B. THEORY AND METHOD

One of the epistemological assumptions of the subsequent analysis is that many features of the observable and tangible realization structures of a society are comprehensible on the basis of, and reducible to, the more simple basic structures, which are hidden and inaccessible to the senses (Sperber 1975:17-50). It has been suggested that the study of the hidden structures might be the common ground on which a number of related sub-disciplines could meet (Adams 1977:265; Campbell 1969). Indeed, the significance of the emic classification of various people has been documented among a number of societies.[1]

In the following I shall briefly discuss one such discipline outside anthropology, viz. general linguistics, where the study of deep structures of language has been a central issue for a number of years. Then I shall very shortly outline the historical development of symbolic anthropology, which is the theoretical basis of this study.

6.   THE SEARCH FOR HIDDEN STRUCTURES IN LINGUISTICS

Much interest has been shown and effort given in recent years to the elaboration of this hidden level of consciousness. Yet, remarkably little has been done to give substance to these considerations by means of analysing concrete societies within the framework of this paradigm.[2] Lévi-Strauss tried to substantiate his theories with a wealth of ethnographic data, but this he did cross-culturally rather than by drawing on a single society. His concern was universal rather than particular, and he tried to reveal the universal laws of the working of the human mind.

Within general linguistics, and particularly within one of its branches of discourse, namely generative linguistics, the distinction between

superficial and underlying structures is explicitly made (Gregersen 1977:92). In it, common features of languages are emphasized at the cost of 'surface' phenomena (Haugen 1977:11). On the other hand, there has been debate on whether these general structures can be revealed by studying languages cross-culturally or by concentrating on a limited range of languages, or even one language (Comrie 1981:228-32; Haugen 1977).

Although this is not a linguistic study, linguistic categories are utilized in analysing thought structures, as well as social and economic orders. As Chomsky and others have argued, a people's 'thought' is coded in language; i.e. mental categories are reflected or realized in language (Chomsky 1957, 1968; Fox 1979:139; Gay 1981:113). Therefore, it should be possible to establish corresponding features between idiosyncracies in a people's language and the mode of thought that has produced that language. Although the Sapir-Whorf hypothesis[3] in its 'strong' form (Lemon 1981:201-02) of the correspondence between a people's language and thought patterns may have few supporters today (Haugen 1977:11-12), it has some relevance in studying societies with a relatively independent and dynamic cultural history. Although language does not directly determine its speakers' way of thought, as the hypothesis proposed originally by Edward Sapir and reformulated by Benjamin Whorf suggests, the interdependence between the lexical and grammatical structures of a people has not been seriously questioned (Lemon 1981:201-02; Haugen 1977:20-23).

In the subsequent analysis I hope to show that there is a certain degree of consistency between linguistic structures and modes of classification in Parakuyo society. This consistency is rooted ultimately in the properties of the mind, which is not, however, conceived as a self-contained entity.

The term 'mind' is not used here in the sense of a 'collective mind' or 'collective consciousness' either (Durkheim 1912), because this would be something too conjectural to be proved. The mind is the inner thought mechanism of individuals, but the ways how these mechanisms work in individuals are directed, or at least influenced, by the

shared system of ideas what we call culture (Steward 1972:37). Here
we are dealing with the question of how much our perception is "in-
fluenced by culturally constituted experiental factors, schematic per-
ception, involving the meaningful aspects of experience" (Hallowell
1977:132). It seems clear that the way how we perceive our surround-
ings is not isomorphic in a cross-cultural perspective. Culture, and
language as its embodiment, directs our perception so that we concep-
tualize our observed and sensed surroundings in a schematic way
(Hallowell 1977:131-32) and it makes us perceive units where there is
in fact only a continuum. Culture guides us to select certain objects
and phenomena from our surroundings and to endow them with meaning,
while others are left unnoticed. There are indications that this is
sometimes done with extreme schematic rigour (Lebeuf 1977; Kamau 1977);
so that facts fit the scheme only partially. There is certainly variation
between societies in the degree to which cultural schemes, patterns,
themes or whatever we wish to call them, direct the perception of
individuals. Schematization seems, nevertheless, to be a universal
phenomenon. In fact, the propensity to perceive one's surroundings in
a spatially ordered way seems to be characteristic also of the analytic
behaviour of the eye/brain (Fletcher 1981:108). Again, this propensity
is universal but the actual modes of realization are idiosyncratic de-
pending on the 'culture type' in question (Steward 1972:23).

7.   ROOTS OF SYMBOLIC ANTHROPOLOGY: FUNCTIONALISM

Although symbolic anthropology is theoretically closer to structuralism
than functionalism, the latter has clearly had an influence on it. In
functionalism[4] (developed within anthropology mainly by Bronislaw
Malinowski and his students, e.g. Raymond Firth, E. Evans-Pritchard,
I. Schapera, Lucy Mair, Audrey Richards, and Meyer Fortes), the ana-
lytic entity has normally been a concrete society or culture in all its
aspects. Although the appoach of Malinowski was, terminologically at
least, cultural rather than sociological (Honko 1972;39-41), his theories
were based on the field material collected from a living society in the
Trobrian Islands. Consequently he operated within the framework of
that society and analysed its constituent parts in reference to other

parts of that society.

In symbolic anthropology the starting-point is similar to that of anthropological functionalism, although the questions posed are different. Concreteness and relevance to 'facts' is ensured by studying concrete societies holistically (Ovesen 1979:1). All significant aspects of the society are taken into scrutiny, and nothing should be neglected a priori as irrelevant.[5] Concreteness and holism are features where functionalism and symbolic anthropology find common ground.

Another proponent of functional theory, but not a student of Malinowski, was A. Radcliffe-Brown. Particularly in his early works, e.g. in "The Andaman Islanders" (1908)[6], he tried to establish the relation of each custom of a society to other customs and also to its system of ideas and sentiments. The problems to be studied were primarily psychological and sociological in character, and the ambitious aim of the Andamanese study was to expose the thought patterns of the society (Radcliffe-Brown 1933:230). This has already some affinity with the scheme of inquiry of cognitive and symbolic anthropology, the problems of which came to be formulated decades later.

The ahistoricity, of which both Malinowski and Radcliffe-Brown, and other functionalists, were criticized, has to be understood in historical perspective. Functionalism emerged during a time when it was customary to explore the problems of the 'origin' of various aspects of human culture. It was a time when the spread of certain cultural features was studied by inferring from similarities of products of material culture in spatially distinct areas. Similarities were taken to prove earlier historical contact without sufficient proof from other sources. Against this questionable 'historicity' and comparative method (without adequate material for comparison) the functionalists laid the model of contemporary research of living societies by means of intensive field research, where a feature was explained with the aid of other features in the same society. The negative view of Radcliffe-Brown regarding history, for example, has to be understood in this perspective (1933;230). The significance of history has been recognized also within functionalism (Honko 1972:49), and Radcliffe-Brown lapsed sometimes into taking re-

course to history, even to the history of 'origins'. He could not, for example, resist the temptation to formulate a general theory of the development of dancing and singing. The Andamanese practice was taken to represent the earliest stage in the development of song, which evolved along with rhytmical dancing (Radcliffe-Brown 1933:247).

Although the approach of Radcliffe-Brown was functional even to the degree that he compared the society to an organism[7] and dismissed such terms as 'dysfunction' as irrelevant, his later works show overtones of 'structuralism'. What he saw in the term 'structure' is quite different from what it came to mean in French structuralism. For Radcliffe-Brown, structure means merely "a set of relations amongst unit entities, the continuity of the structure being maintained by a lifeprocess made up of the activities of the constituent units" (Radcliffe-Brown 1952:180). For him structure is coterminous with social organization, and the relation between structure and function is expressed as the functioning of the structure (Radcliffe-Brown 1952:179,184).

With Radcliffe-Brown we are still far from the kind of questions posed in structural analysis today. Whereas the structuralism of Lévi-Strauss is, as Leach has pointed out (1971b:25), mathematical-logical in approach, the concepts of Radcliffe-Brown were biological-functional. He never searched for the underlying deep structures which occupy many modern anthropologists. Yet, he was not totally unaware of their importance (Leach 1971b).

8.   ROOTS OF SYMBOLIC ANTHROPOLOGY: STRUCTURALISM

Arnold van Gennep, a contemporary of Radcliffe-Brown in France, made his name permanently known with his classic work "Rites de Passage" (1909). From the viewpoint of the present study its main contribution is its lively awareness of the fact that man cuts the continuous flow of events into units. Instead of allowing time to flow freely and man to pass along smoothly to the very end, he lives as if life were composed of steps, or statuses as we would say today[8]. Van Gennep viewed society as being composed of a number of social groups through which

individuals pass and acquire respective social positions. The moments
of passage from one position to another are emphasized and publicized
through ritualization and often by lengthening the ambiguous liminal
period in-between. He noted that not only passages in a people's
life cycle, such as birth, social puberty, marriage, fatherhood, mother-
hood, occupational specialization and death are subject to ritualization,
but also "celestial changes, such as the changeover from month to
month, from season to season, and from year to year" (van Gennep
1960:4). Although van Gennep did not use such terms as 'deep struc-
ture' or 'the property of human mind', his argumentation implicitly
postulates their existence. The extent of data which he collected to
substantiate his views indicates that he was trying to demonstrate
something common to all mankind. Also he tried to prove that human
experience is composed of entities and that life is full of transitions
from one entity to another. In van Gennep, structuralism was already
in an embryonic form.

With Lévi-Strauss, anthropological structuralism which had been operating
more or less on the same level as functionalism (Leach 1971b:13-25);
Glucksman 1974:15-46) gained depth and extent. Because his structural
analysis has been discussed exhaustively by several authors[9] and be-
cause I shall refer to him from time to time, there is no need to go
into more detail here.

9.   TOWARDS SYMBOLIC ANTHROPOLOGY

Structural analysis in the sense used by Lévi-Strauss has laid empha-
sis on the nonobservable and often unconscious mental structures of
the human mind. By pointing out these logical structures as decisive
and primary structures, out of which social institutions and other ob-
servable forms of behaviour are shaped, the structuralists have arous-
ed the anger of functionalists and other empiricists. To the criticism
of ahistoricity and non-empiricism, Lévi-Strauss has repeatedly an-
swered by emphasizing the significance of history in the analysis of
the evolvement of different types of social formations, and by start-

ing from ethnographic data in the analysis of kinship and mythology, for example, and by returning to them as final proof of the analysis (Lévi-Strauss 1973:9-10). But despite these attempts at empirical verification the main interest of structuralism is directed at mental structures.

Among those, who were not contented with the culture-boundedness of functionalism nor with the generalizations and non-empiricism of structuralism, discussions have been initiated concerning new directions in anthropology. Since fitting together these methods into 'structural-functional' analysis has not been easy due to the different units of analysis and different concepts applied in them,[10] a need is felt among a number of researchers to seek for a new theoretical approach, which is here called symbolic anthropology (Schwimmer 1978).

This new approach is at the same time more specific and more comprehensive than the traditional inquiries. In these approaches, which are variedly termed 'symbolic anthropology' (Schwimmer 1978:VIII), 'semantic anthropology' (Crick 1976:2), 'post-structural anthropology' (Hastrup 1978) etc., an attempt is made to work in carefully chosen minute areas, in order to achieve specificity. Functionalist errors are avoided by letting the analysis penetrate through all the structural levels of the cultural system (Schwimmer 1979a, b; Ovesen 1979:4). Crick emphasizes, that this new approach, which he calls 'semantic anthropology', is something totally different from the mere combination of the functionalist and structuralist inquiry. Emphasis has shifted from function to meaning. This shift is based on the assumption, that a characteristic feature of a human being is his capacity to define himself and to create meanings for whatever he does or thinks. This new position is qualitatively so different from the old paradigms, that it is held to be justified to talk of an epistemological break between the old and new (Ardener 1971b: 450; Crick 1976:3-5; Ovesen 1979:2).

Crick criticizes functionalism, which grew out of general scientific positivism, and was therefore forced to leave important areas of social reality unexplored (1976:5). Anthropological investigation in Great Britain has traditionally been the subject matter of social anthropology, and

this close attachment to the social sciences has given shape to inquiries, where linguistics and semiology as a whole have hardly had any place. French structuralism, although methodologically relying heavily on linguistics (Crick 1976:37-38), is basically claimed to be, however, antisemantic (1976:9-10, 56-58). Crick finds nowhere adequate understanding of the primacy of language and semantics, except in the early writings of Saussure where the need of a general science of signs was expressed (Saussure 1931; Crick 1976:10-11). It was already Saussure's notion that linguistics and semantics are not coextensive, and that the realm of semantics is far broader than that of linguistics, the latter being only a subsection of semantics. However, explorations into the field of semantics have regularly been of only one sign system, that of language, and also these have been of a rather technical nature. In social anthropology, at least, language has been primarily in instrumental use (Ardener 1971a:XIII-XIX), a technical device for acquiring information.

Semantic anthropology postulates language and the whole semantic field as dominant characteristics of the human being; man does not only communicate meanings, he creates them. If Crick's claim is true that "the social life of human beings is thoroughly linguistic" and that "language is the most social of all institutions" (1976:6-7), social anthropology has to exceed its traditional boundaries and include the whole system of signs into its scope, although no 'grammars' or 'codebooks' of symbolic communication exist as yet (Leach 1976:9-10). If it was through acquaintance with linguistics that Lévi-Strauss was able to bring structural principles into an anthropological form, it is semiology that leads us to the understanding of the central but largely neglected areas of culture: i.e. symbols, signs and signals (Leach 1976:4-5). These concepts are not mere curiosities, marginal features alongside more central categories, such as kinship, political system, mode of production etc.

These semiological terms remind us of the difficulty of transmitting meaningful messages through ordinary language. If this were the case, far less symbolic representation would be needed. They also point to the crucial notion that there are hidden structures (Eisenstadt 1975:428) aside from or rather behind realization structures, and it is precisely these structures that are the source of information in symbolic communication.

Although these structures can be detected only indirectly, through real-
ization structures, awareness of their existence is crucial for the analy-
sis of the 'total phenomena'[11].

10.   STRUCTURAL LEVELS OF ANALYSIS

In order to elucidate further the significance of the points emphasized
in symbolic anthropology, I shall refer to Ardener's model of the rela-
tionship between structural levels. Although, at least theoretically, sev-
eral structural levels can be assumed to operate simultaneously in soci-
eties, at least two levels can be usefully isolated as basic structural
entities. These can be called **paradigmatic** and **syntagmatic** structures,
as many recent antropological studies indicate.[12] Because of confusion in
the use of these terms Ardener has tried to clarify their content (1971b:
458-59). I am using them in the meaning proposed by him.

Syntagmatic structures (realization structures, or shortly s-structures)
are those observable realizations which we often call institutions, events,
and the like. The anthropologist is foremostly dealing with these struc-
tures in his fieldwork. Kinship, political systems, stratification, econo-
my, aspects of the social system, religion etc. are the classic topics of
anthropology dealing with s-structures. Since Durkheim and Mauss there
has been an awareness of other, deeper, levels of reality, but function-
alism did not penetrate beyond s-structures, and multi-level analysis
had to await structuralism, and particularly the insights of symbolic an-
thropology.

The other structural category, the **paradigmatic** structures (template struc-
tures, or shortly p-structures) are those basic logical and mental cate-
gories, which Lévi-Strauss was searching for, and whose existence has
been postulated by several analysts in a more or less explicit way. Al-
though non-observable, the p-structures are of central significance in in-
vestigation, because these structures are like an input, a programme,
which results in s-structures. P-structures belong to the same category
of concepts as 'deep structure', 'hidden structure', 'ground rules' and
'order of orders' (Eisenstadt 1975; Lévi-Strauss 1972:312-15). P-struc-

tures and s-structures belong to two different levels of abstraction and they are by no means synonymous. The confusion of categories is partly due to unawareness of this basic fact. Therefore, a few misunderstood categories may be classified:

paradigmatic	syntagmatic
opposition	conflict
reciprocity[13]	transaction
alliance	descent
prescription	preference
programme	output

The corresponding pairs belong to the same 'family' of concepts, but they cannot be connected by direct equation, because they belong to different conceptual levels.

What is the relationship between paradigmatic and syntagmatic structures? The following illustration may be helpful, although we have to make some improvements to it later:

Fig. 1. Paradigmatic and syntagmatic structures.
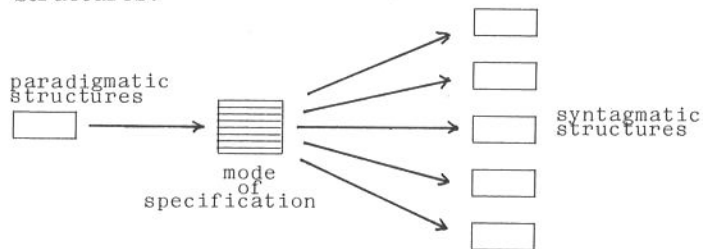
In this illustration, the p-structures are in fact the basic mental structures of the human mind, which are logical in type, and do not show much variation in their distribution through space and time. This is, in a sense, an a priori statement, but also structural studies indicate that the basic properties of the human mind are qualitatively similar in all populations (Lévi-Strauss 1972:230; Turner 1974:3).

Logically, all p-structures, being similar allover, and very limited in

number, should generate equally similar s-structures, if ecological conditions are kept constant. The great diversity in s-structures within different societies makes one suspect that there has to be a kind of modifier, a 'black box' as Ardener calls it (1978:109; Hastrup 1978:130), which functions as a sort of selector and specifier in the particular realization process of the elements contained by p-structures. S-structures are those institutions, events etc., which have been initiated by p-structures but modified and specified by what we may call culture. It is each people's culture that principally directs the choices offered by ecological conditions and basic capacities.

Culture does not only direct and shape s-structures, but it also influences our perception. The images transmitted to our minds are shaped, and distorted, by our own culture precisely because of the specifying nature of culture. This causes a double difficulty in the analysis of events and makes the 'objectivity' of anthropology highly problematic.[14] First, members of society do not perceive and comprehend events objectively as they are in reality; the facts and the comprehension of them are shaped by culture. In other words, people see the 'facts' of their own society through cultural lenses, which colour reality. Secondly, the anthropologist sees the same events, classifying and interpreting them through his own cultural code. Therefore, the idea of 'bare facts' is an illusion, which in reality does not exist. What we have to deal with are the images of facts conditioned by the coloured filters of the culture.

In this structural model of society, culture is given as a mysterious 'black box', which is of central significance in its double function of shaping and comprehending events. Despite its regulating property culture does not, however, have an independent existence outside social structures. Culture is influenced by environmental determinants, but its changes take a long course, and therefore culture represents continuity and cognitive integrity in the everchanging stream of events. In other words, culture arranges and classifies the material according to the cultural code. It defines the principles according to which choices are made. It is not possible to predict through a purely logical inference what plants, animals, minerals and natural phenomena a society chooses to endow with meaning (Lévi-Strauss 1974). It is the task of anthropology

to identify these choices empirically in order to make the defining prin-
ciples of the culture explicit.

In addition to the concept 'culture', also the terms 'world structure'
(Ardener 1978:118-19) and 'global structure' (Friedman 1974:445) have
been introduced to signify the relations between p- and s-structures. It
is the frame for conception and action as well for individual people as
for societies. In fact, the 'world structure' of an individual is usually
homologuous with that of his society, at least in significant aspects.
There is a certain duality in the world structure, because individuals
are at the same time elements of the system and the communicating be-
ings themselves (Ardener 1978:119). People and messages, and the re-
ceivers of these messages are they themselves. This communication system,
as we may call it, is to a high degree homeostatic and selfcontained.

11.   ECOLOGY, HISTORY AND CAUSALITY

The structural model proposed above is, however, defective in three im-
portant points. Although it clarifies the relation between p- and s-struc-
tures, it ignores the ecological and historical variables on one hand, and
the interdependence of culture and s-structures on the other. As far as
the model is intended to clarify the relation between p- and s-structures
from the individual viewpoint, the ecological and historical aspects need
not be important. But if it is meant to illustrate relationships on the
social level, that of groups and societies, ecology and history cannot be
bypassed.

Trying to avoid the extremes of cultural and ecological determinism I
have attempted to show (Fig. 2) that the ecological constraints effective-
ly frame the number of choices in economic adaptation. In addition to
economy, the ecological determinants influence also other aspects of cul-
ture.[15] The degree to which ecological constraints exert influence on
culture and social structure varies greatly. On treeless grass savannahs
with insufficient annual rainfall for agriculture the only choice may be
pastoralism. In other areas with tsetse-infested bush and tree growth
and soils too poor for agriculture,the only real alternative may be hunt-

Fig. 2. The influence of ecology and history in the formation of structures.

ecology

p-structures

s-structures

history

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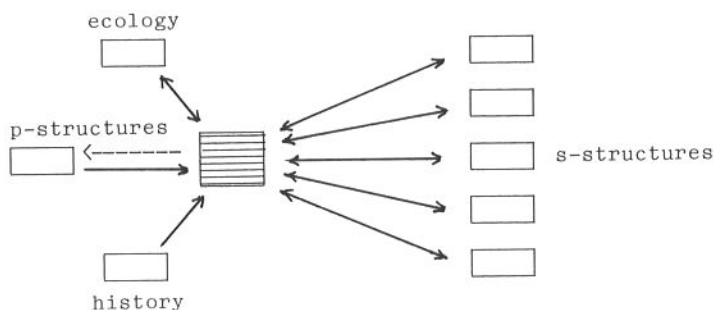
ing/gathering. In these areas the influence of ecological constraints is strong. But there are areas with such natural conditions which would allow more than one type of adaptation. In such cases the question of choice arises. It is also obvious, as Bonte has pointed out, that man may significantly influence the ecology to his advantage (or disadvantage). Pastoralism has been, for example, an important chain in producing vast grass savannahs of East Africa, which support large populations of wild animals and livestock (Bonte 1981:34, 42; Jacobs 1975:40).

In the cases with more than one viable choice of mode of economy the significance of culture is particularly emphasized. An ethnic group inhabiting such an area may develop 'sections' which specialize in different modes of economy according to the possibilities provided by the available niche.[16] Another possibility is that the area is inhabited by different ethnic groups, each with its own specialization in ecological adaptation. Thus, pastoralists, agriculturalists, and more seldom hunter/gatherers, each of different ethnic origin, may be found in a limited area. Adaptation in a multi-choice situation has been guided by factors, many of which can be traced to culture.

Here we come to one of the most central problems of this study. It is: In which way and to what extent does the culture of a people guide, or determine, its adaptation to its niche? What does it mean when we claim, for example, that the choice of pastoralism as the basis of econ-

omy is culturally constituted or that the choice of agriculture of another ethnic group in the same niche is guided by the culture of that group? Why does it seem as if there is no real choice and the 'choices' seem selfevident to the people?

Here it is necessary to emphasize the significance of **history**, and indeed the long historical process, in molding the culture into its integrated form. History is not, of course, such a limiting factor as ecology; it is rather an important dimension to be considered in making the cultural (and also societal) forms intelligible. The cultural forms need a long time to develop. Therefore, what we observe in present day society is a stage in a historical process, which is normally very long and the origin of which is unknown. For understanding the cultural features of the society it is important to know in what kinds of conditions, in what sort of niche, and in contact with what kinds of people, the society has lived the significant stages of its existence. Historical perspective will also help to understand in what kinds of niches the society has been living before it moved to its present area. This historical knowledge enables us to understand later choices in ecological adaptation as well as the multiple cultural and social features which are in a way derivatives of the adaptive choices (Bonte 1981:34).

The relationship of ecology and culture seems quite different depending on whether we see them from the historical viewpoint or not. Viewed from an ahistorical standpoint it may seem as if ecology would have primacy over culture and societal forms. The historical perspective, on the other hand, makes it clear that there has been a series of ecological choices, where the society has purposefully sought such ecological conditions where the already established form of adaptation (economy) could be continued and developed. Economy and culture have become interlinked in many ways and the culture seems to require a certain type of economy and hence ecological conditions which facilitate it. Therefore, what seems an independent variable from one viewpoint is dependent from another.

The third point to be added is that the relationship between culture ('black box') and s-structures is two-directional. Although the s-struc-

tures are essentially the outcome of the p-structures modified by culture,
this influence is neither one-directional nor mechanistic. Culture and s-
structures are interrelated in such a way that the former (in its widest
sense) covers virtually all the s-structures, creating coherence between
them. However, culture is more than the sum of the observable s-struc-
tures; hence these cannot be equated. Two-way interdependence is shown
by twin-pointed arrows (Fig. 2).

The above modifications have been made to the model to account for all
significant variables. In the model, ecology and history have not been
placed in the same category with p-structures, although they all provide
input for the mode of specification. Ecology and history are intervening
variables of a kind in the 'process' where p-structures are transformed
into s-structures.

What makes symbolic anthropology particularly challenging is the attempt
to state something significant of p-structures. The basic difficulty is
caused by the fact that they are not open to the senses; they are intan-
gible, nonobservable. They can be grasped only indirectly, through the
realization structures (s-structures). Because the realization between the
two structural levels is not direct but 'mediated' through the 'black
box', the procedure is doubly difficult. Also the role of ecology might
cause problems in identifying what is truly paradigmatic in structures.

The procedure has to start from the analysis of the realization structures.
All central structural elements on this level should be analysed, because,
according to the model, the p-structures leave their imprint on all real-
ization structures. The comparision of the structural features of the s-
structures should reveal the principles of ordering, i.e. the p-structures.
The procedure is the opposite of what it should logically be, but there
is no other way than to proceed from the tangible and observed to the
hidden and unconscious, although we ourselves, i.e. our minds, are the
initiators.

Another difficulty is caused by the fact that p-structures are on the
level of abstraction where communication through ordinary language is
difficult.[17] Even though ordinary language does not fully correspond to

what should be described, it can be used for 'circumscribing' the p-structures (Ardener 1978:106–08).

Although the procedure of the analysis is from concrete to abstract, from the s-structures to the p-structures, it is not in all phases empirical. The analyst cannot rely solely on the 'hard facts' of the syntagmatic level; otherwise he would be doomed to operate on that level only. There is a need to make use of 'intuitive reflection', a concept that has been repeatedly referred to recently.[18] The intuitive method has to be used cautiously, because the dangers of arbitrary conclusions are imminent.

It derives from the above that the viewpoint has to be holistic (Ovesen 1979:1) and all aspects of social intercourse have to be studied (Liep 1979:1). This is, of course, an immense task in practice, when we apply the principle to a concrete society. For reasons of space and time, se-lection has to be made, and the analysis has to be concentrated on significant structures on the syntagmatic level, such as the economic, social, ideological and perhaps political structures. In order to reduce complexity. Many details have to be omitted and attention has to be paid to the most essential features.

12.   COMMUNICATION OF RESEARCH DATA

The above illustration of structural levels has concerned only the so-ciety to be studied. In anthropological research process, however, the limitations and biases caused by the analyst himself are of crucial im-portance. He also, as an individual and part of his own ethnic group with a distinct culture, has a structure with similar levels as society. As illustrated below (Fig. 3), the researcher can be conceived as having several structural levels. For the sake of simplicity these levels are re-duced to the dichotomical s- and p-structures, which are mediated by a 'black-box', this time termed as 'mode of registration' (Ardener 1978:112–13; Hastrup 1978:129).

The illustration shows that there is a considerable distance from the p-structures of the society, which is the primary object of the study,

to the p-structures of the researcher, his mind, where the final analysis takes place. The common ground where a considerable coverage, although

Fig. 3. The channels through which a researcher receives information of the p-structures of a foreign society.
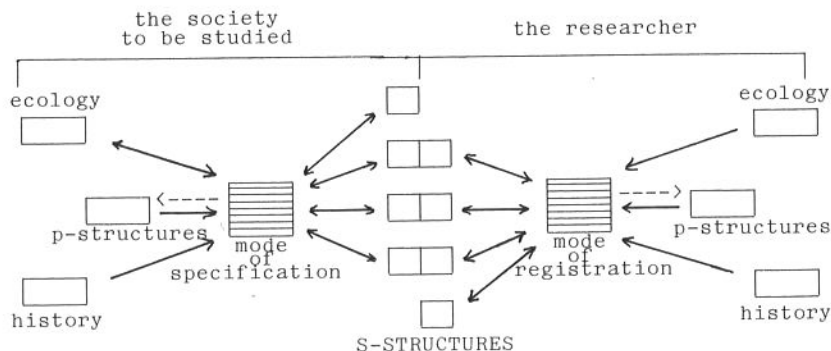
partial, of these two systems can be expected are the s-structures. With some observed 'facts' of the society the observer is immediately familiar, while others are a puzzle to him, because they do not belong to his field of experience. Yet the structures are the only channel of information between the p-structures of the analyst and the observed society. The 'black box' in both systems operates selectively, although in opposite direction. The s-structures of the observed society have been specified or modified by the mode of specification. The sense image which the analyst creates of the s-structures of the society is specified by his own 'black box', which, when operating to this direction, is a kind of mode of registration.

The twin-pointed arrows between 'black boxes' and s-structures show that the stream of information is two-directional. The relation between ecology and 'black-box' is also two-directional; ecology constraints the range of possible modifications, but people also may select their ecology. The arrows ponting to opposite directions between p-structures and 'black boxes' illustrate, that the mind has primacy in forming structures, but that it is not unaffected by the information it receives.

13.   FIELD METHODS AND EXPERIENCE

The fieldwork of this study and also the formulation of research prob-
lems might be characterized as a process which has gone through a num-
ber of modifications. The work in the specific research area (Map 3, p.
13) began in June 1975, at first sporadically, and from June 1976 onwards
on a full-time basis. Prior to this, however, I had become oriented to
African cultures through having lived in Tanzania since November 1967,
excluding a break of two years in 1971-73.

While emploued by the Finnish Missionary Society (FMS) and serving the
Evangelical Lutheran Church of Tanzania (ELCT), I lived with my family
among the Nyakyusa on the northern shore of Lake Nyasa from 1968 to
1971, among the Bena in Kidugala (Njombe District) in 1973-74, and in
Makumira, east of Arusha, from August 1974. In addition to giving fluency
in Swahili, this experience acquainted me with the life of people of dif-
ferent ethnic origins and in various physical environments. This long
period of orientation proved to be advantageous in the actual fieldwork,
and it shortened considerably the period of being an 'interesting stran-
ger' or of 'passive research' (Freilich 1970a:18).

In 1974, while I was teaching at Makumira Theological College, my interest
was drawn to the rapidly spreading spirit possession phenomenon in the
Kisongo Maasai area. This was the actual impetus which led me to study
the Maasai, and I started immediately to study the Maa language. When,
in the beginning of 1975, I was asked to join the Jipemoyo Project as
an associate member, I was offered a chance to start fieldwork among the
Parakuyo in the Lugoba-Msata area, where spirit possession phenomena
were reported to be common and to have functions similar to those in
the extensive Kisongo area. The more I became familiar with the subject,
the more I became convinced that the phenomena were linked to the so-
cial structure and culture, and therefore the structural aspects of the
society should be considered first. The critical comments of other team
members, particularly of Prof. Peter Rigby (1976), were valuable at this
stage of the work.

From the beginning of 1975, when I was still stationed at Makumira, I paid sporadic visits to the Kisongo Maasai area with Steven Wanga, an Arusha, who had lived for a long time in that area and knew the people. During these visits I gained some insight into the general situation, ecological conditions, family structure, medical practices etc. I also had an opportunity to paricipate in male and female initiation rituals, a rain-making ritual, and to observe spirit possession being actualized. From June 1975 onwards I paid visits of a few days to the actual research area and began developing a rapport with people (Freilich 1970b:539-56), so that communication between us became possible.

My entry into the Parakuyo society was greatly helped by Reuben Wanga, the elder brother of Steven, who worked as a butcher and local evangelist in Lugoba. His son Yonas was then employed on the Jipemoyo Project as a research assistant, and his contribution as a language teacher, interpreter, interviewer, transcriber etc. was invaluable up to the end of the first field period in 1976. While they were Arusha, Reuben and Yonas shared the same Maa language with the Parakuyo. Yet they were socially marginal to the Parakuyo society. Their marginality was probably an asset from the viewpoint of getting access to the community. They did not have to take big social risks by being on good terms with me from the very beginning (Freilich 1970b:550), because they were not full members of the society. Yet Reuben and Yonas knew practically all the Parakuyo by name, as well as their living places.
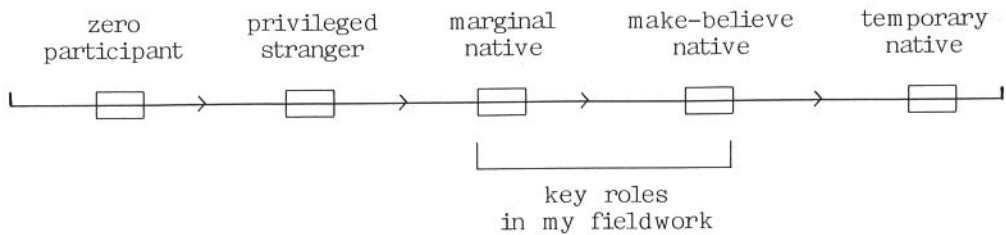
My initial interest in spirit possession phenomena was also a topic which interested the local Church leaders as well as the Parakuyo. Most of the Parakuyo converts were women who had been 'possessed' by unknown spirits. The phenomenon was experienced as a nuisance and they wanted to know what it actually was, and why the spirits troubled only women.

An indication of the relatively rapid development of rapport was, for example, that Paulo Moreto, an educated younger brother of the **oloiboni kitok**, wrote me a letter of invitation to the esoteric ritual of officially closing the initiation period of the Ilmakaa age-set, when I had not yet started full-time field research.

The division of fieldwork into passive and active phases (Freilich 1970b) is partly misleading, because there is great variation in the time needed for gaining access to different types of data. For example, I was able to record Parakuyo history, general and personal, from a number of elders during the first contact with the persons concerned. On the other hand, access to some esoteric knowledge about the activities of the **oloiboni kitok** has remained permanently closed. Transition from the passive phase of orientation to active research was, therefore, a gradual process rather than a sudden change of working methods. The almost total absence of the totally passive phase might derive also from the long period of orientation outside the research area, which enabled me to start active fieldwork soon after coming to the 'field'.

When I started the full-time fieldwork in June 1976, I had already visited a number of the scattered places where the Parakuyo live. I had also participated, by invitation, in two initiation rituals, which normally gather most of the community together. These were also good occasions to introduce me to the community. Also later on I participated in all the initiation rituals which came to my notice.

The roles of a fieldworker in the society to be studied can be placed somewhere between the polar categories 'outsider' and 'insider' (Agar 1980:191). A useful subdivision of these categories has been given by Freilich (1970b:536-39), who distinguishes five levels of participation in anthropological research. These roles are: (1) 'zero participant', who does not participate at all in the life of the society and whose presence does not influence the situation; (2) 'privileged stranger', whose participation level is low and whose acceptance by the society is minimal; (3) 'marginal native', who is accepted in the society as a marginal member; (4) 'make-believe native', who is sometimes welcomed to play the work-break game (to give up the role of an anthropologist temporarily), while the degree of participation increases temporarily; (5) 'temporary native', who may temporarily be tempted to abandon the role of a researcher and become a complete participant. This continuum is presented below.

```
zero            privileged      marginal        make-believe    temporary
participant     stranger        native          native          native
└─────┬────────────┬────────────┬────────────┬────────────┬──────┘
      ▭      →      ▭      →     ▭      →      ▭      →      ▭
                                └────────────────────────┘
```

key roles
in my fieldwork

Although I did not feel myself a 'zero participant' in any phase of field-work, I was certainly a 'privileged stranger' during the first contacts with the Parakuyo of the study area. It was obviously fortunate that I made my first contacts during periods of a few days, with intervals of one month or so. These first contacts were entirely informal, without any purpose of pursuing discussion into any specific topic. Because there was a project of several years to be launched soon, it was natural to talk about the objectives of the Jipemoyo Project in these discussions. For half a year I did not use any recording devices except a notebook for noting down memories in the evening. The remaining time was used for learning more of the language and gathering together all kinds of histo-rical, cultural, and sociological details from the literature, as far as my teaching duties allowed.

Although I explained to all concerned that my share in the project was to study the spirit possession phenomena, I did not start with this topic right away. My first significant field records are from November 1975, when I recorded on tape some general history of the Parakuyo and also the family history of Abdala Samsindo, an elder brother of Mtumia, the chairman of the Mindu Tulieni pastoral village. This contact opened up new possibilities, because I was invited to participate in his daughter's initiation rituals a month later. The contacts with the Parakuyo in Msata, where the project had rented a house, resulted in another invitation to participate in a girl's initiation rituals in Mikongoro (Map 3, p. 13). I took these invitations as an excellent chance to get to know people of all ages and ranks.

Because one aim of the project was to record a disappearing culture and to make it public also to a wider community, we agreed with the persons in charge to use recording devices, i.e. a tape recorder, a film camera,

and photo cameras. Part of the agreement was that I would prepare a movie film of these rituals and show it to them and others interested later on. It immediately became known to the whole society that we, Yonas and I, were, in addition to being guests, doing utafiti ('research' in Swahili) without equipment. We were told in advance about the coming events, so that we could try to be ready in time in the right place.

I found the tape recorder very useful, even essential, in recording many types of oral literature, such as historical accounts, women's religious songs, the songs of warriors and girls, elders' recitative songs, riddles, proverbs, fables, tales etc. It was possible to record much of this material in authentic contexts in various phases of the ritual process, which contained at least 25 different rites or non-ritual formalized acts, each in a strict order. These rituals turned out to be a rich source of data on the complex symbolic system. They also made it possible to establish friendly relations with a number of people for various reasons. Because the whole process of initiation took a period of up to four months, the follow-up of the phases after operation (clitoridectomy) included several visits to the initiate's kraal. These visits also offered an opportunity to obtain answers to a number of questions raised by the initial analysis of the observed and recorded rites.

Because such rites are a critical phase in an individual's life-process, it was also possible to expand inquiries to other subjects related to initiation. For girls, the sequel of initiation is to get married soon after recovery. I had a chance to follow how the suitors negotiated with the bride's father on bridewealth and other formalities needed before marriage.

When I was later in a position to participate also in boys' initiation rituals, which have different aims from those of females, I was offered a natural context for investigating the whole age-set system. The initiation period had been opened in 1970, and only occasional male initiation rituals were arranged during the period of fieldwork. Yet participation in these made it possible for me to observe and document various phases of these male rituals. Discussions before and after rituals with the people concerned helped me gradually to form a complete picture of

these events. Whenever possible, I used a tape recorder for documenting accurately what was said.

During the first stages of fieldwork I principally discussed topics which did not cause embarrassment, and which the people themselves felt worth recording. Thus the historical accounts of the eldest members of the society were recorded first, and these were followed by a number of topics related to initiation rituals. I started recording spirit possession histories in February 1976 and continued until the end of June 1976. Connected with this study of the curing activities of the Parakuyo iloibonok, and also of some prominent Kwere and Zigua healers whose services the 'possessed' women sometimes used.

During the whole period of full-time fieldwork in 1976, initiation rituals were the fixed points which more or less regulated my activities. They turned out to be my major research topics, and at the same time they were excellent occasions to observe symbolism in practice. Gradually, after having participated in several of them, my accuracy of observation increased, while at the same time I became more involved in the activities themselves. The intensity of participation was perhaps greatest in a series of three consecutive initiation rituals which were arranged in September 1976 in different places, partly overlapping. After having been part of the crowd for three days and nights, hearing constantly rhythmic songs and dances and occasionally participating in them, eating quantities of roasted meat, observing with strained attention whether the initiates could stand the circumcision without flinching, and seeing all the time red, black, and dark blue colours around, one could feel temporarily part of the celebrating society.

Although I had started by investigating primarily spirit possession phenomena, the area of investigation expanded with time. Each piece of new information raised further questions, and gradually I learned that it is not possible to isolate single phenomena and to study it without relating it to the social network and the cultural background. While still keeping to my original research object, I started to pay attention to certain recurring regularities in symbolic behaviour, in clan structure, in age-set structure etc. This observation led me to investigate the ritual structure

more extensively, although I had to rely in this mainly on oral information.

Through contacts with the village secretary it became possible to arrange two visits to his elder brother, Mtare Moreto, the **oloiboni kitok**, who lived in Kambala, south of Mvomero in the Morogoro District. A visit to Sangeni, a small village south of Handeni, the area of a number of age-set leaders, was made possible after we, Yonas and I, accidentally met a young warrior of that area, when he was returning home after having been travelling around for six months.

After this intensive fieldwork stage, there was a period of arranging the field material and of theoretical reading and reflection in Finland. I tried to find a theoretical framework which would help to arrange the data in a meningful way. A detailed analysis of initiation rituals corroborated my assumtion that Parakuyo society might provide useful data for studying deep structures, which give form to surface phenomena, and which might help to establish relationships between a number of otherwise unintelligible data.

The intensive field period of three weeks with Paulo Chaparisi in January 1982 was used for obtaining more detailed information on selected topics, such as intra- and intersocietal transfers, kinship, age-set rituals, symbolism, material culture, classification of nature, names and uses of all known trees and grasses etc. By this time I was working almost exclusively with Paulo, whom I did not know before, but who proved to be an excellent co-worker and informant. He was exceptionally intelligent and eager to learn new things. That he had recently become a member of the Lutheran Church does not justify regarding him as a marginal person. This fact obviously increased his confidence in me and made him ready to discuss any subject he knew about. His father had nine wives and was respected by the traditional section of the society. Paulo also represented that section of the society which had to move quite frequently from place to place owing to cattle diseases.
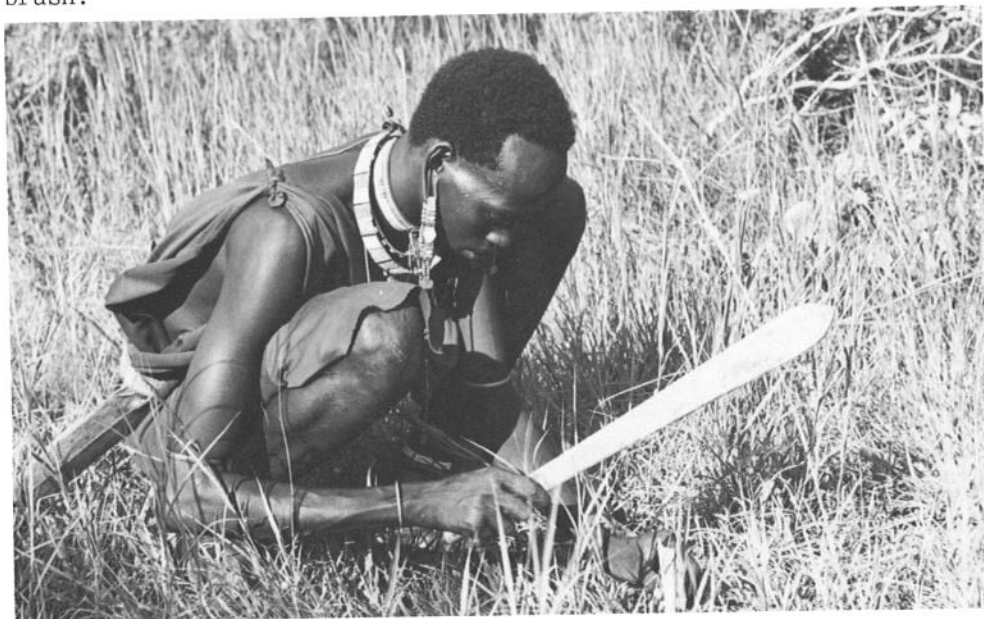
Picture 2. Paulo Chaparisi, my
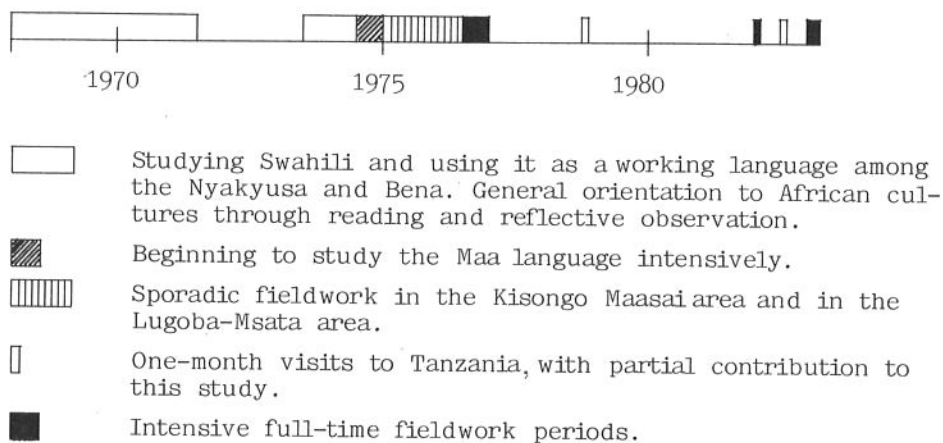research assistant in 1982 and
1983.
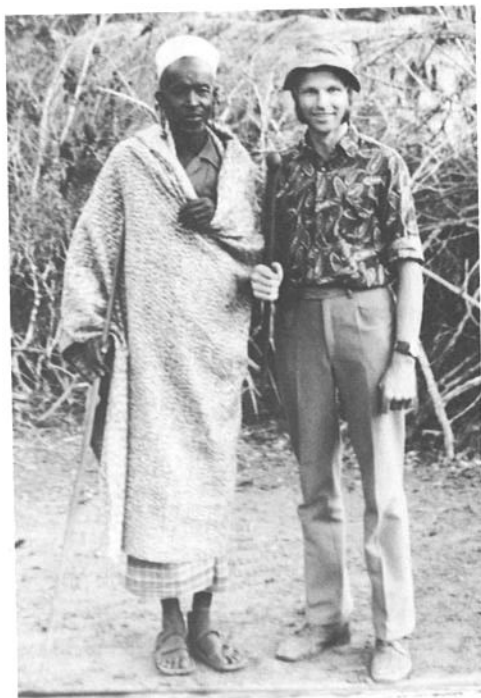
Picture 3. Author and an elder in
Sangeni, south of Handeni.

Picture 4. Paulo with an **olalem**, a kind of 'machete'. It is used for
cutting trees, building a kraal fence, skinning animals, cutting meat,
chopping a cattle stick or a club, and even for preparing a tooth
brush.

I was in the field again in January-February 1983, this time working for a multi-disciplinary project which had rather practical aims. Although it was not directly related to this study, some of its results have been used here. Because it was concerned with tsetse fly eradication and other bovine diseases, nutrition, local migration, and inter-ethnic relations, all of which were issues of direct interest to the Parakuyo, my role was also different than before. Even though I had been involved in the development-oriented Jipemoyo Project even earlier, my role as leader of a team tackling urgent development problems was now clearer. The team's tests on methods of preventing certain trees from pushing out new shoots were particularly impressive and raised expectations that the problem of tsetse fly could be got under control. During this period I also travelled in the Kisongo Maasai area to obtain comparative data among the Pastoral Maasai.

Fig. 4. The phases of fieldwork.

Studying Swahili and using it as a working language among the Nyakyusa and Bena. General orientation to African cultures through reading and reflective observation.

Beginning to study the Maa language intensively.

Sporadic fieldwork in the Kisongo Maasai area and in the Lugoba-Msata area.

One-month visits to Tanzania, with partial contribution to this study.

Intensive full-time fieldwork periods.

In the initial phases the forms of fieldwork were adjusted to the varying local situations, and interindividual contacts were used for creating new relationships and for acquiring access to new sources of information. A wide variety of situations were used for obtaining quite diffuse information. In later phases the fieldwork was selective, directed to chosen topics which were central to the research plan.

One of the factors relevant to successful fieldwork is the identity of the researcher.[19] My role and background as an individual researcher have been discussed above. I was also an associate member of the Jipemoyo Project team, which developed an approach in which researchers and villagers became involved in research and development process for improving people's living conditions. In this situation the expectations of people towards the researchers were sometimes quite high, and we had to take interest in a wide variety of issues. The project also gave rise to theoretical discussion and in one phase of the discussion this orientation was termed the 'Participatory Research Approach' (PRA). It was postulated that the aim is not only to identify problems but also to participate in their solution.

How is the present research related to the PRA which was developed within the Jipemoyo Project? The best characterization might be that it is carried out in tune with it or in the same spirit. The main part of the fieldwork (1975-76) took place before the concept of PRA had been invented, and when some agreement had been reached only on the practical procedures of the fieldwork. Since then there has been theoretical discussion on whether it should be called a method, an approach, or a perspective, and what it should include. There have been demands that it should have a clear theoretical and ideological basis in accordance with marxist historical materialism (e.g. Bryceson 1980:22-23).

It is obvious, however, that a more fruitful conceptualization is the one proposed by Swantz, when she makes use of Åke Sandberg's notion of **perspective**. Swantz has suggested that this term refers to the openness of research partners in adapting their orientations according to the research situation. The theoretical framework and interpretation would be results of the research process rather than a **priori** assumptions (Swantz 1982:26-29).

The present research has followed precisely the above procedure, being inductive rather than deductive in character. The openness during the fieldwork changed the approach significantly from spirit possession phenomena to the more basic structures of the society, its culture, and its socio-economic environment. The issues investigated in 1975 were quite

different from those dealt with in 1982 and 1983. After the first field
period (1975-76) there was a long process of theoretical reading and re-
flection to find a theoretical frame to order the data, and the final
result of this search is the theoretical model presented above. The
later field periods mainly supplemented defective points in earlier data.

14.   VALIDITY AND RELIABILITY OF SOURCES

This study is based principally of unwritten oral information, and there-
fore the question of the validity and reliability of the sources is cru-
cial. As the replication of research procedures is not possible (Salamone
1979:47), the documentation should be ensured by other means.

Concerning the **validity** of the research, i.e. the question of whether the
data obtained really measure what is intended, the material collected in
the manner shown should be sufficiently valid. There was the continuous
control of the researcher in every step of the research process, and by
being present in every situation and able to interfere whenever neces-
sary, the researcher was able to direct the output of information and dis-
cussions to valid issues[20]. If there are weak points in validity, they
are hidden in the analysis itself. It is not selfevident, for example, that
the data on the various realization structures necessarily prove the exis-
tence of basic structures as they are analyzed below. This is, however,
claimed in the study, and this claim is substantiated by data on reali-
zation structures. The problem is hermeneutic and not of the validity
of the field material.

More important in this research, and in anthropological research general-
ly, is the question of **reliability**. To prove it convincingly to the ordi-
nary reader is doubly difficult: the subject area is remote and most of
the recorded material is in the Maa language. Part of it is translated
into Swahili, and some is also originally recorded in Swahili. Almost all
field notes are in English. The field documents are available in the Ar-
chives of the Institute of Development Studies, University of Helsinki.
In contrast to the general practice in anthropological research, referen-
ces are made to this material at appropriate places in the text.

As regards revealing the actual facts the material is uneven. The historical accounts in particular need to be subjected to careful ctiticism, because some of them have been distorted with the purpose of idealizing the past (Vansina 1965:102-08). Although of little value to historian, these accounts might be valuable to a psychologist.

Because the inquiry mainly concerned institutions operative at the present time, the danger of distorting the 'facts' diminished. Opportunities for counterchecking were used extensively, and the same themes were discussed in various situations individually and in groups. There were advantages in both types of interview. In groups there was always the control of others if the speaker did not remember or know the matter concerned, or if he did not know it correctly. In talks with individuals we were able to discuss more intimate matters, many of which could not be written, for ethical reasons, even in field notes, still less published. As background information they are, however, valuable. There always remains the chance that an ethnic outsider may misinterpret even what at the time seem obvious 'facts'[21].

It is largely the task of the researcher to evaluate the information received. Here too the advantages of long-term field research are evident. The characters of people and their position in the society become gradually known, so that one can anticipate the possible direction of bias in each case. By using several sources for the same information the biased elements can be eliminated in the analysis. Observation was used whenever possible. For example, the study of rituals normally comprised discussions before the rituals, observation, tape-recording and photographing during them, and discussions afterwards for further clarification. Photographs and particularly the movie film were valuable in documenting structural relations and forms of symbolism in various phases of the ritual.

The question of reliability has to be raised regarding the substantial amount of information obtained in discussions with Paulo Chaparisi in 1982. This situation was one of an interindividual relationship par excellence, where, if there is no control, the researcher can be led astray.

Paulo Chaparisi was exceptionally bright, with an excellent memory, and
he had learnt very well everything a person of his age (22 in 1982) was
supposed to know. He was not a marginal person in the society; he was
rather wealthy, never having been out of the traditional life context.
He was, however, of the type that had he been given the chance he would
have acquired a good school education - and probably lost the rather ba-
lanced identity of a Parakuyo. Somehow I learned to trust him, the more
so when I heard the judgements of others concerning his character, know-
ledge etc. I noticed that when there was a group of warriors with me
discussing some issue, it was Paulo whose knowledge and memory was ge-
nerally trusted. He was also ready to admit if he did not know, and in
those cases we turned to his father, Chaparisi, who with his nine wives
and a large herd enjoyed great respect in the community.

After all, the type of information I needed could be checked through ot-
hers, and these inquiries reinforced my intuitively formed view of Paulo
as a valuable and reliable informant. Despite my efforts I did not manage
to obtain permanent residence in a Parakuyo kraal. On the other hand,
living in Lugoba enabled us, Yonas and I, to make transcriptions of tapes
immediately after recording. Lugoba was also a place which a substantial
number of Parakuyo visited daily, and they often visited our residence
there. The more friendly relations were established, and the more I learn-
ed from them, the more often we visited each other. It was felt to be
prestigious to invite a researcher to a ritual, and I was kept well inform-
ed of them.

Because this research was based more on observation techniques, partici-
pation, and long-term interpersonal relations than on collecting oral tra-
dition, the question of genre categories is not central here.

Finally a word on the characters of persons engaged in field research.
The field conditions are harsh and the strain on the researcher is some-
times stretched to the extreme. A sense of humor is invaluable in main-
taining good spirits. The Parakuyo generally, but Yonas Wanga and Paulo
Chaparisi particularly, were incorrigible jokers, whose company was

enjoyable throughout the worst strains.

When carrying out fieldwork one is far from academic sterility. A field-
worker cannot rely on his authority or status as an academic person, or
his position in the administrative hierarchy. He is evaluated and treated
according to his personal qualities, fluency in local languages, ability
to learn new things, willingness and capability to live on local terms,
readiness to do people various kinds of services and a sincere interest
in problems not directly related to the research itself. He is immersed
in all kinds of processes which take place within the society; yet he
is not a member of that society. The success of the fieldwork depends
essentially on how well the researcher manages to function in these mul-
tiple roles without pretence.

NOTES to Chapter B

1   In the West African cultures, such as the Yoruba, Ashanti, Fon,
    Dogon, and Kotoko, cosmological classification is sometimes developed
    into systems which combine all of nature, whether animate or in-
    animate, into one all-embracing totality, where fixed correspondences
    exist between sub-classes; Turner 1977:192-94; Lebeuf 1977:185-88;
    Kamau 1977:361-79. The extent of formal cosmological classification
    in the Bantu cultures of Central Africa is generally lower. It has
    been assumed that this is due to the very different ecological,
    geopolitical and social circumstances of these two groups of people;
    Turner 1977:194.

2   For example, a number of articles in McCormack and Wurm (eds.),
    "Language and Thought: Anthropological Issues" (1977), in Ioan
    Lewis (ed.), "Symbols and Sentiments: Cross-cultural Studies in
    Symbolism" (1977), in Lloyd and Gay (eds.), "Universals of Human
    Thought: Some African Evidence" (1981), and the monograph of
    Sperber, "Rethinking Symbolism" (1975), recognize more or less
    explicitly the significance of the deeper structural level.

3   Edward Sapir proposed that a people's thought systems and their
    language habits are closely interrelated. In his opinion, this cor-
    respondence is not monocausal, viz. language would be only a re-
    sult, or reflection, of the people's ways of thinking. The inter-
    dependence of language and thought works also in the opposite di-
    rection, so that "the 'real world' is to a large extent unconsciously
    built upon the language habits of the group"; Sapir 1949:162. Ben-
    jamin Lee Whorf, an enthusiastic ethnographer and linguist, attempt-
    ed to prove, or 'illustrate' (Haugen 1977:23), this hypothesis by
    showing correspondences between language and world view in cer-
    tain American Indian languages; Carroll 1956.

4   His functional theory was originally formulated in his ethnographic

monograph "Argonauts of the Western Pacific" (1922) and refined in some of his later publications, e.g. in "Myth in Primitive Psychology" (1926) and "Magic, Science and Religion and Other Essays" (1948). He presented three basic theses for the basis of the functional theory: (1) a culture constitutes a functionally coherent entity; (2) each cultural feature, custom, rite, belief, object etc. has a definite use, or function; and (3) each cultural feature is necessary and irreplaceable from the viewpoint of the whole, a part of which it is. See also Honko 1972:19-51.

5   In practice, of course, the analysis of all features of social reality is impossible within the normal research routine, and a certain selection of significant features, be they institutions, thought systems, beliefs, economic systems etc., must be made.

6   "Andaman Islanders" was first published in 1908. After a revision in 1913 it appeared in 1922 and again in 1933 with some additions. In this study reference is made to the 1933 edition.

7   A point subjected to criticism. See e.g. Honko 1972:46, 48.

8   Van Gennep used the term 'social position' (e.g. 1960:192) to denote social status, a term which was brought to sociology and anthropology by Ralph Linton (1936).

9   Among his basic works are: "Elementary Structures of Kinship" (1969); "Totemism" (1969/1962); "The Savage Mind" (1966); "The Raw and the Cooked" (1970); and his series of mythological studies "Mythologiques", two first parts of which were published in English in one volume titled "From Honey to Ashes" (1971). In "Structural Anthropology" (1972/1963) seventeen of his numerous articles have been slected to represent his theoretical position. In addition, a number of studies on Lévi-Strauss have been published, e.g.: E. Leach, "Lévi-Strauss" (1970); M. Glucksman, "Structuralist Analysis in Contemporary Social Thought" (1974); I. Rossi (ed.), "The Unconscious in Culture: The Structuralism of Claude Lévi-Strauss in Perspective" (1974); and C. R. Badcock, "Lévi-Strauss: Structuralism and Sociological Theory" (1975).

10 Radcliffe-Brown, for example, tried to combine functional and structural aspects into one method (Radcliffe- Brown 1952). For him, 'structure' was, however, derived from, or rather consisted of, empirical phenomena. The analysis was carried out on the empirical level.

11 Regarding the term 'total phenomena' see Mauss 1967:1, 3.

12 Ardener 1971b:450, 458-459; 1978; Hastrup 1978:128-131, 141; Schwimmer 1973:VIII; 1978:XII-XIII; 1979b:272-73; Rossi 1974b throughout; Eisenstadt 1975; and Crick 1976 implicitly.

13 H. Schneider has used the term 'reciprocity' as a subcategory of 'social exchange', which term in his opinion "comprehends all forms of human interaction seen as forms of exchange, social structures being largely surface manifestations of this exchange or allocation process"; Schneider 1974b:98. See Sarmela 1969:9-15.

14 Regarding issues related to the objectivity of anthropology, see
   e.g. Swantz 1982:6-10. Relying on the ideas of Devereux, she
   maintains that objectivity in research is gained only by laying bare
   one's subjectivity. The researcher receives only subjective images
   of reality, and the denying of this subjectivity would result only
   in objectivism, not objectivity.

15 Also Lévi-Strauss, who earlier emphasized the opposite positions
   of culture and nature, has later suggested that culture is a "syn-
   thetic duplication" of nature; Lévi-Strauss 1974. The emphasis of
   ecology is still more important in such pre-industrial societies
   where land is a subject of labour for direct appropriation rather
   than an instrument of labour; Meillassoux 1972:99.

16 For example Nyaturu and Taturu in Western Tanzania are sections
   of one ethnic group, the former having been specialized in agri-
   culture and the latter in pastoralism.

17 Wilden noticed this difficulty and suggested that although the
   p-structures cannot be analysed by using ordinary language, they
   may be communicated; Wilden 1972:40. In real societies they are
   communicated through several channels, particularly symbols. How-
   ever, this does not solve the problem of reporting, which is es-
   sentially verbal. See also Simonis 1974.

18 See Rossi 1974b:28-51; Ardener 1971a:XVII. Ardener has suggested
   that the inductive search for symbolic and semiotic structures by
   using intuitive perception "in the end is even likely to 'predict'
   more, than ... the hundreds of social surveys daily undertaken";
   Ardener 1971a:XXXIII; see also Friedman 1974:453.

19 The researcher has seldom a self-evident identity. He may be sub-
   ject to manipulation and be persuaded to declare his position for
   or against different groups. Frances Henry (1966) experienced this
   in studying political development in Trinidad, and the maintenance
   of political impartiality was difficult. Diane Lewis (1973) has paid
   attention to the different roles of anthropologists in colonial and
   post-colonial times. She has evaluated the different roles of the
   'objective' outsider and the critical insider (the researcher is a
   member of the society studied), and pointed out their different ad-
   vantages. Given the present conditions where the anthropological stu-
   dy is carried out, emphasis should be shifted from theoretical
   studies to those with involvement in development and change; D.
   Lewis 1973:589.

20 It is apparent, as Pelto has pointed out, that the weak points in
   anthropology are found in reliability, i.e. in the repeatability of
   scientific observations. Deriving from the working methods, there
   is always a danger of subjective assumptions and evaluations, the
   objective control of which is more difficult than assuring validity;
   Pelto 1970:41-44. Furthermore, the recording devices may change the
   psychological setting of the situation where information is obtained;
   Curtin 1968:370; Devereux 1967: Pentikäinen 1977b.

21   Ethical principles of fieldwork have been much discussed in the 1970s. In the name of science, ethical questions cannot be bypassed. In fact, they are a central framework in estimating the feasibility of the whole enterprise. These problems are pertinent not only to biomedical and psychological experimentation (Cassell 1980:29-30); they may be central issues also in anthropology (Gothóni 1977a, 1977b), although situations vary in this respect.