receives V. Solidarity is a symmetrical relationship. The former type of relationship is associated with respect, the latter with intimacy. As pronouns are analysed from the point of view of the speaker the feature has sometimes been characterized as "inclusion of the status of the hearer/exclusion of the status of the hearer" (Buchler, 1967: 39); it is also known as the honorific dimension. Various factors can enter into play here. Friedrich lists ten components as I mentioned already: the topic of discourse

the context of the speech event, age, generation, sex, kinship status, dialect, group membership, relative jurial and political authority, and emotional solidarity (cf. Friedrich, 1966: 229). Brown and Gilman (op.cit.: 255) spoke mainly of institutionalized roles, also of sex and age, and, to a lesser extent, of physical strength and wealth as bases for power. Respect and formality were associated with V, intimacy and condescension with T (ibid.: 258). Jerome C. Ford (1974) has re-examined the question for French after the social upheaval of May 1968 and its aftermath in France. His findings are that V is used to express respect based on power; V is also used without a power basis as an expression of formality, and it is even used with disdain as a refusal of 'camaraderie' and intimacy. What appears to be constant in all cases is the notion of distance. T is used not merely to address those without power: it basically marks solidarity, including the solidarity of those who share power. The use of T can become a bid for acceptance, trying to bridge the gap, to come nearby, hoping for 'camaraderie' and eventually intimacy. It seemed important to me that throughout the various usages the same mechanism appears to be at work, viz. that of creating or maintaining distance, or of diminishing it.

Once we accept that 'distance' is the crucial aspect in the feature of 'solidarity', one might wonder how to relate this to the feature proximate/non-proximate which indicates physical distance, proximity to the speaker. This was in fact the question left open by R. Lakoff in her article on the demonstrative pronouns in English (1973; see

above). I pointed out when first referring to her study that I would want to come to some sort of integration of spatial and emotional distance, or better perhaps: to an integrated view of the feature 'distance'. I am bound to say, however, that it is not my intention simply to do away with the distinction between spatial and emotional distance and retain one, unanalysable concept of distance, or introduce a highly abstract notion of distance which could be realised as either 'spatial' or 'emotional' distance. One very good reason for maintaining the separation between the two kinds of distance is the fact that the two can and do in fact occur in opposition as distinctive features within one system. A clear example of this is provided by the third person pronouns in Bengali, reported by R.J. Di Prieto (1971: 127):

	ini	uni	tini	ẽra	õra	tãra	e	o	še	era
human	+	+	+	+	+	+	+	+	+	+
honorific	+	+	+	+	+	+	-	-	-	-
number	-	-	-	+	+	+	-	-	-	+
proximal	+	-		+	-		+	-		+
	ora	tara	eta	ota	šeta	egulo	ogulo	šegulo		
human	+	+	-	-	-	-	-	-		-
honorific	-	-								
number	+	+	-	-	-	+	+			+
proximal	-		+	-		+	-			

These data show three things. First of all it is possible for pronouns to be marked with a different value for each of the two kinds of distance (i.e. honorific and proximal); this is the case for uni, õra, e, and era. Secondly it

appears that certain pronouns can be marked for one of the dimensions in question but unmarked for the other one: they are tini, tāra, še, tara, eta, ota, egulo, and ogulo. The last and decisive fact, however, is that certain pairs of pronouns are distinguished by the opposition honorific/proximal only, viz. ini and uni, ēra and ōra, ēra and era, ōra and era, and e and o.

If, therefore, we cannot suppress the distinction between the various kinds of distance (spatial, social, or other), in what way can we arrive at an integrated view of this category? I would see it as follows. Deixis is a mechanism in language which serves the purpose of situating people and events relative to the speaker, from zero distance or identification with the speaker to any distance away from the speaker. This mechanism operates both on the syntactic and lexical levels of language.

Not all languages will use the same means to achieve this expression of distance but the mechanism does exist and operate in every language (cf. R. Lakoff, 1972: 908). Five "modes" of distance have been proposed and seem adequate considering the present state of our knowledge. They are different kinds of 'distance' but they are teleologically one (cf. 5.3 above), in that they all achieve one and the same aim.

'Distance' is one concept as a value or moral meaning in Bloch's sense (1971; cf. 5.2.3.3 above): this moral meaning is put to use in different strategies resulting in various tactical meanings. We saw, for instance, how the socio-emotional distance of V in French

could in practice be used to express either respect or its opposite, disdain: in the first case one does not dare to come too close to the other; in the latter it is a question of not wanting to get too close to him. Physical and emotional distance may reinforce one another or cancel one another out. Suppose I owe someone respect and formal behaviour because of his superior position. If he lives, moreover, outside my immediate environment, this formality may increase. On the other hand I may be less overawed by his superior position since I do not experience his authority directly: as a result I may behave in a more relaxed, less formal manner. Which strategic use is made of the basic value of distance and how to interpret it will normally be clear when one takes into account what is 'cognitively salient in the temporarily shared social world of speaker and hearer', to use Rommetveit's terminology (cf. Rommetveit, 1968, 188 f.; 1974: 29 ff.; see above 6.1.1).

6.1.2.2. Dutch Personal Pronouns.

Buchler and Freeze conclude their study of the distinctive features of pronominal systems by a typological characterization of a number of systems, listing for each one the number of units it has, which features are crucial in it and for how many terms. Their typological summary of the Dutch personal pronouns reads:

number of units	M: \bar{M}	S: \bar{S}	H: \bar{H}	SL: $\bar{S}\bar{L}$	ML: $\bar{M}\bar{L}$	P: \bar{P}	PR: $\bar{P}\bar{R}$	MM: $\bar{M}\bar{M}$
9	7	5	7	4	2	0	0	0

(loc.cit.: 100).

I am not quite sure which nine terms were selected, nor are the details of their analysis completely clear to me from this information. I presume they did not include gij. This would be understandable since it is a little used form. Moreover, it would actually be impossible to distinguish between gij and U, as can be seen in the matrix below, merely on the strength of the features proposed here. This, too, makes me think that gij must have been omitted. If this is the case, however, it would mean that they did include het (it): but then I do not understand why the column for person/non-person has zero marked on it. It may be the result of the fact that they do not mark in their feature representations any component which is not strictly required to distinguish the representation of one particular form from that of all the other ones in the set. Not marking het for male/non-male is, in fact, sufficient to distinguish it from all other forms.

I shall now give the complete matrix for the personal pronouns in Dutch, using Buchler and Freeze's features:

	M	S	H	SL	ML	P
ik	+	+	-			+
jij (je)	+	-	+	+		+
U		-	+	-		+
gij (ge)		-	+	-		+
hij	+	-	-		+	+
zij (ze)	+	-	-		-	+
het	+	-	-			-
wij (we)	-	+				+
jullie	-	-	+	+		+
zij (ze)	-	-	-			

The forms in between brackets are the unstressed ones. They are used in normal speech in Dutch while the stressed forms are reserved for emphatic speech.

The spaces left open in the matrix indicate that the feature in question is unspecified for the pronoun concerned. This presentation does not, however, give a complete account of the facts. The fact that het is [- person] implies more than the mere lack of specification with regard to [+ or - male]: it actually rules out that possibility. The third person plural pronoun zij, on the other hand, which can be [+ male], [- male], or [- person] shows that [- person] and [+ or - male] are compatible within one and the same form unless, of course, one decides that there are two homophone pronouns zij, one [+ person], the other [- person]. These points are not made explicit in the matrix as it stands.

Other observations concerning the third person singular pronoun point to further, no less important problems. First of all there is the question of anaphora. Anaphora is sometimes considered to be a special form of deixis. Antinucci (1974: 225) calls it 'internal deixis', while Fillmore seems to treat it under the heading of discourse deixis (1971 b: 227). The pronouns hij and zij can be deictic in the strict sense or they can be anaphoric. They are deictic in as far as they are potential participants in the speech act. Het can practically only be anaphoric.

Once hij and zij are used anaphorically the question of gender rather than sex becomes the most important criterion in the choice between the two, at least in reference to inanimates. The case of using she for ships,

however, goes beyond these rules and in fact breaches them. It could be argued that what is primarily involved here is a change from [- person] to [+ person]; once the word is transferred into the category [+ person] the choice between [+ male] and [- male] imposes itself. In such a case it appears that the choice of the female variant is felt to express greater intimacy or personal involvement. I am not sure how much importance to attach to this last aspect in relation to general usage unless perhaps an inverse relation could be established between the sex of the speaker and the value selected for [male]. But I do not know of any evidence for female speakers using the feature [+ male] to express greater intimacy and the like. If such evidence cannot be found it may be merely a feature of men's speech, not of general usage. There is an indication of a different kind that the choice between [- person] and [+ person] may at times be socially or emotionally relevant. I am referring here to anaphora in connection with neuter nouns. When we compare, for example, kind (child) and meisje (girl), both neuter nouns, we see that the anaphoric pronoun for the former can be either masculine or feminine, i.e. hij or zij, or neuter het for kind, while it can only be feminine zij for meisje. The social reason usually given for explaining this difference is the fact that sex is felt to be irrelevant in the case of children. As a matter of fact the neuter anaphoric pronoun can only be used as long as we are speaking of small children. However, the example of kind is unique in being a simple noun: all other possible examples of neuter nouns referring to human beings are diminutives. Their neuter

gender is merely a grammatical feature.

When we look at the second person pronouns we note first of all that an extra feature becomes necessary if one includes gij in the list of pronouns. In standard Dutch gij is used exclusively in very formal, mainly written, language and more particularly in religious language, though in southern dialects it is simply the normal form of the second person singular. It occurs only in the subject case; standard Dutch uses U for all other cases. The special character of gij could be called [solemn]. The normal form of polite address is U, both for the singular and the plural. This pronoun originated most probably in a nominal expression like Uwe Edelheid (cf. Your Honour): this made it grammatically a distant, third person expression. This still shows in the fact that with reflexive verbs we find the third person reflexive pronoun as often, if not more often, than the formal second person reflexive pronoun when the subject pronoun is U. For instance, we would rather say "U vergist zich" (You are mistaken) than "U vergist U". Politeness demands distance, and third person implies distance.

In addressing one of my parents I shall always use U, never the solidary jij. However, in the plural I can use the solidary jullie as well as the formal U. My interpretation of this would be that the plural makes the deixis less concentrated; as a result distance is more easily kept and too much familiarity is avoided. The use of an U which would combine plural and formality might, by a kind of cumulative effect, come across as too formal in certain family relationships. However, when I am

addressing an audience in, for example, an academic or an ecclesiastical context I must use U.

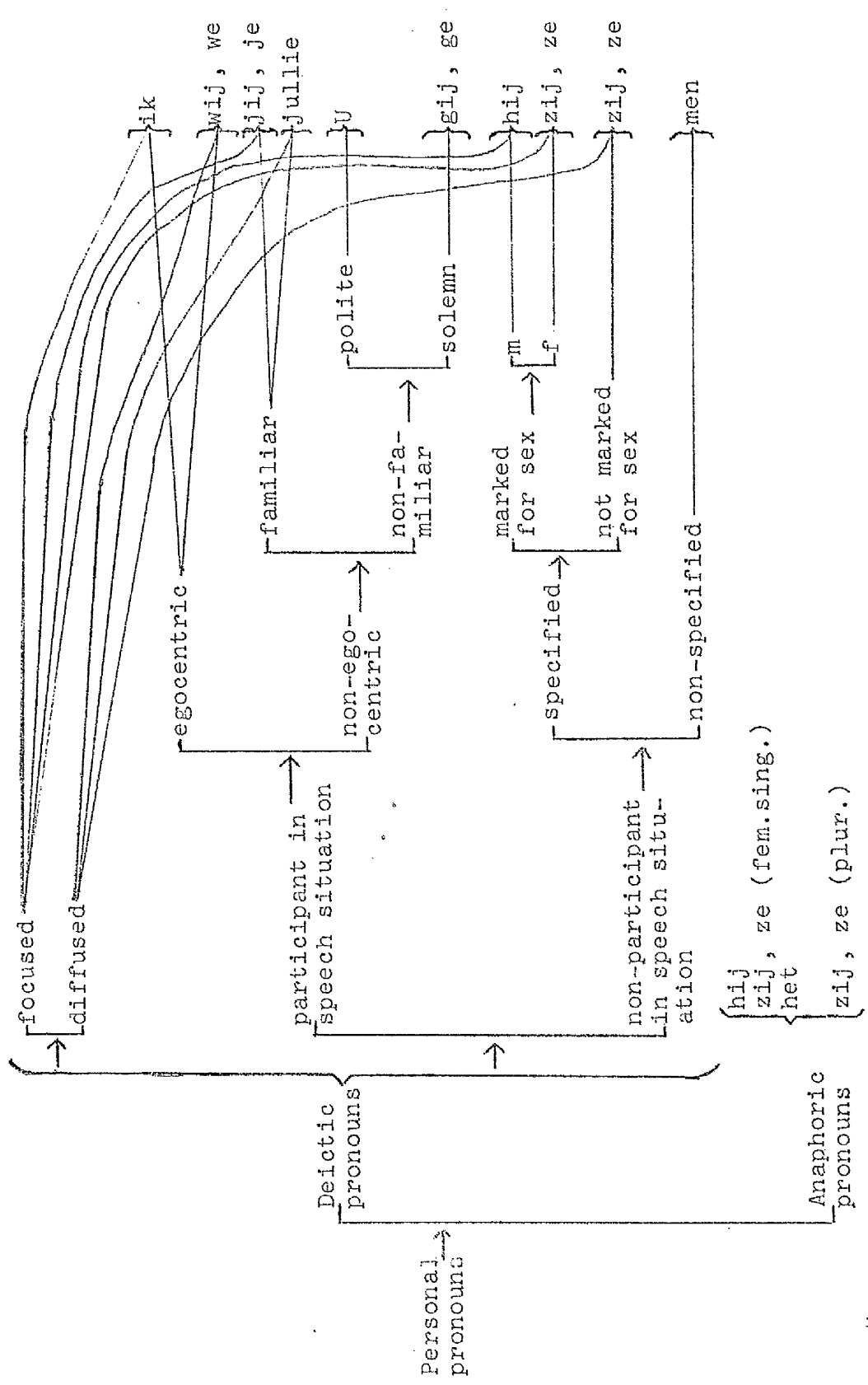
The use of jij/jullie or U is not reciprocal. When I am teaching in a primary or secondary school I will address the pupils with jij/jullie, while they are expected to use U. When I address the whole class I will not automatically use the plural jullie. I could say, for example: "Hoe zou je dat vertalen?" (How would you translate that?) with the intention to suggest or stimulate a greater and more direct personal involvement. If there is no such reason one uses jullie, e.g.: "Jullie kunnen nu wel gaan" (You may leave now). Another way of achieving a similar result of suggesting or stimulating more personal involvement is to switch from the second person plural to the first person plural, e.g.: "Hoe zouden we deze zin kunnen ontleden?" (How could we parse this sentence?). There certainly are different nuances in the use of jij or wij in these classroom situations, chiefly dependent on the degree to which a teacher identifies himself with the student body. I see, all the same, an indication here of a certain similarity between the two usages. Deixis relaxes to the extent it moves away from the "I"; it also relaxes when it is spread out over more than one referent. This would explain why both the shift of person and the change of number do achieve similar, albeit not quite identical, results.

This interpretation of the way we use pronouns would seem to be confirmed by the indefinite pronoun men (one) and its alternatives, viz. the (unstressed) je and ze (third person plural). Men is totally unspecified, remote, and - if used frequently - gives an impression of formality

which borders on pedantry. Ze is an alternative which, by virtue of being third person and plural, is fairly remote and vague; but since it is a personal pronoun it lacks the rigid impersonal formality of men. Je is much more colloquial, suggesting closer personal involvement, but it can only be used in an unofficial, informal setting.

After discussing the Dutch personal pronouns with reference to the distinctive features proposed by Buchler and Freeze and adding some observations on my use of these pronouns I would now like to present this lexical field in a way which incorporates all of these data. This 'deictic' presentation will consist (as explained above 6.1.2.1) of two parts. First we shall analyse and present the basic value of each element of the set; after that we shall try to formulate the tactical meanings which may result from the use of these elements.

My presentation of the system of personal pronouns is inspired in part by Renira Huxley's presentation of deictics in general (1970: 145). However, I wanted to incorporate the singular/plural distinction too. It seemed preferable not to use a branching tree for this presentation in order to avoid the problem of cross-classification. I therefore decided to adapt M.A.K. Halliday's semantic network (e.g. Halliday, 1973: especially p.47) which allows for simultaneous systems having entry condition.



This scheme needs a few words of explanation. First of all I have to point out that the distinction between deictic and anaphoric pronouns as given here can be misleading if this graphic representation were read as if they were on a par: I tend to think that anaphora is a derived form of deixis. Moreover, a proper study and analysis of anaphoric usage would also have to examine its relationship with the demonstrative pronouns. I have not carried out such an analysis here and have simply listed the pronouns in question. It seemed desirable, however, to mention them in order to delimit more clearly the range of person deixis.

Secondly, I have given the stressed and unstressed forms as alternatives: there is no reason to complicate the analysis by trying to account for these variants by means of extra labelled nodes where a single rule will do. The rule is that in spoken language the unstressed forms are normal, the stressed ones are used to mark emphasis. The emphatic use is impossible by the nature of things in the indefinite uses of je and ze. The unstressed form ge is rare in standard Dutch: its very character of extreme formality practically rules out a simple, non-emphatic use. Thirdly, I have added - at the cost of an extra feature - the indefinite pronoun men, mainly because I had referred to it in my previous discussions. Whether this would be the best way of accounting for this term in a complete treatment of pronouns remains to be seen.

Lastly I would like to explain the terms I introduced to mark the singular/plural distinction: focused and diffused. This use is inspired by R. Austerlitz's analysis

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of Gilyak pronouns. He used focus to indicate that deixis was directed at one single point, and spectrum when focus was relaxed (Austerlitz, 1959: 104). He interpreted a politeness form equally as a case of relaxing the deixis, and the use of the third person pronouns rather than the second person ones between newly-weds in the culture he was describing as a manifestation of timidity, a relaxing of deixis out of shyness (ibid.: 106).

When we come to discuss the strategy or tactics of person deictics we shall take as our starting point the various aspects of the deictic mechanism. First of all there is the aspect of distance, the distance away from the speaker, from "I". Next there is the direction of this pointing movement, either between the interlocutors or turning away from this main axis towards a non-participant. Finally there is the concentration or focusing of the deixis. These aspects must be seen as complementary rather than as opposites.

The following tactical meanings can be achieved by using these possibilities:

- distance can express respect, contempt, or informality
- direction can express impoliteness or rudeness (e.g., when speaking about someone who is present in the third person within his hearing and with the intention of being heard by him), or timidity, delicacy, extreme respect (compare the origine of U or German Sie, or the use of impersonal men in tactful reproach: "One should not do this").
- concentration can serve to express either separation (e.g. the jullie of our class-room example; perhaps also the

royal Wij), or inclusion and personalization (cf. the we of the same class-room example). Though I have dealt with the emphatic forms of pronouns separately one could possibly consider to deal with them here as a case of sharpening of the focus.

These various tactical rules can either reinforce each other in a cumulative way (e.g., the distance, direction, and relaxed focus of the impersonal use of ze as opposed to the impersonal use of je), or keep each other in balance (e.g. when the diffusion of jullie compensates for the lack of the politeness factor in it when I use this term to my parents instead of U). These rules would seem to be sufficient to interpret and disambiguate any use of personal pronouns in Dutch. Each particular interpretation will obviously only be possible within a given situation and be dependent upon it. We are merely concerned here with matters relating to competence: the analysis of a system of values and their possible tactical uses.

This, I believe, shows that it is possible to perform an analysis of elements like personal pronouns in terms of deixis, and what form such an analysis could take. It is one thing, however, to give a deictic analysis of pronouns but quite a different thing to claim that it can be done - and can be done to good effect - with a set of nouns. Before I attempt, therefore, to analyse deictically Dutch kinship terms I would like to return for a moment to the notion of social deixis in order to consider the justification of this expansion of deixis and deictic analysis, and its limitations.

6.1.3. Social Deixis.

In my general remarks about deixis (cf. above 6.1.1) I quoted the abstract of an article by L.L. Becker and I Gusti Ngurah Oka (1974) in which it was claimed that words for everything

"are ordered and categorized according to their distance - spatial, temporal, social, biological, and metaphorical - from the first person, the speaker".

R. Rommetveit speaks about the openness which characterizes not merely deictic words but also definite expressions: what is identified by such expressions must be assessed in terms of the intersubjectivity at the moment of speech (1974: 44 f.). This - in his view - makes speaking about literal vs. metaphorical readings of sentences as such rather uninformative. I believe claims of this kind to be exaggerated. I am afraid that if complete vocabularies were seen as organized according to the one criterion of 'distance', even with five modalities, such a classification would be highly artificial and pretty well meaningless. As for Rommetveit's view, I have made it clear earlier on (cf. above 5.2.3.4) what my position is on the question of metaphor. There is a subjective element to any form of language usage but it goes too far to say that definite expressions have the same 'openness' as deictic elements, that they are empty shells, as it were, which are only being filled with meaning by virtue of being deictically anchored (to use Rommetveit's favourite term) in the intersubjectively established 'here-and-now' of the act of speech.

Can it, then, be meaningful at all to speak about social deixis in connection with general vocabulary, or at

least some parts of it? When we accept Bühler's clear-cut distinction between "Zeigwörter" which depend for their meaning on the "Zeigfeld", the experiential, ever-changing context of a speech act, and "Nennwörter" which are symbols and are meaningful within a given semantic field (cf. 1934: 80 f.) it is impossible to speak of the deixis of "Nennwörter". It is perhaps significant in this respect that the widening of deixis to include social deixis has started with, and has until now been chiefly concentrating upon, marginal cases of pronominal deixis, viz. those cases where the relative social position and relationships of the participants have an influence upon and are reflected in the choice of special pronouns, the so-called honorifics. Wunderlich referred to these explicitly (1971: 159), as did Fillmore (1971:b: 224), and most of R. Lakoff's article "Language in Context" (1972) was an analysis of an example of context-bound usage, viz. honorific expressions. As the choice of pronouns is dictated by politeness, intimacy, or rudeness, a certain amount of attention is also given to the question of speech levels. Generally speaking these studies did not concentrate exclusively, or not necessarily even principally, on lexical elements; they also consider 'appropriateness' in sentential expressions. A good example of this is R. Lakoff's article on "The Logic of Politeness" (1973).

Within systems of polite address, however, we find not only pronouns but also certain pronominal expressions, e.g. "Your Honour", or "Your Majesty". More significant, however, are probably the words for certain roles or functions used in some cultures as forms of polite address. Howard H. Hatton mentions in his study of Thai pronouns that the old

watchman at his apartment used to greet him with the honorific title of ?aacaan, "teacher", to which he replied using the kin term luŋ, "uncle" (1973: 228). Dieter Wunderlich mentions among the examples of "sprachliche Ausdrucksmittel von deiktischer oder situationsexplizierender Art" (1970: 24) first of all deictic expressions but immediately afterwards "Formen der Kontaktaufnahme" (ibid.: 26). With this general formula he refers to various forms of greetings, expressions of politeness, respect, or intimacy, but even more, in his view, to different role relations that exist between speaker and addressee. Since such social roles involve two or more persons, each one of those relationships can be specified according to the relative positions of the persons involved. The linguistic utterances related to these roles are equally specified according to the relative positions of the role-bearers. Probably the most fundamental social role system, one which exists in every society, is the kinship system. One is, therefore, fully justified in approaching such a system in terms of deixis, social deixis. I mentioned in my general discussion of person deixis (cf. above 6.1.2.1) that P. Friedrich distinguished ten discriminations or cognitive components underlying Russian pronominal usage; in a postscript to his study he pointed out that "the discriminations, in modified form, also underlay the equally important symbolism of the kinship terminology" (Friedrich, 1966: 252). This would seem to confirm my conclusion. J.L. Fischer, in an article entitled "Words for Self and Others in Some Japanese Families" (1964) proposed a new approach to the study of kinship terminology. Rather than study various formally defined lexical categories separately

he began to look at the entire range of forms used to refer to "self" and "addressee" within the household. Suzuki Takao has taken up the same idea in a perspective which seems similar to my deictic approach, as the following quotation shows:

"Upon examination it becomes clear that although the Japanese use so-called personal pronouns, we also use many other words in conversation to identify speaker and addressee. For example, in many families today fathers identify themselves in conversation with their children as otōsan (father-polite) or papa. But if that same man who calls himself 'papa' to his children is, let us say, a school-teacher, then he will probably call himself sensei (teacher) when he is talking to his pupils in school. And should he find a lost child by the roadside, he will like as not call himself ojisan (uncle; general term for male adults when addressed by children), and say something like, 'Stop crying, now. Ojisan will see that you get home all right'. The same may be said for modes of identifying the addressee in a conversation. In the last example of the man who found the lost child, he might well have said, 'Oniichan (big brother, endearing), stop crying,' and the child could just as well have answered, 'Does Ojichan (mister) know where I live?' When looked at this way, words for identification of speaker and addressee in Japanese conversations are more complex than generally thought. Personal pronouns are, if anything, in the minority.

I have decided to call the phenomenon of speakers using different words to indicate themselves 'speakers' linguistic self-definition'. Linguistic self-definition constitutes a determination of the speaker's location of himself along the axis of language. Further, I call the speaker's behavior in choosing a word to identify the addressee, 'linguistic other-definition'. This too sets a linguistic coordinate, and, in tandem with linguistic self-definition, serves to conform linguistically the human relationship involved" (Takao, 1976: 255 f.).

If there is this similarity between pronominal and kinship systems and if, as we have implied, anaphora

or reference is not the most essential part of deixis, we could ask ourselves what to make of the traditional distinction in kinship studies between terms of reference and terms of address. Was John F. McLennan right after all when he claimed that kinship terms were merely "a code of courtesies and ceremonial addresses in social intercourse" (1886: 273; cf. above 2.2)? I made it clear when I discussed his position that the distinction would have to be looked at again in connection with use, context, and pragmatics. In the light of my deictic approach I would see this matter as follows. What is essential is that kinship terms are the linguistic means of indicating the position of relatives in relation to one another, as seen from the point of view of whoever happens to be the Ego in a given situation. These terms have a basic value and, moreover, are used according to certain strategies. Which term fits a particular situation depends on those two things. When we are referring to a relative we are in a different situation than when we are addressing him: this will be one of the aspects to be taken into account, but then so will, for example, the purpose for which we refer to him or address him, the mood in which we address him, etc. Certain terms are more suitable in some situations than in others; some are exclusive to a special kind of situation. Under this last heading would come the terms which are traditionally classified as 'terms of reference only' or 'terms of address only': their tactical use is limited. Some languages have such strict restrictions on the use of certain terms that other uses are not merely uncommon but outright unacceptable. In many other languages it will be a matter of certain terms being more appropriate

in certain situations with some overlap between different uses. These appropriateness conditions will depend, for instance, on what we want to achieve by a particular speech act, what kind of feeling we want to convey, or, in a given context, who is in our audience, whether a person is being addressed directly or merely listening in, etc.

My claim is that there exists a similarity between the pronominal and kin terminological systems, and that consequently the latter too can be analysed deictically. I shall now present the Dutch kinship system in order to try and verify this claim.

6.2. The Dutch Kinship System.

6.2.1. A First Analysis.

The Dutch kinship system was described thirty years ago by the anthropologist H.T. Fischer (1947) with a view to deepening our understanding of society by the study of kinship terms. It was an effort to broaden the scope of kinship studies and include modern Western European societies in the range of these studies. I will take Fischer's article as my starting point.

He distinguishes between terms of reference and terms of address in accordance with common practice. As I have explained earlier (cf. above 6.1.3) I hope to account for this difference in use in another way, within the framework of the general tactical rules of appropriate usage.

Fischer gives a list of 33 terms; square brackets indicate the terms or the kin-types which presuppose a female Ego.

This is his list:

1. vader (Fa)
2. moeder (Mo)
3. vrouw [man] (Wi [Hu])
4. kind (Ch)
5. zoon (So)
6. dochter (Da)
7. broeder (Br)
8. zuster (Si)
9. oom (FaBr, MoBr, FaSiHu, MoSiHu)
10. tante (FaSi, MoSi, FaBrWi, MoBrWi)
11. neef (BrSo, SiSo, WiBrSo, WiSiSo, [HuBrSo, HuSiSo], FaBrSo, MoBrSo, FaSiSo, MoSiSo)
12. nicht (BrDa, SiDa, WiBrDa, WiSiDa, [HuBrDa, HuSiDa], FaBrDa, MoBrDa, FaSiDa, MoSiDa)
13. grootvader (FaFa, MoFa)
14. grootmoeder (FaMo, MoMo)
15. oudoom (FaFaBr, MoFaBr, FaMoBr, MoMoBr, FaFaSiHu, MoFaSiHu, FaMoSiHu, MoMoSiHu)
16. oudtante (FaFaSi, MoFaSi, FaMoSi, MoMoSi, FaFaBrWi, MoFaBrWi, FaMoBrWi, MoMoBrWi)
17. achterneef (BrSoSo, SiSoSo, BrDaSo, SiDaSo, FaFaBrSo, MoFaBrSo, FaFaSiSo, MoFaSiSo, FaMoBrSo, MoMoBrSo, FaMoSiSo, MoMoSiSo, FaBrSoSo, MoBrSoSo, FaBrDaSo, MoBrDaSo, FaSiSoSo, MoSiSoSo, FaSiDaSo, MoSiDaSo)
18. achternicht (BrSoDa, SiSoDa, BrDaDa, SiDaDa, FaFaBrDa, MoFaBrDa, FaFaSiDa, MoFaSiDa, FaMoBrDa, MoMoBrDa, FaMoSiDa, MoMoSiDa, FaBrSoDa, MoBrSoDa, FaBrDaDa, MoBrDaDa, FaSiSoDa, MoSiSoDa, FaSiDaDa, MoSiDaDa)
19. kleinkind (SoSo, DaSo, SoDa, DaDa)

20. kleinzoon (SoSo, DaSo)
21. kleindochter (SoDa, DaDa)
22. overgrootvader (FaFaFa, MoFaFa, FaMoFa, MoMoFa)
23. overgrootmoeder (FaFaMo, MoFaMo, FaMoMo, MoMoMo)
24. achterkleinkind (SoSoSo, DaSoSo, SoDaSo, DaDaSo,
SoSoDa, DaSoDa, SoDaDa, DaDaDa)
25. achterkleinzoon (SoSoSo, DaSoSo, SoDaSo, DaDaSo)
26. achterkleindochter (SoSoDa, DaSoDa, SoDaDa, DaDaDa)
27. schoonvader (WiFa, [HuFa])
28. schoonmoeder (WiMo, [HuMo])
29. schoonbroeder/zwager (SiHu, WiBr, WiSiHu, [HuBr, HuSiHu])
30. schoonzuster (BrWi, WiSi, WiBrWi, [HuSi, HuBrWi])
31. schoonzoon (DaHu)
32. schoondochter (SoWi)

(cf. Fischer, 1947: 105 f.)

I will return below to the elements one could add to the list (see 6.2.2). Before commenting further on these matters I will first discuss the analysis of the data listed.

K. Ishwaran, who had direct experience of both Indian and Dutch cultures, noted the great importance of the nuclear family for the understanding of Dutch kinship (Ishwaran, 1965: 2*). This does not mean that the relations between parents and children are not particularly important in every human society: there is, however, a clear shift of emphasis towards a greater importance of the nuclear family in complex societies like the Dutch one. The precise degree of importance is difficult to measure: if we accept, however, that the occurrence of a special term in a given language for a particular social institution can give an

indication of its importance to the speakers of that language, we certainly find confirmation for the sociological claim about the centrality of the nuclear family in Dutch social life in the fact that we distinguish in Dutch between gezin and familie. Gezin is the unit of the nuclear family, familie refers to other relatives. As a matter of fact, in everyday usage familie can be used as an alternative for verwant, "relative", probably an elliptic use. We say, for example: "Is hij familie van jou?" (Is he a relative of yours?). If the person in question were my father or brother I would not deny it but, unless I did not want to specify the exact relationship, I would answer: "Ja, hij is mijn vader/broer" (Yes, he is my father/brother), not: "Ja, hij is familie van mij" (Yes, he is a relative of mine). The relationships within the nuclear family are highly individualized, too special, I feel, to be adequately covered by the general term familie.

A Dutchman normally belongs to three families (in the sense of gezin): his family of origin, his family of procreation, and the family of origin of his wife. This finding is supported by a number of facts. With regard to kinship terminology Fischer (loc.cit.: 112) has pointed out that the terms used for relatives in these three families are all either 'descriptive' (i.e. indicate one person only, viz. vader, moeder, man, vrouw, schoonvader, schoonmoeder) or 'semi-descriptive' (i.e. may indicate more than one relative but no relatives in different kinds of relationship, viz. broer, zus, zoon, dochter, schoonbroer, schoondochter), while outside these three families no relative is referred to by these descriptive or semi-descriptive terms. From the

sociological point of view Ishwaran (loc.cit.) points to the lack of continuity in the Dutch family which, at least in an urban setting, seldom consists of more than two generations. Moreover, the nuclear family is neolocal,¹ and has usually little or nothing to do with the economic occupation of its members. If we think of Wilmott's study (1958; cf. above 5.2.2) of kinship in connection with social legislation and the range of statutory liability, we see that legislation in force in the Netherlands since 1 January 1965 (almost twenty years after Fischer's article was published), the so-called "Algemene Bijstandswet", has in fact reduced this range to husband and wife, children, parents-in-law, and children-in-law. The case of foster parents who enter into this obligation voluntarily and temporarily is clearly different. A similar picture emerges from the legislation on inheritance "ab intestato". It divides the relatives into four groups of potential inheritors. Group one consists of the marriage partner and the children, group two - which inherits in case there is no relative from group one - contains the parents, and the brothers and sisters. When one gets to groups three and four of the more remote relatives the system of partitioning the inheritance changes. No longer does one consider the individual relative: parts of the inheritance are assigned to the patrilineage or matrilineage taken as a group. This would mean, for example, that a patrilateral grandfather would get the same share as a matrilinear second cousin if both happened to be the first in line in their respective groups.

However, even though the nuclear family appears to

1. i.e., it establishes itself as an independent household.

be the most striking characteristic of the Dutch kinship system, the distinction between lineal and collateral relatives is not wholly to be discarded. In fact, certain terms are used exclusively for lineal consanguineal relatives; they are never used for any other type of relative. The terms for collateral relatives (with the exception of broer and zus) are also used for affinal kinsmen.

Another important feature of the Dutch kinship system is the distinction of generation, reflected in a number of prefixes (groot, overgroot, klein, achterklein, oud) which make it possible to mark generation difference with accuracy. The only terms for which this is not possible are neef and nicht: their referents, as well as the referents of their derivatives with the prefix achter, belong to two or three different generations respectively. Fischer explains the importance of this feature in terms of the need for authority in a properly functioning society while he also seems to accept the anthropological theory that this need is the real reason for the existence of the (near universal) incest prohibition (loc.cit.: 110). It is doubtful whether many anthropologists would still accept this explanation for the incest prohibition (cf. e.g. Fox, 1967: 54 ff.), but this need not concern us here. He does, however, put forward an opinion in this context concerning marriage regulations and kinship terminology which requires comment. He mentions that by law marriage is forbidden between uncle and niece, or between aunt and nephew, and he writes:

"De sociale onaanvaardbaarheid van dit soort huwelijken springt in het oog, wanneer wij de consequenties, die zij

voor de verwantschapsnomenclatuur hebben,
beschouwen"

(The social unacceptability of this kind of marriages is obvious when we consider the consequences they have with regard to kinship nomenclature). He refers to the fact that, e.g. Ego's father would become his/her brother-in-law, that he would be both grandfather and uncle to Ego's children, etc. However, I am totally unconvinced by this reasoning. In the previous chapter I have discussed the concept of role (cf. 5.2.3.5): every member of society enacts many roles, it was said, but not necessarily all of them simultaneously. If in a given society forms of marriage which are prohibited in our Western European culture are socially acceptable, no problems of the kind mentioned by Fischer seem to arise. As a matter of fact the uncle/niece marriage is common in many parts of Africa (cf. Fox, 1967: 58). When such a marriage is contracted the niece simply exchanges her role of niece for that of wife. The fact that we make a problem out of the possible terminological confusion merely reflects our uneasiness about this kind of marriage but is in no way an explanation for existing prohibitions. When the Civil Law concerning marriage regulations was changed in the Netherlands in 1970 the prohibition of the uncle/niece type of marriage was maintained after much discussion. In all the twenty years it took to prepare this major legal reform of 1970, the main arguments in favour of maintaining the prohibition in question were related to matters of genetics and decency. The argument of generation distinction, authority, and proper functioning of society (if ever it was

an argument at some previous time, which is not certain), was never used.

A last feature which has to be taken into account in analysing Dutch kinship terms is sex. The only terms in Fischer's list to which this does not apply are kind and its complex derivatives. This exception is of negligible importance, especially since (as if often pointed out) these terms refer primarily to relatives of non-marriageable age.

We could represent the characteristics of the Dutch kinship system more systematically in the following way. I would call the first dimension we discussed, viz. the place and importance of the nuclear family in the system, the nature of the relationship; the second dimension would be generation, the third one sex.

- Nature of the relationship.

I make the following distinctions here:

1) Relationship within the families of origin or procreation, or not:

N = near relatives, belonging to either the family of origin or the family of procreation

R = remote relatives, i.e. all relatives outside those families

2) Relationship through blood or by marriage

K = consanguineal Kin

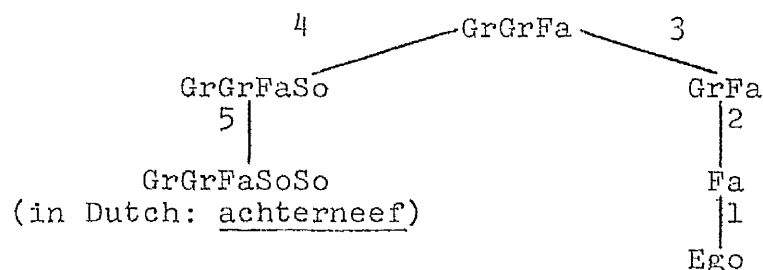
A = Affinal kin

3) Relationship in direct line or not:

L = lineal

C = collateral

The dimension of collaterality requires further specification into C = collateral up to fifth degree relatives, and C² for fifth degree relatives, i.e. relatives who are five steps removed from Ego in their genealogical link reckoned through their common ancestor, for instance:



I have based the N/R distinction on the families of origin and procreation only, not adding - as Fischer did in his analysis - the family of origin of Ego's marriage partner. This allows me to single out the husband/wife relationship as the special type of affinal relationship which it is in comparison with all other more remote types of relationship through marriage.

The lineal/collateral distinction is not completely superseded by the near/remote distinction: it remains necessary in order to distinguish, for example, between achterneef and kleinzoon, and to support the distinction between the husbands/wives of aunts/uncles and the parents-in-law. The details will be clear from the sample list of feature definitions given below. My main concern in choosing these features has not been to achieve the greatest possible economy of statement. I have chosen this solution because I wanted to represent the elements which play a part in the way people view and experience kinship. I consider that it

is only from an understanding of that reality that we can hope to come to an understanding of the actual use of kinship terms in a variety of contexts and situations.

- Generation.

On this dimension we need a seven-way distinction in order to account for the terms in Fischer's list, viz.: G3, G2, G1, G, G-1, G-2, G-3.

It must be remembered that generation is used as a social concept: age difference between marriage partners is irrelevant, through marriage they both belong to the same generation.

-- Sex.

Here I will use the symbols 'm' and 'f' for male and female, and 'x' for 'unspecified with regard to sex'.

We are now in a position to define the kinship terms of our list componentially in the form of class-products. I shall only give a limited number of definitions as the rest can easily be supplied either by replacing 'm' by 'f' (or 'x' by 'm' or 'f'), or by changing the index of the generation dimension.

1. vader	m.G1.N.L.K
2. moeder	f.G1.N.L.K
3. vrouw	f.G.N.A
4. kind	x.G-1.N.L.K
5. zoon	m.G-1.N.L.K
6. dochter	f.G-1.N.L.K
7. broeder	m.G.N.C.K
8. zuster	f.G.N.C.K
9. oom	m.G1.R.C.K <u>or</u> m.G1.R.C.A

11. neef	m.G-1.R.C.K <u>or</u> m.G.R.C.K
13. grootvader	m.G2.R.L.K
15. oudoom	m.G2.R.C.K <u>or</u> m.G2.R.C.A
17. achterneef	m.G-2.R.C.K <u>or</u> m.G-1.R.C ² .K <u>or</u> m.G1.R.C ² .K
19. kleinkind	x.G-2.R.L.K
27. schoonvader	m.G1.R.A
29. schoonbroeder	m.G.R.A
31. schoonzoon	m.G-1.R.A

6.2.2. Further Observations Concerning Dutch Kinship Terms.

Fischer's list of kin terms is reasonably complete. There are, however, alternative usages which he does not mention. I will take this point up now. One would not normally replace a term by an alternative regardless of certain social and/or personal factors: these must, therefore, be included in our considerations. Moreover, there are certain extended or metaphorical usages which may reveal something about the value judgements of the people who use them. I would also want to discuss the general term ouders which Fischer does not list. Finally I would like to examine the complete list of prefixes used in connection with kin terms. In my discussion of alternative usage and idiomatic use of kin terms I will follow the order of our list of kin terms (cf. 6.2.1), and I shall concentrate on my personal use and understanding of them as I explained in the introduction to this chapter (cf. 6.0).

6.2.2.1. Kinship terms and their use.

The term vader has a number of alternatives, basically variants of the same form. We find: pa, paatje, pap(p)a, papaatje, paps, pappie. We also know the use of mijn oude heer (cf. English "my old man"). This last usage is either affected or lacking in respect in my appreciation, and therefore to be avoided. Of the forms I mentioned before I would use pappa or the diminutive paatje most often in address. They are more intimate than vader which I hardly ever use in address, with the exception of the context of letter writing. Paatje reflects greater intimacy still than pappa. My sisters make a still wider use of these various forms of intimacy than I tend to do. When I refer to my father to outsiders or remote relatives I will say: mijn vader, but with the immediate family I normally refer to him as pappa. I believe that this distinction is fairly common. It seems relevant in this connection that there is a change with age: children will use mijn pappa (or pappie perhaps which is more typical, though not exclusively, of children's usage), even when referring to their father to outsiders. It is an interesting fact, moreover, that reference to the father within the near family or outside it is connected with the absence or presence of the possessive pronoun, very much like in English "Where is father?" as opposed to "Where is my father?". In the latter sentence the possessive my emphasises the unity of the nuclear family with regard to those outside it. In Dutch - though this may be a regional rather than a general feature - we can use the first person plural possessive pronoun ons with kin term or proper name for reference to other members of the tightly

knit immediate family. The plural expresses the idea of "we-as-a-group", as distinct from all others. All idiomatic or metaphorical use is restricted to the term vader. In some cases it stresses the aspect of 'responsibility for looking after dependents', e.g. vader for the man in charge of a youth hostel or an orphanage, or also the expression Vadertje Staat (Father State). At other times it stresses authority and dignity, e.g. de Heilige Vader (the Holy Father, i.e., the Pope), Vader Cats (a famous Renaissance Dutch poet whose highly moralizing work was read extensively for several centuries because of its edifying qualities). We do not have in Dutch the equivalent of the use of English father for a priest, at least not in everyday usage. Vader is used to address a priest in the context of religious worship with its rather hieratic liturgical language: the emphasis would seem to be on authority and respect though there may be a possible meaning of 'giver of spiritual life' as I have pointed out previously (cf. above 4.4.3). One form of use which does not carry the connotation of authority or dignity is the diminutive vadertje as a familiar, slightly ironical or condescending form of address, e.g. "Dat gaat zo maar niet, vadertje", meaning roughly "You cannot do that, my friend, and get away with it". I find it difficult to explain this usage. I would suggest that the element 'vader' evokes a certain closeness which may well serve to take the edge of what could be impolite, rude, or too harsh if said impersonally, while the diminutive suffix conveys the ironical or condescending connotation.

The term moeder has a similar range of alternatives: moe, moes, moeke, ma, mam(m)a, maatje, mamaatje, mammie. Here again my personal preference goes to mamma and maatje: in this case, however, I would also use it in letter writing. For purposes of reference the usage is the same as for father, i.e. mijn moeder to outsiders and mamma within the close family, while children normally do not yet make this distinction and use mijn mammie throughout. The abbreviated forms of moeder, viz. moe, moes, moeke, may well be in part regionally determined variants, the diminutive form moeke again expressing special intimacy.

The term moeder is used as well in a non-kinship context for the woman in charge of a youth hostel or some institution, or as moeder overste (i.e. mother superior) for the head of a religious community of women. In proverbs moeder stands for source, origin, e.g. "Ledigheid is de moeder van alle kwaad" (Idleness is the mother of all evil). We also speak of "Onze Moeder de heilige Kerk" (Our holy Mother the Church) in connection with the Roman Catholic Church: the idea behind this expression is both the notion of loving care and of giver of (spiritual) life.

There is a very colloquial form moer which occurs in a number of idiomatic expressions, e.g. "Loop naar je moer" (roughly translated: "Get lost"), "Naar zijn moer zijn" (to be beyond repair, said of things). I doubt, however, if the connection with moeder still plays a role at present in these expressions with moer.

The terms vrouw and man are the terms people normally use for wife and husband. If one wants to turn them into terms of endearment one can do so either by suffixing

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lief to man or vrouw, or - in the case of vrouw - by using the diminutive vrouwtje or vrouwke. The diminutive of man, though possible as a term of endearment, is not so easily used for this purpose. The usual connotation of mannetje or manneke is rather one of disdain or disparagement.

In official language we tend to use echtgenoot (Hu) and echtgenote (Wi). To hear these terms actually used as terms of reference gives the impression of (perhaps, slightly outdated) formality or of affectation. Still more archaic are the terms gade or eega (both meaning 'spouse', Hu or Wi) which are now so far removed from actual usage that people will only use them jokingly, with mock reverence. Man and vrouw are the standard terms of reference, and for some people even of address. My impression is that this last type of usage was originally connected with the position of husband and wife at the head of the "extended household" of, for example, the big farms. Just as parents tend to call each other "father" and "mother" within the nuclear family, they may well have called one another by the terms which were linked with their special position in the 'extended household', viz. man and vrouw.

Everything concerned with the legal side and consequences of marriage is qualified by the adjective echtelijk, related with the noun echtgenoot; the word for divorce is echtscheiding.

The term kind refers to either son or daughter, regardless of age. In this sense, for instance, one can say, "Zij hebben vijf kinderen" (They have got five children). Nevertheless, the narrower meaning of 'young child' is probably

more common. It is the one found as well in certain expressions, e.g. "Hij is er maar een kind bij" (In comparison he is a mere child, a beginner), or "Hij is geen kind meer" (He is no longer a child).

Kind is used as term of address: it usually gives expression to either tenderness or to slight impatience tempered by kindness and love. Parents can continue this usage even when the children are grown-up, though the occasions for using it will naturally be less frequent. It can even be used to express tenderness by a husband for his wife, as a term of endearment.

Corresponding with the use of moeder in a spiritual sense for referring to the Church we find kinderen used in a spiritual sense for the members of the Church.

The term zoon is in my experience a term of reference only. Apart from using the first name of course, parents would address their son as jongen (boy), or with greater tenderness jongetje or jongenlief. This is not a typical kin term, however. "Jong!" can be used by anyone to address a young boy (cf. English "lad"); boys use jongens among one another, and one can even hear girls use this form of address among each other when, for example, they are cheering on other girls at sports. This particular use by girls is limited to younger girls. When boys grow up they too, stop using it among one another. However, it always is an acceptable way for adults to address a group of boys, e.g. in class at a primary or secondary school. Parents, too, can continue to address a son as jongen regardless of his age: it evokes, and possibly keeps alive, the special intimacy between parents and young children.

One could also mention the use of jongen as a term of reference. To the question: How many children have they got?, one possible answer would be: "Ze hebben drie jongens" (They have got three boys). In fact, one is more likely to hear this answer than "Zij hebben drie zoons" (They have got three sons). If a parent says: "Dit is mijn oudste jongen" (This is my eldest boy), it sounds more personal - with a note perhaps of parental pride and protectiveness - than when he says: "Dit is mijn oudste zoon" (This is my eldest son).

Very much the same thing can be said about dochter and meisje or meisjelijf. It must be noted, however, that the diminutive form meisje must be used: the word meid has pejorative meaning. Furthermore, as a term of reference meisje is probably less used in the sense of daughter since mijn meisje is the normal way of saying "my girl-friend".

The term for brother is in practice always the shorter form broer. The longer broeder is nowadays used only for members of certain special, non-kinship groups. This can be mankind as a whole ("Alle mensen zijn broeders", All men are brothers), a Church ("Broeders en Zusters in de Heer", Brothers and Sisters in the Lord), a religious community of men, or the like. In a similar sense we find the expression: "Het is een zwakke broeder op school" (He is a weak brother in school, i.e. he is one of those who are not doing too well).

Broer is never used as a term of address (as opposed to the term broeder within the special groups mentioned), though it is sometimes given as a first name.

The case of the word for sister is very similar. The shorter form here is zus. The longer form zuster is used for female members of a Church, and particularly for members of religious communities of women. As many of these religious communities were working in hospitals and in nursing zuster is now the normal term of addressing all nurses.

The respect due to one's sisters can indirectly be inferred from slightly rude expressions: one tries to be more forceful by shocking people through the inappropriate use of terms with a high emotional value, e.g. "Je zus!" (Your sister!, i.e. You are wrong if you think that!), or "Je zal je zus bedoelen" (You must be referring to your sister: a way of expressing incredulity).

With the term for uncle, oom, we go beyond the nuclear family. There is a slightly familiar variant of this term, viz. ome. This variant is also used in the expression "hoge ome" (highly placed uncles), said of people in authority who supposedly care for us but who remain distant all the same. The expression conveys a fair amount of scepticism, albeit good-natured. This resembles the remoteness of real uncles who often do not play a great role in our lives unless they are close to us as individuals. This explains why non-kinsmen can and do fill the role of uncle just as well. It is quite normal for a good friend of the family to be called oom. Seuren (1975: 116) reports the fact that in a certain part of Amsterdam the man a woman is living with will be called oom at first by her children; after about six months they tend to switch to vader. He is neither their uncle nor their

father in a genealogical sense, but the terms translate a social situation of growing closeness. In this same context of a certain vagueness in the use of oom, especially as a term of address, I would like to refer back to my componential definitions. The definition of oom is m.Gl.R.C.K; in the same generation there is an achterneef who is defined as m.Gl.R.C².K. The only difference is the degree of collaterality, but as the term oom is somewhat vague anyhow in its use it is likely that this type of achterneef will be addressed as oom if he is referred to by a kin term at all.

The word tante, aunt, is as vague as its male counterpart oom. It is also used in an expression similar to one mentioned among the usages of the term for sister, viz. "Je tante!" (Your aunt!), mainly used for expressing incredulity. Moreover, an interfering lady or one who tends to cause problems is called "een lastige tante" (an awkward aunt): I believe that we have here the same kind of connotation as we had in "hoge omes", viz. of a person who claims certain rights or a certain amount of attention, while we do not really feel close enough to accept such claims as natural or rightful. The expression "een tante Bet" (an aunt Betty) means a chatterbox: here again we have an example of a critical appraisal being expressed by means of a term which denotes a relative who does not automatically share in the respect and affection, and consequently in the tolerance too, which characterize relationships within the limits of the close family.

As for the term neef it should be said that though Dutch does not distinguish between cousin and nephew

terminologically, there are ways of specifying what one means. If one wants to refer to a sibling's son one can speak of een volle neef (a full nephew). Alternatively we find comzegger/oomzegster for nephew/niece, i.e. 'he/she who calls a given person uncle'; the context must indicate clearly who this person is, either by means of a possessive pronoun used with the term or by mentioning by name whom one is oomzegger of. Complete clarity can obviously be achieved by using compound nouns, e.g. neefs-dochter (NeDa), oomsdochter (UnDa). The fact remains, however, that the term neef is extremely vague. We defined one type of achterneef componentially as m.G-l.R.C².K, while neef was m.G-l.R.C.K, a distinction of degree of collaterality only. In practice they would both be called "neef so-and-so" if the kin term were used at all. Fischer claimed that no in-law qualification is added to neef: his list of kin-types includes, for instance, both BrSo and WiBrSo (loc.cit.: 111 and 106). Schneider (1968: 80) has made a similar claim for English cousin. While I would certainly agree that the prefix schoon (in-law) cannot be used with neef, the adjective aangetrouwd (married-to, by marriage) would be quite acceptable to me.

The only supplementary remark about the female counterpart of neef, nicht, is that this term is used as a slang word for a male homosexual. I believe the expression began to spread ten or fifteen years ago and found its way into the dictionary only a couple of years ago. I am unable to explain this particular usage.

The terms grootvader and grootmoeder came into the Dutch language in the 16th century as translations from

the French. Before that time the prefix used was beste, not groot. I use the terms as terms of reference only, but this usage is not necessarily universal or even widespread. As terms of address I use exclusively opa and oma, words which originated in children's language. This preference is no doubt based on the greater affective connotations of these latter terms.

The expression "Je grootmoeder!" (Your grandmother!) is used very much like "Je zus!" and "Je tante!", mentioned before, colloquial ways of expressing incredulity or similar feelings. An alternative form for oma, opoe, can sometimes be used in an disrespectful and derogatory way when one speaks of "een ouwe opoe" (an old granny).

Before we turn our attention to prefixes, suffixes, and compound nouns I would like to discuss briefly the word for parents: ouders. Fischer included kind in his list of kin terms but not ouders. His reason for omitting the term is the fact that the word is not used in the singular (loc. cit.: 108). Apart from the use of the singular ouder in one or two standing expressions this observation is correct. I believe, however, that the use of this term reflects the idea of the strong unity of the nuclear family vis-à-vis the outside world. When one says mijn ouders rather than mijn vader en moeder one is definitely more formal and one keeps the interlocutor outside the personal relationship, so to speak, by presenting a closed front. When speaking about my parents to close relatives I would never use ouders as a term of reference; very few people would, I believe. One does hear occasionally the familiar de oudelui (the old folks) or de oudjes (the old ones): I suppose that the easy

familiarity of these expressions compensates for the absence of the strong personal feelings which the words vader and moeder convey automatically. The intention of the speaker and the whole context are extremely important, however, for the same expression could be used in a disdainful, insulting manner.

To establish a connection between this point and what I was saying earlier about the use of alternative terms for father and mother, it would seem to me to be significant that in my way of speaking I tend to refer to my parents as vader en moeder when speaking with my brother, but I prefer pappa en mamma when I talk to my sisters.

6.2.2.2. Affixation processes.

We can now discuss briefly the various affixation processes. Generation difference is accurately expressed by a number of prefixes:

- ascending generation: G2 groot
- G3 overgroot
- G4 betovergroot

I suppose Fischer left out the last one because there is no parallel original device for G-4. But the expression betovergrootvader or -moeder is certainly quite common. It is to be expected that there is a term of reference for persons who - though they may be deceased now - have played a role in the family history. It is unlikely, on the other hand, that there will often be a need for a term for the fourth descending generation. The terms of address for all the ascending generations above the second are the same as the ones described in

in the previous section (6.2.2.1) for the grand-
parents, the second ascending generation.

- descending generation: G-2 klein

G-3 achterklein

G-4 achterachterklein

The question of address does not arise here. One uses
either the first name, or jongen/meisje.

- for collateral kinsmen:

oud is normally reserved for the terms oom and tante
though some people use it as an alternative prefix
with grandparent terms in the fourth or fifth ascending
generations.

An oudoom will be addressed as oom.

achter is used with neef and nicht but less to express
generation than collateral distance. In forms of
address the generation factor may appear: one type of
achterneef will be called oom, the others neef, or more
likely by their first name.

To express relations by marriage we have in the
first place the prefix schoon, used exclusively in reference.
In address one copies the use of the marriage partner: here
too the nuclear family operates as a closely knit unit.
This means, for example, that the relative called zwager, i.e.
BrLa, can be SiHu, HuBr, WiBr, WiSiHu, or HuSiHu. Strictly
speaking the in-law relationship does not extend beyond the
consanguineal kin of one's marriage partner: this would rule
out WiSiHu and HuSiHu from the BrLa category. A little used
expression, koude zwager ("cold" brother-in-law), reflects
this fact. Normally, however, one uses the same term as one's
marriage partner, viz. zwager.

Other relations established by marriage are half and stief (step) relationships. The complex terms formed with these prefixes are used in reference only. What way of address is used will depend on each individual case, but there is no doubt that the ideal, most natural situation is considered to be the mode of address of the nuclear family. Due to the isolation in which every family-unit lives any other solution is bound to lead to severe tensions in the long run. This may be different in the case of a pleeg (foster) relationship which by definition is meant to be merely a temporary arrangement.

Spiritual parenthood is expressed by a limited number of terms. Franco-Gallic church language, a form of late Latin, had the terms 'patrinus' and 'matrina' for godfather and godmother. From these the Dutch terms peter and meter were derived. A prefix peet or pete is formed from the word peter. This prefix has a fairly limited application. We find peet-ouders (godparents) and petekind (godchild); the distinction between son and daughter is hardly ever made here. The relationship stresses the element of responsibility for the child's (spiritual) well-being. In Roman Catholic Canon Law this link results in the prohibition of marriage between a godparent and his or her godchild.

The only other terms the prefix peet combines with are oom and tante since those kinsmen were often chosen for this role. Nowadays the tendency is to choose godparents on merit so to speak: the moral pressure to choose from among one's relatives, preferably in turn from among the relatives of husband and wife, has disappeared as the nuclear

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family unit grows towards an ever greater independence and isolation. Non-kin godparents are referred to as peter and meter; they will often be addressed by the godchild as (peet)oom or (peet)tante according to the general tendency which was explained when we discussed the use of the terms oom and tante.

6.2.3. A Deictic Interpretation of Dutch Kinship Terms.

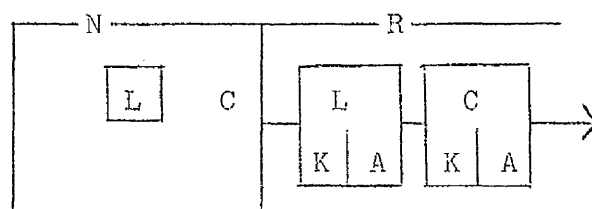
In my conclusion to the previous chapter I defined the aim or teleological function of kinship systems as that of 'situating people' in a network of social relationships (cf. 5.3). In this chapter I have shown how people are situated by means of pronominal deixis in Ego's "Zeigfeld" according to three parameters: distance from "I", direction of deixis, and concentration or intensity of deixis. Using those same parameters I would now like to summarize more systematically the findings of my analysis of Dutch kin terms as an instance of social deixis.

The dimension of the distance away from Ego is basically represented by the distinction between near and remote relatives, i.e. the relatives in the nuclear families Ego is a member of, and his other relatives. It is possible to refine the distinctions along these axes further, especially for the remote relatives. Among these the lineal ones are considered to be nearer to Ego than the collateral ones: lineal descent is an important feature of the Dutch system. In either case consanguineal kin are considered to be nearer than affinal kin.

In the near family such distinctions are less important given the tight unity of this group; there are

indications, however, that these same degrees of distance are valid here too, albeit in a limited way. It is the case, for example, that when the children grow up and start their own families the ties between brothers and sisters become looser to a greater extent and more quickly than the bond between each single one of them and the parents: lineal relationships are nearer and more important than collateral ones. The difference in feeling concerning consanguineal and affinal relations, too, can sometimes be noticed in the nuclear family when, in cases of divorce, the affinal husband-wife relation can be broken off more easily than the parent-child relation.

The following figure sums up the characteristic of 'distance away from Ego':



The dimension of the direction of deixis corresponds with generation. A two-way distinction must be made here: an upward direction (G1, 2, etc.), and a downward direction from zero included (G, G-1, etc.). This distinction is supported by differences in the way of address and, to a lesser extent, of reference. For upward relations one uses kinship terms in address as well as in reference. For zero generation or downward relations one uses the proper name or terms of endearment in address; for reference one uses kin terms, though the use of a proper name with a possessive pronoun is fully acceptable. It must be noted

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here that the term neef is ambiguous with regard to this dimension: as a result the term is deictically extremely weak and vague.

The last parameter is the concentration or intensity of deixis. In our study of the pronouns (cf. above 6.2.2.2) we have made a distinction between focus and diffusion. When we apply this distinction to kin terms we see first of all a confirmation of our division of relatives into near and remote relatives. When the deixis is concentrated, a kin term indicates one relative, or at the most one kind of relationship, only; Fischer spoke in this connection of descriptive and semi-descriptive terms as we mentioned earlier (cf. 6.2.1). When terms covered more than one kind of relationship, e.g. oom, he called them classificatory. I consider these terms to be cases of relaxed or diffuse deixis. The deixis gets more diffuse when it moves from the lineal to the collateral relatives. Within the area of concentrated or intense deixis distinctions can be made as well. Here, however, they cannot concern the number of relationships involved as the terms of this range stand for one type of relationship each; in this case it becomes a question of degrees of emotional intensity resulting in a variety of alternative usages (e.g. vader, pappa, paatje, etc.).

These dimensions determine the value or moral meaning of our kin terms, they provide the coordinates on the chart of our social behaviour. But the points of reference they create are still unoccupied, empty. Not empty in the same sense as pronouns for, as nouns, they carry a much more precise meaning, but empty in the sense that they

have yet to be put to use; and in that process we can use them tactically to serve our basic aim of building up social relations by situating people at their appropriate place, appropriate according to the situation and to our intention. These points of reference are carved out, so to speak, in genealogical material, but according to social specification. If our intention is to refer to genealogical positions, then the terms will be genealogical; if not, the genealogical base merely serves to carry a different tactical meaning.

The genealogical terms require precision: this is provided for by the processes of affixation. However, the genealogical meaning is not the most important one from our present point of view. We are examining how social relations are established between people, and, more particularly, how language operates and is used in this connection. In my opinion this is done primarily in verbal exchange, through the system of address. It is only when we speak about our social relationships that the genealogical basis gains in importance. The two systems, of reference and of address, are both essential aspects, however, of a full linguistic study of the way we situate people by means of words.

The tactical meaning which complements the basic value of the kin terms determined by their deictic dimensions will provide specification on two points.

First of all it will specify which referents can fill the various positions in the social network. There are different degrees of flexibility in this respect. When the deixis is focused sharply it is directed at single individuals. In the Dutch kinship system one can only have

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one vader and moeder, man or vrouw. The uniqueness of sons, daughters, brothers, and sisters is safeguarded by addressing them by their first name only, never by a kin term. No individual can move into any of these positions unless the basic unit of the nuclear family is adapted officially according to socially and legally accepted regulations, e.g. by marriage, re-marriage after divorce, adoption, etc. It is possible, however, to use the basic kinship values tactically in other social institutions, e.g. Churches, where vader, moeder, broeder, and zuster are used in a related, non-genealogical meaning. In as far as the genealogical base is not fully congenial to this context one is justified in calling this use a case of metaphorical meaning. As the focus relaxes it becomes more and more a matter of a personal decision which referents are possible for a certain position. There are no specific regulations for such decisions, and they do not have any official social consequences. All that is required is that the person is of the right sex and age-group, and lives up to the normal expectations of a given kinship role, and - most importantly - that both Ego and that person accept the relationship. The possibilities are still fairly limited in the case of lineal relatives: they become much wider, however, with collateral ones, especially oom and tante.

The second point which the tactical meaning will specify is the question of alternative terms for one single referent. This is a matter of intensifying emotionally the deictic focus by a speaker's choice of a term depending on context (reference or address; who am I speaking to; who is listening in; written or spoken language; etc.) and on

intention (various kinds of speech acts, etc.). Greater intimacy is achieved by using terms derived from children's language, e.g. pappa, etc., mamma, etc. The same effect is sought by the continued use of terms like kind, jongen, meisje, regardless of age: one aims at recalling or keeping alive the bond of deep feelings which exist, as a matter of course almost, as long as a child is young and dependent.

The use of diminutives is particularly apt for expressing familiarity and affection: calling a person or a thing by a diminutive noun makes them very much our own. Affection can be expressed explicitly by the suffix lief: this device, and the use of a diminutive form, are the only ways of manifesting one's feelings through the choice of terms for more remote relatives.

A number of linguistic facts highlight the strong emotional unity of the nuclear family. There is the general practice by husband and wife of adopting one another's relatives, frequently to the extent of imitating the idiosyncratic ways of address or reference used in the family of one's marriage partner. Next, I would see the pair of terms man and vrouw as opposed to echtgenoten as words which are at the same time more intimate, less formal, and express the living unity rather than the legal, institutional side of marriage. It would seem to me, moreover, that the habit of parents of calling one another vader and moeder (or their alternatives) in front of the children can, at least in part, be seen as an expression of the unity of the nuclear family. At an early stage in the children's lives this may have something to do with taking their point of view and using the terms ostensively rather than denotatively.

During that same period, however, parents talk to their babies in the third rather than in the second person; this they discontinue after a while, whereas calling one another vader and moeder continues as long as the children live at home and possibly longer. I would suggest that this can be explained at least in part by a desire not to stress too much the difference between the parents on the one side, and the children on the other. By using vader and moeder almost like names, the younger generation can address their parents in a way similar to the one they are addressed in, viz. by name only. The politeness expected from the children is reduced to its minimal manifestation in the use of the pronouns of politeness with the corresponding plural verb endings: the required distance is maintained. Another factor expressing this intimate unity is the special use of the possessive pronoun ons. One of the children in a family could say both ons vader, to refer to the father in the presence of other members of the nuclear family, and, for example, ons Piet, to refer to one of the male children in the family.

Deixis would seem to be more easily emotionally intensified when male speakers address women, or refer to them, or refer to others in their presence. The expectation of deeper feelings in connection with the use of female terms by men explains why it is such an effective way of being rude when one uses them out of context or in a derogatory way.

I believe that it is significant for the very personal character of the intensified deixis of such alternative usages as "pappa", "mamma", etc. that they are

never used within metaphors. Whatever metaphors and expressions we have found were based on the standard, more formal, or at least more neutral terms. The emotional implications of the alternative terms are simply too strong to allow their use in a different context.

The solution here proposed does not give one, unequivocal answer to the question "Who calls whom what?", for the simple reason that there is no such answer. The system is used differently by different people, but always according to its inherent possibilities and in line with its basic function. What I have tried to describe is the mechanism of this system and the way in which people, by using its possibilities judiciously, succeed in realising its basic function: establishing precise social relations between each other.

7. Conclusion.

It would seem that our study has established several points. The claims for the explanatory value of the historical approach in the late 19th and early 20th century are not all acceptable but the more prudent and balanced views of recent representatives of the historical comparative school of linguistics show that there is room for some form of historical approach. However, the fact that theories in this field cannot always be verified in detail means that their validity does not rank as highly as one might wish. The problem is not unfamiliar to philologists; it merely becomes greater when we set our sights on the reconstruction of language and culture, or, more accurately perhaps, of language as the reflection of a lost culture. Nevertheless, the basic issues stood out clearly from very early on: the genealogical basis of kinship systems, the use of terms in reference or address, the importance of social factors, the role of psychology, and an interest in cognition.

As in linguistics, the historical approach was pushed into the background by the structural or functional approach. While the type of explanation thus changes, the basic issues remain very much the same but now reformulated and supported by more and better evidence. Some of the contributions here were very restricted in linguistic insight: specialization is growing. Each theory tends to stress certain aspects and on the whole many partial and valuable insights are gained. None of the theories, however, would seem to qualify as the one and only, the definitive answer.

There was a growing feeling that more rigour was required in order to arrive at verifiable statements. Similar developments within linguistics in general provided the means for these more formalized types of analysis. The question of correlation between terminological and social systems is not completely forgotten but the main effort goes into internal analysis. If anything, it is interest in psychology and cognition which tends to become dominant rather than concern about the social structure. On the whole, however, the conclusion has to be that most of these analyses are satisfactory judged from the point of view for which they were devised, or as part of a wider linguistic or anthropological framework. But the multiplicity of different analyses, often carried out on the same data, throws considerable doubt on the possibility of establishing a clear, direct link between a type of terminology and a form of social organization, or of cognitive structure.

In my evaluation I believe to have shown that if we want to break the dead-lock (if it can be done at all) we have to rephrase our question. There did not seem to be any point in expecting an answer to the question of the correlation between the linguistic and social aspects of kinship in terms of efficient causality. Consequently we decided to settle for a solution based on final causality, specifically the aim of "situating people".

In our last chapter we developed this idea in the form of a deictic theory and analysis of the linguistic aspects of kinship. We were able to indicate certain legal and social factors which seemed to confirm our interpretation

of Dutch kin terms. The main advantage of this approach is its flexibility. One is no longer obliged to force elements into a theory, or to admit to a number of baffling exceptions. This flexibility may entail a certain loss of formal precision in the analysis. It seems, however, to be a very adequate way of capturing the intricate and delicate relationships between language and culture in the domain of kinship. It provides a framework but is not deterministic: man uses the available possibilities in order to achieve the basic aim of establishing one's relative position vis-à-vis another person on the basis of, or in parallel with, the genealogical network.

This proposal needs without doubt to be tested further. I can see two ways of pursuing fruitfully the findings of this study. A first way would be to apply the same form of analysis to several languages. The main advantage to be gained would, I believe, be a better insight into the tactical part of the meaning of kinship terms. For the moment I would be inclined to expect that there may well be a fundamental similarity of system in this respect between languages - with, however, differences of emphasis corresponding to the place kinship holds in the general scale of values in a given society. It would be necessary, however, to ensure that the data for each language cover the whole range of the use of kinship terms, not merely their use in genealogical classification:

Another point to pursue further is the place of the deictic approach to language. Though I see no objection to developing an analytical technique specifically for

dealing in depth with a single, particular linguistic problem, one would hope to be able eventually to extend its application to similar problems. The things which come to mind here are the whole of the pronominal system, expressions for time and place, parts of the vocabulary which play a role in the social structure, and perhaps vocabulary in general in as far as its use is socially determined (e.g. in social dialects) or emotionally coloured.

I would hope that a cross-cultural comparison based on analyses of this kind would lead us to establish basic universal values and tactics, while the linguistic relativity would be expressed in the particular configuration of strategic use in each language. It may eventually form the basis for an approach to the problem which started me off on this track: how to translate correctly with full respect for both the source language and the target language, and for their associated cultures.

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