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A CRITIQUE OF COGNITIVE ANTHROPOLOGY

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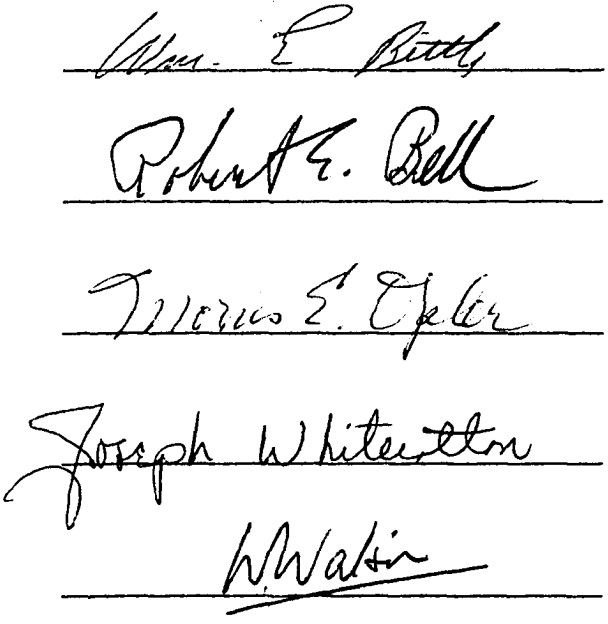
BY



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I

INTRODUCTION

Cognitive anthropology, according to its advocates, describes a new theoretical orientation and approach to ethnography. The term covers a variety of studies which aim to reveal the manners in which different peoples organize and utilize their cultures. More explicitly, the methods of cognitive anthropology focus on discovering the organizing principles underlying behavior by gaining access to the cognitive categories which constitue these organizing principles. In essence cognitive anthropology seeks to answer two questions: What material phenomena are significant for the people of a particular culture; and how do they organize these phenomena (Tyler, 1969:3).

The term "cognitive anthropology" is herein used as a descriptive supercategory which includes those ethnographic and linguistic investigations presented in the literature under such terms as folk taxonomy, ethnoscience, new ethnography, ethnosemantics, componential analysis, and formal analysis. These various titles denote types of studies which represent definite similarities in basic a priori assumptions, general methodology, and desired goals. In this study I will employ as a stylistic aid the terms "cognitive anthropologist" and "cognitivist" as labels for those who practice cognitive anthropology because I feel that the cultural and social insights which represent the goals of the cognitivist's methodology rest basically on their conclusions concerning some manner of interpretation of the cognitive structures of the culture under study as

revealed by their methodology.

The field of cognitive anthropology has arisen only in the last ten to fifteen years. As yet no comprehensive critique of this area of inquiry has been attempted. In 1964 William C. Sturtevant (1964:101) wrote regarding the ethnoscientific approach:

> ...most previous discussions and exemplifications have been couched in such terms that many anthropologists assume that what is being described is not ethnography but some kind of linguistics of 'kinship algebra' or both, so that there may now be room for a more informal, less technical characterization.

In Cognitive Anthropology, the most recent general work dealing with the area, Tyler (1969:1) adds:

> Assessment of such new departures is always difficult. What are the historical antecedents and what do they augur for the future of anthropology? Are these genuinely viable reformulations or are they simply short-lived fads and blind alleys, detrimental in the long run to significant research?

In response to the suggestions of Sturtevant and Tyler, I propose to offer a critique of cognitive anthropology. My evaluation and assessment will be directed toward several problem areas, including those stated above by Tyler. Perhaps the best means of explaining my approach and the various explanatory models I intend to employ will be briefly outline them.

Evaluation is inherently a relative operation. Something must be evaluated in terms of something else. My first problem will be to derive a type statement of cognitive anthropology. This generalized statement will be the core with which I will work in the subsequent portions of this paper. I will abstract the type from the literature on cognitive anthropology utilizing a structure which equates with the cognitivist's view of his field and its major components. This model will be the frame of relevance by which I will evaluate cognitive anthropology for the

purposes of contructing a typical or general series of propositions.

The second procedure will be the assessment of cognitive anthropology in relation to a history of relative anthropological, psychological, and sociological theory. This step will involve tracing the influence of such theoretical orientations as structural linguistics and Gestalt psychology and such writers as Sapir, Boas, Opler, Benedict, and Kluckhohn on cognitive anthropology. A related evaluation will deal with cognitive anthropology in terms of a sociology of knowledge: Why cognitive anthropology? and why now?

The third evaluating position will be couched in terms of the philosophy and psychology of perception. Certain kinds of assumptions pertaining to the nature of perception lie at the very foundation of methodology in cognitive anthropology. Many of these assumptions I believe to be false and misleading. I will attempt to demonstrate my contentions in the material to follow.

The fourth section of the critique will deal with an evaluation of cognitive anthropology from the viewpoint of the philosophy of science. This broad philosophical perspective will deal with the philosophy of science, scientific methodology, and epistemology, and it will offer a special focus on the nature of discovery procedures. To this extent it will be a general critique. Sturtevant (1964:111) notes that if an ethnography is to reflect the cognitive system of the bearers of a culture, the validity of the description depends on the discovery procedures.

A final consideration will deal with the utility of cognitive anthropology for anthropological theory in general. For example: Of what heuristic value and theoretical usefulness is the theory of culture implied in cognitive anthropology? In what ways can cognitive anthropology

contribute to ethnological theory? Is cognitive anthropology ultimately detrimental to the growth of viable theory in anthropology?

I am aware that any one of these topics is potentially amenable to near infinite treatment. However, the length with which I choose to deal with any particular problem will be a reflex of the level of generality and inclusiveness I employ for the specific task. Obviously, every problem area related to cognitive anthropology and psychology, the philosophy of science, ethnological theory, linguistic theory, ethnographic methodology, and the history of anthropological theory cannot be surveyed in detail. Two major foci of interest and orientation, however, will pervade the various discussions to follow and decide those areas to which I will give the lion's share of attention.

The first of these foci is methodology. I intend the term to have a wider extension of meaning than is usually given it: viz., the specific procedures utilized by a particular science in the pursuit of the goals of its inquiry. Kaplan's (1964) notion of methodology and Kuhn's (1962) concept of "scientific paradigms" most closely approximate the parameters of the conception of methodology that I will employ as a guiding interest in this study.

Kaplan (1964:18-23) writes:

> The word 'methodology', like the words 'physiology', 'history', and 'logic'....is also one which is used both for a certain discipline and for its subject-matter. I mean by 'methodology' the study--the description, the explanation, and the justification--of methods, and not the methods themselves....I shall mean by 'methodology' a concern with midrange techniques and principles, which I shall correspondingly designate 'methods'. Methods are techniques sufficiently general to be common to all sciences, or to a significant part of them. Alternately, they are logical or philosophical principles sufficiently specific to relate especially to science as distinguished from other human enterprises and interests. Thus, methods include such procedures as forming concepts and hypotheses, making observations and measurements, performing experiments, building models and theories,

> providing explanations, and making predictions. The aim of methodology, then, is to describe and analyze these methods, throwing light on their limitations and resources, clarifying their presuppositions and consequences, relating their potentialities to the twilight zone at the frontiers of knowledge.

Kaplan's "methodology" perfectly compliments Kuhn's view of "scientific paradigms" for my purposes. Kuhn (1962:viii) states:

> ...the practice of astronomy, physics, chemistry, or biology normally fails to evoke the controversies over fundamentals that today often seem endemic among, say, psychologists or sociologists. Attempting to discover the source of that difference led me to recognize the role in scientific research of what I have since called 'paradigms.' These I take to be universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners.

The methodology inherent in the paradigmatic focus of a particular science defines and describes in a most fundamental manner the nature and limits of the "real world" for that science. Reality is tested and allowed only within the criterial boundaries prescribed by the paradigm. It is the world view of a science. A different methodological orientation and paradigmatic stress portray a profoundly different world and reality. Kuhn argues that the paradigm of a science, through the training of students of the science, becomes the description of reality which is internalized by the scientist. Kuhn utilizes this notion to account for the often non-rational defenses employed by the advocates of one scientific paradigm when their paradigm is threatened by the advocates of another--defenses involving social power and dominance rather than rational persuasion and demonstration.

In this paper I am interested in questions raised by the paradigm which is implied., though seldom expounded in the literature which deals with anthropological studies of cognition, and what they bode for the development of anthropological theory in general. What is the nature of the world that is assumed by the cognitive anthropologist? How does this world "fit" the perspectives offered by the classic methodology and

philosophy of science and recent studies in the psychology of perception? Does cognitive anthropology represent a theoretical and philosophical advance or a retreat from Kaplan's "twilight zone at the frontiers of knowledge?"

As can be understood from the preceding discussion, I will be least concerned with the material content of the various studies which are subsumed under the rubric "cognitive anthropology" and most concerned with matters of methodology. I am not as interested in Frake's (1961) presentation of how the Subanun of Mindanao diagnose disease or Conklin's (1955) description of Hanunoo color categories as I am in how and why these researchers arrived at their particular analyses.

A second major orientation of this study is best described by a partial and restrictive use of the term "pragmatism." Philosophical and metaphysical cul de sacs often render methodological investigations sterile. Throughout this paper I will attempt to avoid an appeal to the irreducible and irreconcilable. I agree with Pierce (in Sahakian,1968:256) that a major function of thought is to produce habits of action. It is my intention in this paper to investigate cognitive anthropology as a live issue rather than a dead one and to discuss problems and phrase conclusions in a manner relevant to what I consider to be the most valid direction for the continuing and positive growth of theory in anthropology. I do not wish to belabor the term "pragmatism" in an effort to make explicit the basic underlying concerns which will permeate the succeeding sections. Pragmatism can be understood as both a method and as a theory of truth. A pragmatic stance (i.e., concern with consequence and action) will guide my selection of issues which I consider relevant in a critique of cognitive anthropology, and to that degree I can claim a pragmatic methodology.

However, I reject the pragmatic theory of truth, i.e., the notion that if an idea "works" when applied to the concrete facts of experience, then it is a true idea. As the body of this study will reveal, I consider "concrete facts of experience" to be not so concrete at all and, further, a static conception of truth to be anathema to a science of anthropology.

The term "science", however, presents another problem. Most contemporary philosophers of science forgo the task of attempting to concisely formulate a definition of science (e.g., Kaplan, 1964:27; Weatherall, 1969: vii). Weatherall (1969:vii) suggests that the foundation of science is not any particular body of factual knowledge but a manner of thinking and acting. Cohen and Nagel (1934:191-192) write that "in essence, scientific method is simply the pursuit of truth as determined by logical considerations." All writers in this field would agree, I believe, that the basic orientation of any science is toward a logically formulated systematic knowledge of its universe of inquiry. It is this deceptively simple notion of science that I will hold when speaking of a "science of anthropology."

The question of the nature of science and anthropology is a valid one to raise in a discussion of cognitive anthropology. It is continually stated by cognitive anthropologists that a distinguishing characteristic of their manner of ethnographic description and investigation is its scienticity. Furthermore, Tyler, for example, feels that cognitive anthropology raises this crucial question: Is cultural anthropology a natural or a formal science? He (Tyler,1969:14) notes:

> Traditional cultural anthropology is based on the assumption that its data are discrete material phenomena which can be analyzed like the material phenomena of any other natural science. 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.

In summary, my main concern in the following is, to paraphrase Broom (1963:xviii), with problems that are intrinsically important and recurrent rather than with transitory considerations or matters of mere technique.

II

COGNITIVE ANTHROPOLOGY

In the following section I will attempt to characterize and describe the methodological, epistemological, and theoretical system which is cognitive anthropology. An analysis of the roots of this orientation will be approached in the succeeding chapter. At this point I merely seek to identify and explicate the goals and concepts of the so-called "New Ethnography" (Sturtevant, 1964:99).

Cognitive anthropology is a movement within anthropology dedicated to the improvement of standards of ethnographic description and analysis, and having as its source and inspiration the techniques of linguistics (Harris, 1968:568). Because cognitive anthropology represents an ethnographic methodology, a major portion of its essential characterization must deal with conception of culture held by the cognitivists as well as their appreciation of the purposes and goals of ethnography. The more specific assumptions and techniques employed by researchers in this area of interest flow from and are bounded by the sets imposed by the particular notions of the nature of culture and ethnography which they utilize both overtly and covertly in their research and analyses. Further, a first step in the characterization of cognitive anthropology must attend to the cognitivists' position concerning the relationship of language and culture, as well as their stance concerning the desirability and feasibility of the utilization of the techniques of structural linguistics in their search for relevant and valid ethnographic description.

Ward H. Goodenough is unquestionably the major high-visibility figure in the recent florescence of anthropological studies of cognition. With Floyd Lounsbury, Goodenough is the most often quoted cognitivist, particularly with regard to the cognitivist's position on the nature of culture, the proper aim of ethnography, and the method of componential analysis. His articles "Property, Kin, and Community on Truk" (1951), "Componential Analysis and the Study of Meaning" (1956), and "Cultural Anthropology and Linguistics" (1957) are noteworthy in this respect. However, in a private communication, Goodenough (1970) writes:

> I trust that in looking at my work you will look at more than componential analysis, which from my point of view is just a method for trying to describe systematically significational (as distinct from connotational) aspect of meaning. The kind of cultural theory that it fits for me is set forth at greatest length in the tenth and twelfth chapters of my book Cooperation In Change (1963). Various writers have attributed to me views regarding the nature of culture which they deduced from reading two or three of my articles; but with one or two exceptions anthropologists have failed to consider what I said at considerable length in Cooperation In Change on the nature of culture, custom, institution, and especially the relation of the individual to cultural change.

To honor Goodenough's suggestion above I will present what is, in his opinion, his most cogent statement concerning his vision of the nature of culture and the relationship of an individual to his culture. It is informative to commence with a presentation of what might be termed Goodenough's (1963:253) cosmological view as stated in his Cooperation In Change, (1963).

> All of nature, indeed, can be conceived as containing more or less internally stable systems which are the components of larger and yet larger ones, from atoms to stellar galaxies, from individual cells to complex organism, and from single species to ecological systems. No part of the real world, of course, is perfectly stable, though it may appear to hang in a state of balance for some time, especially when viewed microscopically, But the repetition of events within it is never exact, merely a modal clustering of tracks. These modal clusterings, however, are essential to human cognition, for in their absence people would be unable to discern discontinuities

> in their surroundings by which to discriminate categories of phenomena and thus build the percepts and concepts with which they discern the real world, cognitively organize it, and orient themselves to it.

A key to Goodenough's anthropology, as well as cognitive anthropology generally, lies in its psycho-biological or "drive" orientation. This "drive" orientation is one of the features which led George and Louise Spindler (1963:548) to conclude that "...the cognitive model is one of high potential utility...(because) of the possibility it affords of doing research on comparatively limited sets of relationships between operationally definable variables...." An individual has a certain range of needs which inexorably relate him to a reality which is given him by his culture. It is the nature of the juncture, according to Goodenough, that is the crucial concern of cognitive anthropology. Goodenough (1963:54, 65, 147) continues:

> What people see as needed to gratify a want is not necessarily what is needed in fact. What they see depends on their cognitive knowledge; what states of affairs they have learned to discern, what they believe to be the relations between them, and what they understand to be the processes by which one state can be transformed into another...In order to do something about their wants, people need to have sensory contact with their surroundings. They need a vocabulary of constructs by which to discriminate things around them, feelings within themselves, and ways in which they interconnect. They need to be oriented, in short, in terms of some coherent and internally consistent cognitive system...(People) come to perceive things in terms of classes or categories of phenomena. We operate with sets of color categories, shape categories, taste categories, and so on, whose combinations provide the basis for a perceptual taxonomy of our world. Cognitive organization also includes those ways in which the phenomena we discern appear to us to be mutually associated or arranged, and it includes the transformations from one to another perceptual category that phenomena appear to undergo as their mutual associations change. These discriminations of phenomena and process are our percepts, as psychologists have called them.

With regard to "culture," Goodenough (1963:258-265) writes:

> All that we can see of a culture is its products or artifacts, the things people make, do, and say. Because we are able to make inferences about a culture's content only through the study of its artifacts, we rather easily confuse it with them;...anthropologists

> frequently define culture as the shared products of human learning. More precisely these may be said to comprise: (1) The ways in which people have organized their experience of the real world so as to give it structure as a phenomenal world of forms, that is, their precepts and concepts. (2) The ways in which people have organized their experience of their phenomenal world so as to give it structure as a system of cause and effect relationships, that is, the propositions and beliefs by which they explain events and design tactics for accomplishing their purposes. (3) The ways in which people have organized their experience of their phenomenal world so as to structure its various arrangements in hierarchies of preference, that is, their value or sentiment system. These provide the principles for selecting and establishing purpose and for keeping oneself purposefully oriented in a changing phenomenal world. (4) The ways in which people have organized their experience of their past efforts to accomplish recurring purposes into operational procedures for accomplishing these purposes in the future, that is, a set of 'grammatical' principles of action and a series of recipes for accomplishing particular ends. They include operational procedures for dealing with people as well as for dealing with material things.

In a similar vein Sturtevant (1964:99) states, "...a culture itself amounts to the sum of a given society's folk classifications, all of that society's ethnoscience, its particular ways of classifying its material and social universe." Tyler (1969:3) adds:

> Cultures are not material phenomena; they are cognitive organizations of material phenomena. Consequently, cultures are neither described by mere arbitrary lists of anatomical traits and institutions such as house type, family type, kinship type, economic type, and personality type, nor are they necessarily equated with some over-all integrative pattern of these phenomena.

Frake (1964:133), echoing Goodenough, Sturtevant, and Tyler, conceives of culture in terms of a system of communicable codes by which the bearers of a particular culture make sense of the world around them. Concerning the "code" conception, D'Andrade and Romney (1964b:231) write:

> In saying that the primary interest of anthropologists who are studying cognition consists of socially learned codes, we are not implying that codes are the primary type of data for all anthropologists. However, if "culture" were to be defined within the communicative vocabulary, perhaps 'code', rather than 'signal' or 'information,' describes most accurately what most anthropologists intuitively feel is the proper object of study.

Perhaps the most often quoted cognitivist definition of culture is found in Goodenough's (1957:167-168) "Cultural Anthropology and Linguistics":

> A society's culture consists of whatever it is one has to know or believe in order to operate in a manner acceptable to its members, and to do so in any role that they accept for any one of themselves... It is the forms of things that people have in mind, their models of perceiving, relating, and otherwise interpreting them.

The cognitivists' notion of culture can be more fully understood by a discussion of their views concerning the nature and aims of ethnography, the description of a culture. I feel that an insight into the temper of cognitive anthropology is offered by the fact that, with the exception of Goodenough's relevant chapters in his Cooperation in Change, the concept of culture (as well as the concept of cognition) is more often assumed, in aspects more favorable to the goals of cognitive anthropology, than demonstrated.

With regard to the aims of ethnography, Frake (1964:132-133) notes:

> Ethnography...is a discipline which seeks to account for the behavior of a people by describing the socially acquired and shared knowledge, or culture, that enables members of the society to behave in ways deemed appropriate by their fellows. The discipline is akin to linguistics; indeed, descriptive linguistics is but a special case of ethnography since its domain of study, speech messages, is an integral part of a larger domain of socially interpretable acts and artifacts. It is this total domain of 'messages' (including speech) that is the concern of the ethnographer. The ethnographer, like the linguist, seeks to describe an infinite set of variable messages as manifestations of a finite shared code, the code being a set of rules for the socially appropriate construction and interpretation of messages.

In "Cultural Anthropology and Linguistics," Goodenough (1957:167-168) writes concerning ethnography:

> Ethnographic description...requires methods of processing observed phenomena such that we can inductively construct a theory of how our informants have organized the same phenomena. It is the theory, not the phenomena alone, which ethnographic description aims to present.

In his Cooperation in Change, Goodenough (193:284) adds:

> Ethnographic method in anthropology is necessarily concerned with developing techniques for allowing an anthropologist to have the range and kinds of experience needed for constructing a valid model of a public culture within the constraints imposed by time and by his not having been born and reared a member of the community under study.

A valid ethnography, according to Tyler (1969:5), would answer the questions: How would the people of some other culture expect me to behave if I were a member of their culture; and what are the rules of appropriate behavior in their culture?" Tyler (1969:5) adds:

> ...this description itself constitutes the 'theory' for that culture, for it represents the conceptual model of organization used by its members. Such a theory is validated by our ability to predict how these people would expect us to behave if we were members of their culture.

In his "A Structural Description of Subanun Religious Behavior" (1964), Frake (in Tyler, 1969:470) writes:

> The problem is not to state what someone did but to specify the conditions under which it is culturally appropriate to anticipate that he, or persons occupying his role, will render an equivalent performance. This conception of a cultural description implies that an ethnography should be a theory of cultural behavior in a particular society, the adequacy of which is to be evaluated by the ability of a stranger to the culture to use the ethnography's statements as instructions for appropriately anticipating the scenes of the society. I say 'appropriately anticipate' rather than 'predict' because a failure of an ethnographic statement to predict correctly does not necessarily imply descriptive inadequacy as long as the members of the described society are as surprised by the failure as the ethnographer. The test of descriptive adequacy must always refer to informants interpretations of events, not simply to the occurrence of events.

In his paper concerning a program for an ethnography of communication, Hymes (1964:13-14) writes:

> Ethnography here is conceived in reference to the various efforts of Conklin, Frake, Goodenough, Metzger, Romney and others to advance the techniques of ethnographic work and to conceptualize its goal, such that the structural analysis of cultural behavior generally is viewed as the development of theories adequate to concrete cases, just as the structural analysis of behavior as manifestation of a linguistic code

> is viewed. One way to phrase the underlying outlook is as a question of validity...analysis of cultural capabilities generally must determine what sets of features are to be taken as relevant to identification and contrast of cultural behavior on the part of the participants in same.

The major criterion of validity for an ethnographic description, from the cognitivist's point of view, reflects the dictum of Levi-Strauss (1966:113): "Against the theoretician, the observer should always have last word; and against the observer, the native." Colby (1966:12) notes that with regard to cognitive anthropology, the primary means of establishing descriptive validity is simply informant response. He (Colby, 1966:12) adds as an example, "When Hymes speaks of prediction, he means mainly an affirmative informant response to the correct naming of objects in the environment showing that the meaning has been attained by the investigator." Conklin (in Tyler, 1969:93-94) writes:

> Criteria for evaluating the adequacy of ethnographic statements, with reference to the cultural phenomena described, include; (1) productivity (in terms of appropriate anticipation if not actual prediction), (2) replicability or testability, and (3) economy.

The "native orientation" as a foundation for testing and validating stands as a diagnostic feature of all recent anthropological studies of cognition. This position stems, to a great degree, from the cognitivists' view of the role of language in their studies. Cognitive anthropology seeks to reveal how the bearers of a culture classify, categorize, and generally make socially and psychologically significant discriminations in the range of their culturally given experience. For the cognitivists the main evidence for the existence of a category is the fact that it is named (Sturtevant, 1964:106).

Tyler (1964:6) states:

> ...we are interested in the mental codes of other peoples, but how do we infer these mental processes? Thus far, it has been assumed

> that the easiest entry to such processes is through language, and most of the recent studies have sought to discover codes that are mapped in language. Nearly all of this work has been concerned with how other peoples 'name' the 'things' in their environment and how these names are organized into larger groupings. These names are thus both an index to what is significant in the environment of some other people, and a means of discovering how these people organize their perceptions. Naming is seen as one of the chief methods for imposing order on perception.

Goodenough (1963:148) examines his notion of the importance of language in cognitive studies in the following excerpt from his Cooperation in Change.

> Our language provides us with a set of behavioral precepts that serve as a code for our other precepts. It enables us to reduce the rest of experience to a set of coded items and propositions about them. By substituting one item of the code for another in various propositions, we can symbolically create new arrangements of phenomena by analogy with old ones, new arrangements that we have not experienced directly at all....Such analogies bring us to new discernments that we have not perceived in direct experience but have conceived as products of the manipulation of coded experience. These products, our concepts, may be perceivable in sensory experience or may remain, like one's more remote ancestors or like the ether of the nineteenth-century physics, things whose existence can be postulated but never directly observed. Our concepts, once coded as part of our language, can be manipulated along with our precepts to produce even more concepts.

In his article "Notes on Queries in Ethnography," Frake (1964:133) states:

> There are a variety of methods one might use to discover those aspects of cultural situations relevant to rendering appropriate performances... The method considered here attends to the way people talk about what they do. Since the knowledge that enables one to behave appropriately is acquired from other people, it must be communicable in some symbolic system which can travel between one mind and another as code signals in a physical channel. The procedures of this paper seek to reveal the knowledge that is communicated by talking. This may not include everything a person knows which is relevant to his cultural performances, but it will certainly include a sizable chunk of it.

Regarding the basic assumptions of cognitive anthropology, Lounsbury (1963:570) writes:

> The referential classification made through lexicon often vary strikingly from language to language and are seen to exhibit classificatory principles in different languages. It is posited that the

> principles of referential classification embodied in lexical usage in a given speech community bear some relation to their relative utility in communication in that community and to the frequency with which the distinctions implied by them are of crucial significance. This, in turn, it is posited, may be a function of the ways in which a people's social interaction and their activities in relation to their natural and man-made environment are organized. Some of the best cases in support of this hypothesis come from special vocabularies such as those of kinship systems, numeration, ethnobotanical, ethnozoological, and ehtnometeorological terminology, etc.

The importance of language to cognitive anthropology goes much further than the above, which is obvious when the cognitivist notion of the nature of culture and the objectives of ethnography are considered. Language (more correctly, speech) as the subject matter of structural linguistics, perhaps the single most important stimulus to the development of cognitive anthropology, and new approaches in semantics more significantly exhibit and explain the cognitivists' abiding concern with language. When the basic concepts and methods of cognitive anthropology are discussed in a later portion of this section, the heritage of structural linguistics will appear more clearly. A this point it will be advantageous to examine exactly what the cognitivist mean when they claim an interest in the ethnographic study of meaning. When discussing ethnographic semantics, Colby (1966:3) states:

> Ethnographic semantics can be defined...as the study of those aspects of meaning in a language which are culturally revealing. It is directed toward words as a means rather than an end. The ultimate goal is an understanding of the evaluations, emotions, and beliefs that lie behind word usage....The attraction given to minute details of meaning relationships marks a new phase in descriptive ethnography. The techniques are popular because they show promise of solving the problem of ethnographic selectivity; that is, they may lead to psychologically meaningful elements of a culture which are analogous to psychologically meaningful elements of a language (e.g. phonemes).

Two works by Charles W. Morris, Foundations of the Theory of Signs (1938) and Signs, Language, and Behavior (1946), have been of particular significance in giving the cognitivists a terminological system for

indicating precisely which aspects of linguistic behavior hold their concern. Osgood (1963:245) quotes Morris (1946) from Signs, Language, and Behavior with regard to a definition of language:

> What is language? Morris has suggested five necessary criteria: We have a language when (1) a plurality of arbitrary signs (2) having a common or shared significance to a group of individuals (3) regardless of the situation in which they are used (4) can be produced by these individuals as well as received and (5) together constitute a system following certain rules of combination. When ever the stimuli received or the responses produced satisfy these criteria, the psychologist can say he is studying 'verbal' behavior.

Floyd G. Lounsbury (1956:158) in his article, "A Semantic Analysis of the Pawnee Kinship Usage," writes:

> A well known formulation (of the total context surrounding a linguistic event) is that of Charles W. Morris. For any sign system, linguistic or other, there are distinguished: (a) the properties of the signs and their systematic relations to each other, or 'syntactics'; (b) the relations of the signs to their areas of designation and their features of signification, or 'semantics'; and (c) the relations of these to behavior in response to signs, or 'pragmatics'.

With regard to the study of language, the first area of Morris' formulation describes the interest of the science of linguistics proper. For Lounsbury (1956:158) linguistics "...is limited to the analysis of the properties of the signal systems or 'codes' themselves as inferred from the structure of messages, while it excludes on the one hand the primary nonlinguistic stimuli which prompt messages or are coded in them, and on the other hand also the nonlinguistic responses which the messages may evoke." For cognitivists, the second and third portions of Morris' scheme, "semantics" and "pragmatics", best circumscribe the focus of their study.

The conception of "structure" appears often in the literature of cognitive anthropology and in discussions of this literature. The term is used in several different manners; however, the assumption of patterning

and systematic interrelatedness holds throughout the various uses of the term. Indeed the dedication demonstrated by the cognitivists to formal methods of analysis would be difficult to defend without the "systems" assumption.

Lounsbury (1959:400) states that semantic structure revealed by the methods of cognitive anthropology can best be viewed as a model--"which model in turn comes satisfactorily close to being a facsimile or exact replica of the empirical data whose interrelatedness and systematic nature we are trying to understand" (Lounsbury, in Tyler, 1969:212).

Colby (1966:3-3), in his "Ethnographic Semantics: A Preliminary Survey," writes concerning the concept of "structure":

> Structure may mean either an overall cognitive system with an encyclopedic world view behind the linguistic and semantic elements a person carries in his head, or a semantic structure that is independent of such a cognitive system. I shall speak mostly of the broader conception of structure--that which includes both a semantic system and an organized world view.

The term "structure" is also sometimes used in the Levi-Straussian manner of "infra-structure"--the order behind order, the meaning behind meaning, etc. Harris (1968:570) states that structure is the order in a system. Further, "structure" occurs in the cognitive literature in the sense of a "formal" or "logical" account of a certain range of eventing. This approximates the meaning of "structure" utilized by Lounsbury. With regard to this version of the conception of "structure," Wallace (1965:247) writes:

> A set of scientific propositions about human behavior may be more or less 'true' in the sense that they yield accurate predictions of certain future events under specified conditions...The classic method of componential analysis enables the ethnographer to simulate the taxonomic behavior of his subjects. A successful simulation procedure has what I call 'structural validity': it permits accurate prediction of a terminological event because it correctly identifies sufficient sociological, or other objectively defined, characteristics of the reference objects.

The over-all and self-conscious "native orientation" of cognitive anthropology with regard to the structure concept, the culture concept, notions of the nature and objectives of ethnography, and the tenor of validating procedures for ethnographic descriptions is typically presented and discussed by means of the "etic vs. emic" distinction. The terms themselves were coined by the linguist Kenneth Pike (1954:8) on analogy with the "emic" in phonemic and the "etic" in phonetic (Harris, 1968:569). These terms label two different approaches to the study of human behavior, as well as indicate, according to Pike, the kinds of results that can occur depending on the approach. Emic analysis produces structural results: etic analysis, non-structured results.

> Emic statements refer to logico-empirical systems whose phenomenal distinctions or 'things' are built up out of contrasts and discriminations significant, meanigful, real, or accurate, or in some other fashion regarded as appropriate by the actors themselves... Etic statements depend upon phenomenal distinctions judged appropriate by the community of scientific observers (Harris, 1968:571-575).

The emic approach outlines the methodological objectives of cognitive anthropology: it is an attempt "to discover and describe the behavioral system (of a given culture) in its own terms, identifying not only the structural units but also the structural classes to which they belong" (French, 1963:398). Traditional ethnography, according to the cognitivist, exemplifies an etic approach to the description of culture (Sturtevant, 1964:102).

To summarize to this point: for cognitive anthropology, culture is identified with cognition, and thus its focus is in the minds of the bearers of the culture in question. Ethnography is conceived of as the discovery of the conceptual models with which a society operates. "Ethnographic description...requires methods of processing observed phenomena such that we can inductively construct a theory of how our informants

have organized the same phenomena" (Goodenough, 1957:168). Descriptive validity is insured, according to the cognitivists, by informant response. Language, therefore, is the critical avenue of access to the conceptual models which are the objectives of the cognitivists. The means of access through language to the relevant conceptual models is achieved by the cognitivists by a partial use of the linguistic model energized by methods of formal analysis, or logical operations, gleaned from recent advances in such areas as set theory (a calculus of the relations between groups of elements), game theory, communications theory, Boolean algebra (an algebraic calculus for stating logical relationships among classes, semantics, cybernetics, and topology.

The application of the linguistic model in the quest for cognitive models is facilitated by a notion of language as a codable communicative event of wide extension (Morris, 1938:1946; Osgood, 1963). As noted, it includes a stress on "semantics" and "pragmatics," in Morris's sense. The methods of cognitive anthropology are rationalized by adherence to the linguistic model as parameter guide and aim at discovering "...psychologically meaningful elements of a culture which are analogous to psychologically meaningful elements of a language (e.g., phonemes)..."(Colby, 1966:3). Goodenough (1956:196) adds:

> It is an object of linguistic analysis by systematically examining the mutual distribution (in recorded speech) of the acoustical phenomena as phonetically noted, to produce the most adequate possible theory as to what are the language's phonemes, its elementary phonological components.

Claude Levi-Strauss (in Manners and Kaplan, 1968:532), a leading advocate of the utilization of the methods and assumptions of structural linguistics in the solution of certain kinds of "sociological" problems, writes:

> In the study of kinship problems (and, no doubt, the study of other problems as well), the anthropologist finds himself in a situation which formally resembles that of the structural linguist. Like phonemes, kinship terms are elements of meaning; like phonemes, they acquire meaning only if they are integrated into a system. 'Kinship systems,' like 'phonemic systems,' are built by the mind on the level of unconscious thought...The problem can therefore be formulated as follows: Although they belong to another order of reality, kinship phenomena are of the same type as linguistic phenomena. Can the anthropologist, using a method analogous in form (if not in content) to the method used in structural linguistics, achieve the same kind of progress in his own science as that which has taken place in linguistics?

The cognitivists, of course, answer in the affirmative.

As noted above, cognitivists utilize methods of formal analysis and aim at delivering a formal account of their subject matter. Formal analysis is directed to the description of cultural phenomena in unambiguous culture-free language and the demonstration of relationships with precision and parsimony. Concerning "formal accounts," Lounsbury (in Tyler, 1969:212), in his article "A Formal Account of the Crow- and Omaha-Type Kinship Terminologies" (1964), writes:

> We may consider that a 'formal account' of a collection of empirical data has been given when there have been specified (1) a set of primitive elements, and (2) a set of rules for operating on these, such that by the application of the latter to the former, the elements of a 'model' are generated; which model in turn comes satisfactorily close to being a facsimile or exact replica of the empirical data whose interrelatedness and systematic nature we are trying to understand. A formal account is thus an apparatus for predicting back the data at hand, thereby making them 'understandable,' i.e., showing them to be the lawful and expectable consequences of an underlying principle that may be presumed to be at work at their source.

Tyler (1969:191) notes with respect to formal analysis:

> A formal analysis presumes that the items to be analyzed are part of some legitimate semantic domain, and that the data are adequately described. Formal analysis is basically a translation procedure. It seeks to explain the semantic features of one language (the target language) by reference to features whose values are known in some other language (the reference language). The aim of formal analysis is to discover and state the relation of features in the reference language as parsimoniously as possible. Since the features available

> in the reference language, this is a two step procedure which entails: (1) listing the features of the reference language relevant to each semantic category of the language to be translated: (2) elimination of redundant features in the reference language. A third step (is) the arrangement of features...

The first step in the application of formal, or logical analytic techniques as used by cognitivists is to determine the domain or particular universe of discourse to which the formal methods will be applied. Kay (1966:20) states:

> ...we assume...that a basic problem of ethnographic semantics is the following; given a finite set of lexical units ('lexemes') that share some feature of meaning, we say (a) that the set of lexemes form a domain and (b) that our task is to discover something about the formal pattern of meanings underlying the domain.

Every writer in the field of cognitive anthropology is compelled to explicate what he intends by the conception of "domain," perhaps the most crucial notion in the cognitivists' program. All methods and techniques of cognitive anthropology are subsequent to the determination of domain. Tyler (1969:8) writes:

> A semantic domain consists of a class of objects all of which share at least one feature in common which differentiates them from other semantic domains. Chairs, sofas, desks, end tables, and dining tables, have in common the designation 'furniture'.

Conklin (1954, 1962) and Lounsbury (1956) use the term "segregate set" instead of "domain" and state that a terminologically distinguished array of objects is a segregate, and that segregates represent categories. Colby (1966:7) writes:

> A 'lexical set' can be defined rigorously as a group of contrastive words with a defining feature (or component) in common... I shall use the word 'domain' to indicate the conceptualized reality designated by the lexical set--the semantic range...By concentrating on a domain (e.g., kinship) we keep meaning rather than word form foremost and exclude those meanings of the word forms that fall outside the (kinship) domain...A domain considered in terms of its conceptual structure may be called a semantic field.

A domain, then, is a labeled grouping of "things" that are assumed,

largely because of the label, to be of a kind. Some of the domains investigated by researchers in cognitive anthropology include kinship categories, color categoris, botanical categories, firewood categories, and disease categories. However, collecting the lexemes which correspond to the segregates of a particular domain presents only the beginning of this kind of formal analysis. Cognitive anthropologists are interested in generating emicized accounts of cognition. They therefore seek to discover how one knows that such and such an "object" belongs to a certain domain: that A does or does not belong to category B.

It is at this point that the spirit of structural linguistics forcefully appears in the form of the notion of contrast, distinctive features, binary opposition, and components. As Colby (1966:6) explains:

> Phonemes are considered in relation to other phonemes in terms of negative or opposing characteristics. Phoneme /x/ differs from phoneme /y/ in a different way than it differs from phoneme /z/. Concepts, too, have this characteristic. Basic meaning resides in differences among concepts more than in any special inherent quality of the concepts themselves. This has recently been re-emphasized in the analysis of contrast sets and in componential analysis.

Tyler (1969:32) notes:

> In a situation in which a person is making a public decision about the category membership of an object by giving the object a verbal label, he is selecting a term out of a set of alternatives, each with classificatory import. When he asserts "This is an X," he is also stating that it is not specific other things, these other things being not everything else conceivable, but only the alternatives among which a decision was made...Those culturally appropriate responses which are distinctive alternatives in the same kinds of situations--or, in linguistic parlance, which occur in the same 'environment'--can be said to contrast. A series of terminologically contrasted segregates forms a contrast set.

Pursuing Tyler's (1969:8) example noted above, it can be said that chairs, sofas, desks, end tables, and dining tables have in common the designation "furniture;" the domain is the domain of "furniture." The problem, however, is to discover how the segregates in the domain maintain

their terminological autonomy while remaining legitimate members of the same domain. What are the critical differences between a chair and a table, for example? It can be noted that chairs differ from tables by the presence of two attributes, a seat and a back, and the absence of a top. These underlying features are "components" or "features" of meaning, and, as Tyler notes, they are some of the dimensions of meaning underlying the general domain of "furniture." Each segregate (chair, in this example) can be defined as a distinctive bundle of components (Frake, in Tyler, 1969:36) or features of meaning (Tyler, 1969:8).

Cognitivists seek to describe the formal pattern or the precise nature of the semantic arrangements or structures which are assumed to form the meaning-foundation of a domain. Tyler (1969:25) writes:

> ...we must discover that method of arrangement which provides the best statement of relationships...our concern is only with relationships among facts which can be demonstrated to comprise a single domain...We are interested only in the question of internal ordering. Since it is evident that there are a great number of possible semantic features, which may enter into a variety of relations with one another, it might be supposed that orderings too are exceedingly diverse. This statement is true insofar as it pertains to the overall organization of a semantic domain. Yet, it is paradoxical that these diverse organizations appear to result from relatively few principles of ordering. The principles of ordering which cognitive anthropologists have so far dealt with most frequently are: (1) taxonomies; (2) paradigms; (3) trees.

Segregates within a domain may be semantically interrelated in various ways. "The kind of relationship between segregates which has so far received the most attention is that of inclusion; segregates related in this way form a taxonomy--a folk taxonomy in the case of folk classification" (Sturtevant, 1964:110).

Frake (1963, in Manners and Kaplan, 1968:511) writes:

> Segregates in different contrast sets...may be related by inclusion. A system of contrast sets so related is a taxonomy; this definition does not require a taxonomy to have a unique beginner, i.e., a

segregate which includes all other segregates in the system. It requires only that the segregates at the most inclusive level form a demonstrable contrast set.

With regard to the concept of "taxonomy," Colby (1966:21) states:

> A lexical domain may be analyzed with or without reference to the dimensions of meaning (and their component features) that underlie it. When an attempt to describe the underlying dimensions either is not made or is not successful, the semantic analysis is not properly speaking, componential. In this case, the major concept ordinarily used to represent 'something about the formal pattern of meaning underlying the domain' is the notion of inclusion of reference not absence of componential definitions is the distinguishing feature of taxonomy...A characteristic of all taxonomies is that they contain levels of contrast.

Tyler (1969:7-10) adds:

> ...we subjectively group the phenomena of our perceptual world into named classes. These classes are not disparate and singular. They are organized into larger groupings. To the extent that these groupings are hierarchiacally arranged by a process of inclusion, they form a taxonomy...two processes characteristic of taxonomies: (1) items at the same level contrast with one another; (2) items at different levels are related by inclusion. At the bottom level are the more highly discriminated classes, at the top is the most inclusive class...A taxonomy typically asserts that items in lower levels are kinds of items in higher levels.

In a taxonomic arrangement, segregates at the same taxonomic level contrast. However, when they are included in the next more inclusive level of the taxonomy they appear to be more alike. As Tyler (1969:26) notes, "...there must be some reason behind this arbitrary neutralization of difference at higher levels. In general, this corresponds to our intuition that certain things go together because they share some underlying elements." The semantic arrangement termed the "paradigm" accomplishes the task of identifying significant definitive features of meaning within a domain.

A paradigm is a set of segregates which can be partitioned by features of meaning, i.e., a set some members of which share features not shared by other segregates in the same set (Sturtevant, 1964:108).

Contrasting "taxonomical" and "paradimatic" arrangements will aid in revealing their distinctive structures. The example which follow are after Tyler (1969:9-10).

TAXONOMY OF "LIVESTOCK"

cattle	horse	sheep	swine
cow	mare	ewe	sow
bull	stallion	ram	boar
steer	gelding	wether	barrow
heifer	filly	lamb	gilt
calf	colt		shoat
	foal		piglet

PARADIGM OF FEATURES FOR "HORSE" AND "SWINE"

MATURITY		SEX		
		MALE ♂	FEMALE ♀	NEUTER ⚲
	Adult M-1	stallion H boar P	mare H sow P	gelding H barrow P
	adolescent M-2		filly H gilt P	
	child M-3	colt H shoat P		
	baby M-4	foal H piglet P		

Because this diagram exhibits at least two major features (maturity and sex) which intersect, it is a paradigm. "Features are paradigmatically arranged when they are: (1) multiple; (2) intersect (Tyler, 1969:10)." As noted above, a taxonomy arranges its components by contrast and inclusion;

its components are mapped hierarchically. A paradigm orders its components in terms of simultaneous intersection. Both a taxonomy and a paradigm, however, order their components on the basis of sameness and difference.

Paul Kay (1966:21) notes:

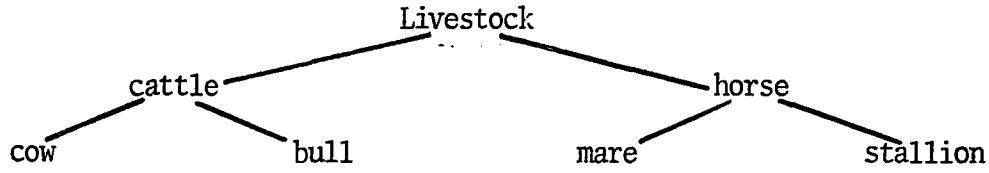
> The semantic structure of a domain is characterized by a perfect paradigm if and only if each componential definition corresponds to a unique minimal classification event, and conversely...Perfect paradigms have zero redundancy in the sense that a change in a single feature of a componential definition changes it into the componential definition of another lexeme in the domain. It is probably for this reason that perfect paradigms are empirically rare.

The example used of the paradigm of features for "horse" and "swine" is not a perfect paradigm in Kay's sense because, for example. "P♂M-3" and "P♀m-3" are both componential definitions of the lexeme "shoat." In this instance zero redundancy is not achieved.

"In polar opposition to the minimally redundant (paradigmatic) system of feature definitions, there is a maximally redundant system in which no two componential definitions contrast on more than one dimension" (Kay, 1966:21). This type of semantic arrangement is called a "tree." Tyler (1969:26,10) writes:

> ...the features in a tree do not intersect one another simultaneously and they contrast on only one dimension at a time. Relationships in a tree are expressed as dichotomous oppositions selected one at a time...Unlike a paradigm, the features of a tree do not intersect, and unlike a taxonomy items at lower levels are not included in higher level.

A tree requires representation by a semantic key or branching structure where the first node indicates the "root" or domain feature, and each succeeding node represents a selection of a single feature from some particular dimension (Kay, 1966:22).



KEY DIAGRAM OF THE TREE STRUCTURE

Kay (1966:20) makes the following interesting point concerning when and why the various semantic arrangements discussed above are utilized by the analyst of a particular domain.

> When the feature definitions of all the lexemes are known, the basic problem for representing the cognitive structure of the domain is to decide whether, or to what extent, informants apply the semantic dimensions simultaneously as against sequentially...The simplest and most elegant formal structure consonant with a psychological theory of simultaneous application of dimensions is the paradigm... Paradigms can also be represented by keys, but there is no reason to represent them unless there is behavioral--as contrasted to linguistic-cultural--evidence that the dimensions are in fact applied sequentially.

The mapping of a domain in which the conceptual segmentation and hierarchical levels are indicated by lexical units is a preliminary step for another, more detailed, analysis in which the relevant (i.e., domain related) signification of each unit on a given level is analyzed into components or distinctive features (Colby, 1966:8). This kind of operation is called "componential analysis." Regarding componential analysis, Paul Kay (1966:20) writes:

> Componential analysis is best conceived as an analytic process in which the investigator searches for (1) the dimensions of meaning underlying the domain and (b) the mapping of the values on these dimensions (the features of meaning) onto the set of lexemes. The process of looking for these mappings is not to be confused with particular types of such mappings such as paradigm and tree.

The first use of componential analysis in cognitive anthropology was by Goodenough (1956) and Lounsbury (1956). The domain of kinship has been the realm most often attacked by cognitivists utilizing this method. Wallace and Atkins (1960:60) note that the componential analysis

of a kinship lexicon commonly consists of the following steps:

> (1) the recording of a complete set (or a defined sub-set) of the terms of reference or address, using various boundry-setting criteria, such as a constant syntactic context, a type of pragmatic situation, or common inclusion within the extension of a cover term for 'kinsmen'; (2) the definition of these terms in the traditional kin-type notions (i.e., Fa, FaBr, DaHuBr, etc); (3) the identification, in the principles of grouping of kin-types, of two or more conceptual dimensions each of whose values ('components') is signified (not connoted) by one or more of the terms; (4) the definition of each term by means of a symbolic notation, as a specific combination or set of combinations, of the components; (5) a statement of the semantic relationship among the terms and of the structural principles of this terminological system. (It should be noted here that the semantic structure of the terminological system is only one aspect of the 'kinship system' of a society. The semantic structure to which we refer is a structure of the logical relationships of definitional meanings among terms and does not pretend to describe such phenomena as marital exchange, or authority relations.)

Sturtevant (1964:109) offers the following description of the method of componential analysis:

> A componential analysis is an analysis of a paradigm in terms of the defining features, the 'dimensions of contrast' or 'criterial attributes' of the segregates in the set. The aim is to discover the 'rule for distinguishing newly encountered specimens of (a) category from contrasting alternatives' (Frake, 1963:512). The procedure is to search for the minimum features of meaning which differentiate segregates in the set. Each feature has two or more contrasting values, termed 'components'. Each segregate is then defined in terms of the presence or irrelevance of each component; i.e., a bundle of components defines the segregates. It is normally assumed that the number of segregates they define. The paradigm may then be viewed as a multidemensional structure, in which the categories are placed according to the componential dimensions.

Componential analysis, a formal method of description first developed by linguists, is applied, for example, to the study of kinship terminology to produce descriptions that are abstract, structural, and formally elegant. The immediate product of such a terminological analysis is a series of componential definitions. What is revealed by a structural analysis of these componential definitions is, to paraphrase Romney and D'Andrade (1964a:152), based on "taste, previous knowledge of the system, emphasis on core kin types, and other factors."

Colby (1966:8-9) notes that beyond better semantic specification, the purpose of componential analysis is to find conceptual units (Goodenough, 1956) or to reveal the structure of the logical calculus which is employed in the given taxonomy associated with the terms (Wallace, 1962). Another objective, mentioned by Lounsbury, is to discover the structure of non-linguistic behavior. Goodenough and Wallace emphasize psychological correlates in componential analysis while Lounsbury speaks more of sociological correlates. An additional possibility in componential analysis is to go beyond the conceptual distinctions embodied in a set of lexical items to concepts that are not lexically objectivized (Goodenough, 1956). A further possibility, implied in the paper by Wallace and Atkins, "The Meaning of Kinship Terms" (1960), is the facilitation of hypotheses testing. In this instance, Wallace and Atkins performed a componential analysis of American-English kin terms to test, as one facet of their total problem the hypothesis that the dimensions of sex, generation, and lineality would be sufficient to define componentially all the terms in the kinship domain under study.

How is the data utilized by the cognitivist acquired? Discussions of this problem area usually fall under the rubric "Discovery Procedures" or "Eliciting Procedures." Sturtevant (1964:111) notes:

> Since the ethnoscientific method aims at discovering culturally relevant discriminations and categorizations, it is essential that the discovery procedures themselves be relevant to the culture under investigation...If an ethnography is to reflect the cognitive system of the bearers of a culture, the validity of the description depends on the discovery procedures.

"Working with informants, one can learn something of the boundaries and dimensions of synonyms or related words by distributional frame-and-substitution techniques" (Colby, 1966:11). Another eliciting method

utilizes deliberate error by the ethnographer in naming stimulus objects in order to evoke corrections from the informant which presumably will occur at the same contrast level as the erroneously used lexeme (Frake, 1963). The eliciting procedures used by cognitivists are self-consciously aimed at eliminating ethnographer bias by discovering how the bearers of a culture ask questions, make corrections, and, in general, talk about a particular category of "objects" which the ethnographer assumes is a culturally valid domain.

The major thrust in the development of rigorous discovery procedures by cognitivists is concerned with discovering emically appropriate questions related to a particular domain. Frake's explication of interlinked topics and responses of queries in Subanun is an excellent example (Sturtevant, 1964:112).

> Sarles (1963) describes a related procedure, in the case applied to Tzotzil, for identifying questions and their responses in conversational texts, determining acceptable permutations of the questions, and manipulating these to discover classes of appropriate responses.

Tyler (1969:12-13) makes the following comment when discussing controlled eliciting:

> Controlled eliciting utilizes sentence frames derived from the language of the people being studied. The aim of such eliciting is to enable the ethnographer to behave linguistically in ways appropriate to the culture he is studying. This involves the use of linguistically correct questions which relate concepts meaningful in that culture...Controlled eliciting...is designed to provide the ethnographer with not only the answers, but also to assist him in discovering the relevant questions. It clearly derives from the fact that the questioning process is itself the dominant factor in scientific investigation. Where the procedures and results of controlled eliciting are contained in the report, two things are achieved: (1) there is an explicit record of how the data were gathered; (2) a public record of the results is available.

Black and Metzger (1956:145) add:

> It is basic to communication theory that you don't start getting any information from an utterance or event until you know what it is in

> response to--you must know what question is being answered. It could be said of ethnography that until you know the question that someone in the culture is responding to you can't know many things about the response. Yet the ethnographer is greeted, in the field, with an array of responses. He needs to know what question people are answering in their every act.

Metzger and Williams, in a series of papers published between 1962 and 1963, have stressed the discovery, selection, and use of question "frames" appropriate for eliciting specific folk classifications (Sturtevant, 1964:112). They emphasize the recording of both the question and the response by the field researcher. With regard to the work of Metzger and Williams in this area, Colby (1966:11) writes:

> A method developed by Metzger and Williams, modelled to some extent on programmed learning techniques, aims at reducing ambiguity and ethnographic bias by forcing the ethnographer systematically to learn correct word usage in a specified domain of the language. The ethnographer's question (eliciting frame) comes from previously recorded native textual materials, to insure that the phrasing is indigenous. The process, in the form of verbatum statements of both ethnographer and informant, is presented as evidence so that the reader can judge for himself...the exact questions put to the informant are added to the record of his answers.

In this chapter I have attempted to develop a type statement of the field of cognitive anthropology by describing concepts, methods, and assumptions generic to this approach. To this end I have avoided discussion which employed specific ethnographic examples from the various cognitive studies. It should also be noted that no single cognitive study employs all the notions and methods presented in this chapter. All studies in cognitive anthropology, however, make use of a similar conception of culture, the objectives of ethnography, and the critical importance of linguistic techniques. Further differences in this field appear specifically with regard to the problem of the psychological validity of the products of componential analysis. Other problems of this kind will be discussed in terms of a general critique in Chapter V.

III

HISTORICAL PERSPECTIVE

Sturtevant (1964:99), in his article "Studies in Ethnoscience," refers to the new approaches in ethnography, which I have termed in blanket fashion cognitive anthropology as "the New Ethnography." As part of a general argument against the positions held by "the New Ethnography" and in order to point to the long history of many of its most basic notions, Marvin Harris (1968:597) has called this area of interest "the new old ethnography." Both statements approximate a portion of the truth. There are facets of cognitive anthropology which extend new ideas and methods in the pursuits of ethnographic description: there are also features of cognitive anthropology which are ancient in the history of the fields of sociology and anthropology. In this chapter I will indicate some of the sources which doubtless contributed to the development of the contemporary sub-field of cognitive anthropology. In this manner I hope to distinguish what is new from what is old in cognitive anthropology.

The bundle of ideas, which are now systematically exploited by cognitive anthropology, includes the notions: (1) that an important aspect of culture is made up of the principles by which a people classify their universe (Sturtevant, 1964:100), (2) that language provides the main avenue of access to discovering the ways in which the members of a particular culture perceive and organize their experience, (3) that a valid ethnography must be an emic ethnography, (4) that the linguistic model affords the best means through which emic accounts may be acquired,

and (5) that cultural descriptions should be structural and synchronic. These are some of the core ideas which I will at least partially trace in this chapter.

Hymes (1964b:12) makes the following interesting point:

> The transcultural study of cognition is a thread running through the history of thought about mankind. It is concerned with phrasing in one way or another the question, do they and we think the same?

According to Benjamin Lee Whorf, Antoine Fabre d'Olivet (1768-1825) must stand as one of the earliest pioneers in the linguistic approach to cognition. Concerning d'Olivet, Whorf (in Hymes, 1964c:134) writes:

> ...one of those amazing geniuses who baffle their contemporaries and leave no successors. The real originator of such ideas as rapport-systems, covert classes, cryptotypes, psycholinguistic patterning, and language as part and parcel of a culture...

D'Olivet was a French grammarian who studied Semitic languages. With regard to d'Olivet's study of Hebrew, Whorf (in Hymes, 1964c:134) states:

> His Hebrew stands on its own feet as completely as does Boas' Chinook. He reorganized the treatment of verb conjugations on a psycholinguistic basis, considered individual prefixes and suffixes from the standpoint of their meaning and function, went into the semantics of vowel patterns and the semantic coloring of vowels... Refusing to identify the letters of Hebrew writing with the actual phonetic elements and yet perceiving that these elements are not mere sounds, but stereotyped, codified, and patterned semantic sounds, he advanced to a conception of the phoneme, which he called the 'sign' or the 'vocal sign'--struggling with terminology but showing real insight into linguistic actualities...Moreover, Fabre d'Olivet thought in an anthropological and not simply a grammatical way; to him, speech was not a 'faculty' exalted on its own perch, but something to be understood in the light of human behavior and culture, of which it was a part, specialized but involving no different principle from the rest. The vocal sign (phoneme) was a highly specialized gesture or symbolic act, language a development of total somatic behavior becoming symbolic and then diverting its symbolism more and more into the vocal channel--such is his teaching put into the modern idiom.

Fabre d'Olivet's Hebrew study, La langue hebraique restituee (The Hebraic tongue restored), was published in 1815. At this same time two

other authors, Claude Henri duc de Saint-Simon and his secretary Auguste Comte, were propounding their philosophy concerning the proper method of studying society and social history. D'Olivet was probably as greatly influenced by the tremendous scientific advancement of the time as was Saint-Simon and Comte. It should be remembered that the early 19th century was the age from which the birth of modern physics, chemistry, and biology, as well as sociology can be dated. John Dalton's atomic theory was applied to chemistry around 1805. The discovery of the transverse wave-motion of light was made in the first decade of the 1800's. Curier studies the laws of structure in living organisms, and Schwann and Schleiden announced the cell-theory of animals and plants--all in the first several decades of the 19th century. The major scientific accomplishments of this period, which were reflected in d'Olivet's work, shared certain common traits which were also to be incorporated into a science of society by Saint-Simon and Comte. These landmark achievements were all, in Comte's terms, 'statical,' i.e., they were synchronic and structural in orientation. Furthermore, they depended upon a 'positif' or scientific method of approach, i.e., they were systematic and empirical studies. The organismic analogy, energized no doubt by the breakthroughs in cell-theory and medicine, offered, according to Comte and Saint-Simon, the model by which society might be studied. Comte viewed society as being composed of differentiated interdependent elements which functioned together for the maintainance of the total organism. Saint-Simon observed that industrialization, an economic process, was instigating change in every aspect of society. In this fundamental sense structuralism was born. D'Olivet's study of Hebrew also illustrates an early structural and functional approach, both in his conception of the

'sign' and in his notion that language was to be best understood in its interrelation with human behavior and culture.

Two more notions make Saint-Simon and Comte significant in the stream of thought and events which form the background to contemporary cognitive anthropology. Although they recognized the statical and dynamical (structure and process) as describing the boundries of their new science, they stress the statical, or structural approach. Further, Saint-Simon was the first to utilize the idea which he termed "Collective Being." This notion was to appear in a more refined manner in the work of Durkheim and Mauss as "collective representation." For Saint-Simon the "Collective Being" notion was religious in tone, though it did stand for a syndrome which was, in his words, "the essence of history." It can also be suggested that the concept, "Collective Being," may mark Saint-Simon's struggle toward a concept of culture--a concept which, with the deletion of "being" and the addition of "structure," would not be that dissimilar from the general culture concept of cognitive anthropology.

Another idea of the 19th century which casts its shadow over cognitive anthropology is that of the psychic unity of mankind. Briefly, it is the belief that the human mind is and was the same everywhere. "In the formulation of Adolf Bastian, psychic unity was freely invoked to explaitn similarities of culture wherever they occurred" (Harris, 1968:137). This idea has survived intact in the sciences of man as a vital facet of the theory systems of such giants as Jung, Freud, and Levi-Strauss.

A brief survey of the channels of research in 19th century linguistics also indicates further aspects of the background of cognitive anthropology. Linguistic research at that time was, roughly speaking, proceeding in two directions. One of these directions, connected with such men as

Franz Bopp, Rasmus Rask, and August Schleicher, was historically oriented. The characteristic feature of the scholars belonging to this current was their effort to penetrate as far as possible into the pre-history of language and to reconstruct sounds and word-forms of the non-preserved, prehistorical stages of language by comparing the earliest preserved documents of languages that had been developed from the non-preserved parent languages (Vachek, 1966:15-16).

The second major tradition, associated with such men as Wilhelm von Humboldt, Steinthal, Misteli, Finck, and Gaklentz, viewed language as a phenomenon hic et nunc and attempted to study it by nonhistorical methods. Trinks (1948), in his "Linguistics and The Ideological Structure of the Period" (in Vachek, 1966:159), notes that this general line of linguistic research was heavily influenced by the philosophical systems of Herder and Kant.

Humboldt conceived of languages as representing a mental system, or mental image of the world (Colby, 1966:3-4). Humboldt's general notion has appeared in recent time under various names; e.g., cognitive map, cognitive structure, image, eidos, model, mazeway, infra-structure. Goodenough, for example, has acknowledged his debt to Wilhelm von Humboldt.

The tradition of nonhistorical approaches to the study of language, a tradition first identified with Wilhelm von Humboldt, was continued and modified by Vilem Mathesius and his associates. In 1911, Mathesius made a convincing plea before the Royal Czech Learned Society for the synchronistic approach to language phenomena thus anticipating Ferdinand de Saussure's Cours de linguistic generale by five years (Vachek, 1966:4). Mathesius' methods and ideas were to be responsible for the epithet 'structuralist' by which the Circle Linguistique de Prague, founded by

Mathesius in 1926, was to be known. Mathesius' notion of "structure" indicated his position that no element of language could be correctly evaluated if considered in isolation from the other elements of that same language. Mathesius and the Prague School also utilized the concept of language function to indicate their position that language exists for the purpose of communication. The Prague group exhibited a steady concern for meaning, by which they intended what is often termed content, or more exactly, the references made by an utterance and by the parts composing it to what we call extralinguial reality (Vachek, 1966:30). This reference is what the Prague linguists understood by the function of language.

Mathesius, (1936, In Vachek, 1966:144) in his "Ten Years of the Prague Linguistic Circle," noted: The functionally and structurally oriented analysis of speech was also to establish our close connection with the tradition of Jan Baudouin de Courtenay (Russian)...as well as with American linguistics represented mainly by Edward Sapir." Pursuing the European current, it can be observed that de Courtenay and the Russian linguistic tradition which he fostered exerted a great influence on the theoretical direction taken by Jakobson and Troubetzkoy, the most rigorous early formulators of the structuralists approach in linguistics. Scerba and Fortunatov also exerted powerful influences on Jakobson and Troubetzkoy. Perhaps the most prominant event in the development of many of the ideas of Jakobson and Troubetzkoy, however, was Ferdinand de Saussure's Cours de linguistic generale, published in 1916.

A major contribution of Saussure's is that his work served as a bridge between the current of French structuralism, which follows a line highlighted by Comte, Durkheim, Mauss, and Levi-Strauss, and the development

of structural linguistics out of the Prague Linguistic Circle. Saussure stands close to being the first cognitive anthropologist. His influence was perhaps most responsible for stimulating the application of linguistic methods to extra-linguistic phenomena.

Saussure studied the problem of language in terms of what he called "semiologie." He saw semiology as the study of the common reference plane underlying both language and culture (Colby, 1966:6). Saussure felt that myth, kinship, rituals, and customs, as well as language proper could be treated as signs. The signs to an infra-structure which formed the parameters of both linguistic and extra-linguistic phenomena. His notion of contrast was the focal point of semiology. Saussure felt that signs gain their significance by their distinctiveness from other signs, rather than by any inherent quality residing in each sign. He used the term 'phoneme,' although it does not closely resemble 'phoneme' as it is used at present. Saussure says expressly, "les phonemes sont avant tout les entities opposivites, relatives, et negatives" (in Vachek, 1966:19). Josef Vachek (1966:19) states that it was Saussure's concept of "les phonemes" and his stress on synchrony, rather than diachrony, which was his major influence on the early development of structural linguistics in Prague. It was Saussure who first insisted in stringent fashion on the distinction between what he called "synchrony," the study of language in its static state, and "diachrony," the study of language in its evolutionary stages (King, 1969:2). Saussure was also responsible for the distinction in linguistics between "la langue" and "la parole." This classic distinction corresponds to the division of language into syntactics and semantics on the one hand, and pragmatics on the other (Hymes, 1964c:28).

Influenced by Saussure, the Russian tradition, and the Prague Circle,

Troubetzkoy, the illustrious founder of structural linguistics, according to Levi-Strauss, advanced to a conception of the phonological method. These influences, plus Troubetzkoy's additions, can be seen in the outline of the four fundamental steps in the phonological method (Levi-Strauss, in Manners and Kaplan, 1968:531-532).

> First, structural linguistics shifts from the study of conscious linguistic phenomena to study of their unconscious infrastructure; second, it does not treat terms as independent entities, taking instead as its basis of analysis the relations between terms; third it introduces the concept of system;...finally, structural linguistics aims at discovering general laws, either by induction or by logical deduction, which would give them an absolute character. Thus, for the first time, a social science is able to formulate necessary relationships. This is the meaning of Troubetzkoy's last point, while the preceeding rules show how linguistics must proceed in order to attain this end.

Roman Jakobson's major contribution to structural linguistics and cognitive anthropology is noted by Harris (1968:493-494):

> ...to demonstrate the systematic nature of the set of phonological contrast employed by each language in building its repertory of significant sounds. The structure of such a system cannot be described by a simple linear catalogue of the significant sounds; the structure consists rather of the matrix or network of oppositions in which binary groupings of sound differences take their position in a multidimensional space.

The above, of course, describes the semantic arrangement termed "paradigm." Jakobson was the first to develop the method of componential analysis. Sturtevant (1964:113) writes:

> Analysis in terms of semantic components was first applied to paradigms of affixes, particularly to sets where the components are at least something over, i.e., components with separate phonemic identities. In these instances, the contrast set is defined morphologically, in terms of its linguistic environment. The first development of the method is due to Roman Jakobson, who applied them in an analysis of the semantic components of the Russian case system (in 1936).

A year later Troubetzkoy made a componential analysis of the Slovak case system.

Jakobson was responsible for introducing into America the concepts

and procedures of the Prague school of structural linguistics and the emphasis on "binary opposition" and "distinctive features" (Leach, in Manners and Kaplan, 1968:544). Jakobson's major work was the development of the theory of binary opposition in which all distinctive features are supposed to participate (Vachek, 1966:49). According to Harris (1968:493), Levi-Strauss' notions of binary contrast analysis stems from his contact with Roman Jakobson while both were teaching at the New School.

As has been noted, Jakobson, Troubetzkoy, and the Prague Circle all owe a debt to the innovative work of Saussure. Saussure's presence is also felt in the line of thought stemming from Durkheim and Mauss. It is significant that Mauss and Saussure were contemporaries. Further collaborative possibilities appear when it is noted that Mauss was a student of Durkheim, and Levi-Strauss was a student of Mauss. The continual convergence of French structuralism and structural linguistics is probably the single most significant fact leading to the development of the varieties of contemporary cognitive anthropology.

For the purposes of this chapter, Durkheim's conception of "collective representations" and/or "collective consciousness" is most important in noting his place in the general line of development toward modern forms of cognitive anthropology. In Elementary Forms of The Religious Life, Durkheim (1915:444) writes:

> ...the collective consciousness is the highest form of the psychic life, since it is the consciousness of the consciousness. Being placed outside of and above individual and local contingencies, it sees things only in their permanent and essential aspects, which it crystallizes into communicable ideas. At the same time that it sees from above, it sees farther; at every moment of time, it embraces all known reality; that is why it alone can furnish the mind with the moulds which are applicable to the totality of things and which make it possible to think of them. It does not create these moulds artificially; it finds them within itself; it does nothing but become conscious of them.

Durkheim's "moulds" appear very close to the more modern conceptions of "model," "infra-structure," "cognitive map," etc. It is Durkheim's conception of "collective representations" which had the greatest influence on his devoted pupil Marcel Mauss.

In Mauss' most influential work, L'Essai Sur le Don (The Gift) published in 1924, he:

> ...endeavored to reduce the worldwide varieties of gift-giving practices, including potlatch, the Kula, Melanesian and Indian feasting and festivals, to their 'elementary form.' In conformity with the standard practice of Durkheim's school Mauss is able to discern in these apparently disparate phenomena an underlying principle which is supposed to render them, at one fell stroke, intelligible. All of these phenomena are examples of an 'archaic' form of exchange in which there is a 'circulation of objects side by side with the circulation of persons and rights.' This circulation is maintained neither by barter, purchase, nor economic utility, but rather by the threefold obligation deeply ingrained in the human mind to give, to receive, and to repay (Harris, 1968:486).

Mauss' conception of "elementary forms" is closer to modern conceptions of cognitive structure than Durkheim's notion of collective representations, moulds, forms, etc. because Mauss was operating with a close degree of rapport with assumptions drawn from psychology (Harris, 1968:484) and linguistics (Levi-Strauss, in Hymes, 1964c:40). Durkheim insisted upon a distinct separation between the collective and the individual mind. However, as Harris (1968:484) notes, there are many passages in Mauss' The Gift which could be construed as being concerned with the conscious and unconscious meanings of gift-giving from the individual actor's point of view.

According to Mauss' student, Levi-Strauss, Mauss is to be credited with the recognition that there are hidden inner 'structures' of the mind which are causally prior to collective representations as objective social facts.

The special achievement of The Gift is related to this reorientation

> of functionalism toward 'unconscious mental teleology.' What really set Levi-Strauss' heart beating and head boiling in The Gift was that Mauss had achieved the threshold of a specific discovery concerning the 'unconscious teleology of the mind' which was to provide the basis for The Elementary Structures of Kinship (Levi-Strauss, 1949) and the entire pattern of French 'structural' anthropology (Harris, 1968:486).

Leach (in Manner and Kaplan, 1968:542) writes:

> Sociologists (and social anthropologists) are concerned with 'man in society,' with systems of relationships rather than with individuals in isolation. Mauss' insight was to recognize that the concept of 'relationship' is itself an abstraction from something quite concrete. We say of two individuals that they are 'in relationship' when we see that they are in communication, that is when they pass 'messages' to one another, and these messages are conveyed through material media, sound waves in the air, ink scribbles on a piece of paper, the symbolic value embodied in a gift of flowers. The 'gift,' that is to say the material thing which passes from one individual to the other, is an 'expression' of the relationship, but the quality of the relationship is something both more abstract and more mysterious...this theme links up directly with (Levi-Strauss') view that in any cultural system the conventional modes of person to person interaction constitute a language which can be decoded like any other language...he applied the same kind of argument to all kinds of conventional action and also to the thematic symbols which appear in myth and ritual.

Mauss defined the target for Levi-Strauss' work, and structural linguistics provided Levi-Strauss with the method and the model for finding his mark. Levi-Strauss (in Hymes, 1964c:41) enthusiastically states: "Phonology cannot fail to play for the social sciences the same revitalizing role that nuclear physics, for example, played for the exact sciences." In his "Structural Analysis in Linguistics and in Anthropology" (in Hymes, 1964c:41), Levi-Strauss writes:

> In the study of problems of kinship (and undoubtedly also in the study of other problems), the sociologist is in a situation exactly like that of the linguist in phonology; like phonemes, kinship terms are elements which have a signifying function; like them, they acquire this function only by being integrated into systems; 'kinship systems,' like 'phonological systems,' are elaborated by the mind at the level of unconscious thought; finally, the recurrence, in distant regions of the world and in profoundly different societies, of forms of kinship, rules of marriage, attitudes similarity prescribed between certain types of kin, etc., leads one

> to believe that, in the one case as in the other, the observable phenomena result from the play of general, but hidden, laws. The problem can then be formulated in the following fashion: in another order of reality, the phenomena of kinship are phenomena of the same type as linguistic phenomena. Can the sociologist, utilizing a method analogous in form (if not in content) to that introduced by the phonologist, bring about in his science a progress like that which has just taken place in the linguistic sciences?

Edmund Leach, (1965, in Manners and Kaplan, 1968:546) in his "Claude Levi-Strauss--Anthropologist and Philosopher" (1965), offers the following observation when commenting on Levi-Strauss' first major work, Les Structures Elementaires de la Parente (1949):

> ...though Structures is best regarded as a splendid failure it does contain one fundamental idea of great importance; this is the notion, distilled from Mauss and Freud and Jakobson, that social behavior (the transactions which take place between individuals), is always conducted by reference to a conceptual scheme, or model in the actor's mind of how things are or how they ought to be. And the essential characteristic of this model is that it is logically ordered. Levi-Strauss recognizes that the actual behavior or actual individuals may be full of irregularity and improvisation, but these practices are nevertheless an expression of the actor's orderly ideal scheme just as the ideal scheme is itself a programme for action produced by the praxis of the whole society. As his ideas have developed Levi-Strauss has come to see himself more and more as being concerned with the logical structures which are to be found not in the empirical facts themselves but at the back of the empirical facts.

Turning to developments in America which proved to be part of the system of currents leading to cognitive anthropology, it can be reiterated that Vilem Mathesius, founder of the Prague Linguistic Circle, indicated Edward Sapir in America as taking essentially the same tact in language studies as the Prague group. The work of Sapir's teacher, Franz Boas, offers some early insights into American notions concerning language and thought and the general program of linguistics.

Benjamin Whorf (in Hymes, 1964c:129) writes:

> ...the problem of thought and thinking in the native community is not purely and simply a psychological problem. It is quite largely

> cultural. It is more-over largely a matter of one especially cohesive aggregate of cultural phenomena that we call a language. It is approachable through linguistics and, I hope to show, the approach requires a rather new type of emphasis in linguistics... Boas enunciated it decades ago in his introduction to the Handbook of American Indian Languages (1911).

Boas' introduction to the Handbook proposed that an important aspect of culture is made up of the principles by which a people classify their universe (Sturtevant, 1964:100); and "...showed for the second time in history, but for the first in a scientific manner, how a language could be analyzed sui generis and without forcing the categories of 'classical' tradition upon it " (Whorf, in Hymes 1964c:136). Boas' emic orientation prevailed throughout his work. For him "...the definitive test of a good ethnography was whether or not it faithfully mirrored the world of the natives as the native saw it" (Harris, 1968:316). Two quotations from Boas' (Boas, in Hymes, 1964c:19, 22) "Linguistics and Ethnology" prove instructive on these points.

> Of greater positive importance is the question of the relation of the unconscious character of linguistic phenomena to the more conscious ethnological phenomena. It seems to my mind that this contrast is only apparent, and that the very fact of the unconsciousness of linguistic processes help us to gain a clearer understanding of the ethnological phenomena, a point the importance of which can not be underrated. Thus it appears that from practical, as well as from theoretical, points of view, the study of language must be considered as one of the most important branches of ethnological study, because, on the one hand, a thorough insight into ethnology can not be gained without practical knowledge of language, and on the other hand, the fundamental concepts illustrated by human languages are not distinct in kind from ethnological phenomena; and because, furthermore, the peculiar characteristics of languages are clearly reflected in the views and customs of the peoples of the world.

Boas' theoretical particularism is reflected by Stephen A. Tyler (1969:14), when in the introduction to his Cognitive Anthropology he writes:

> What we need is a more limited notion of culture which stresses theories of culture. Rather than attempt to develop a general theory of culture, the best we can hope for at present is particular

> theories of culture. These theories will constitute complete accurate descriptions of particular cognitive systems. Only when such particular descriptions are expressed in a single metalanguage with known logical properties will we have arrived at a general theory of culture.

Boas' students, Alfred Kroeber, Edward Sapir, and Ruth Benedict, also hold important positions in the general trend that has culminated, in one of its aspects, in cognitve anthropology. Among Boas' students, Edward Sapir is perhaps the greatest contributor to the modern field of cognitive studies. Benjamin Whorf (in Hymes, 1964c:136), a student of Sapir, enthusiastically states, "Sapir has done more than any other person to inaugurate the linguistic approach to thinking and make it of scientific consequence, and moreover to demonstrate the importance of linguistics to anthropology and psychology."

Perhaps the most often quoted statement of Sapir which relates to the development of the famous Sapir-Whorf hypothesis and to our discussion of historical perspectives on cognitive anthropology comes from Sapir's article, "The Status of Linguistics as a Science" (1929:209).

> Human beings do not live in the objective world alone, nor alone in the world of social activity as ordinarily understood, but are very much at the mercy of the particular language which has become the medium of expression for their society...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. No two languages are ever sufficiently similar to be considered as representing the same social reality. The worlds in which different societies live are distinct worlds, not merely the same world with different labels attached.

Sapir (in Hymes, 1964c:128) discusses the relation between language and experience in a way that is identical to the idea on this subject maintained by cognitive anthropology.

> Language is not merely a more or less systematic inventory of the various items of experience which seem relevant to the individual, as is 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. In this respect language is very much like a mathematical system, which, also, records experience, in the true sense of the word, only in its crudest beginnings but, as time goes on, becomes elaborated into a self-contained conceptual system which previsages all possible experience in accordance with certain accepted formal limitations.

Dell Hymes (1964c:7) in his "Directions in (Ethno-) Linguistic Theory" views Sapir and DeSaussure as the two great pioneers in the linguistic approach which had as its leitmotifs synchrony and the imminence and autonomy of linguistic form. Yakov Malkiel (1959:133) writes:

> The dramatic re-discovery of Sapir which, one ventures to predict, has just begun to gather momemtum, marks a return not to such surface phenomena as mentalism, conjectural psychology, poetization of knowledge, but to a fuller, less schematic grasp of the facts of language in all its dimensions and layers, nuclear and peripheral alike, embedded in the broader facts of culture.

The previously discussed "etic/emic" distinction is regarded by Pike, its contemporary originator, as having been anticipated by Sapir. Sapir (Quoted in Pike, 1954:9-10) writes:

> It is impossible to say what an individual is doing unless we have tacitly accepted the essentially arbitrary modes of interpretation that social tradition is constantly suggesting to us from the very moment of our birth. Let anyone who doubts this try the experiment of making a painstaking report of the actions of a group of natives engaged in some activity, say religious, to which he has not the cultural key. If he is a skilled writer, he may succeed in giving a picturesque account of what he sees and hears, or thinks he sees and hears, but the chances of his being able to give a relation of what happens, in terms that would be intelligible and acceptable to the natives themselves, are practically nil. He will be guilty of all manner of distortion; his emphasis will be constantly askew. He will find interesting what the natives take for granted as a casual kind of behavior worthy of no particular comment, and he will utterly fail to observe the crucial turning points in the course of action that give formal significance to the whole in the minds of those who do possess the key to its understanding.

Sapir utilized the method of componential analysis in a study of English totalizers in a paper entitled "Totality," published in 1930. Colby (1966:8) describes this important study.

> A 'totalizer' is any term expressing a quantitative judgment... whose function it is to emphasize the fact that in the given context the quantifiable is not to be thought of as capable of increase, e.g., all, the whole flock. Using sixteen categories of totalizers, Sapir made a classification based on the following four component dimensions: general (abstract)--specialized (concrete); direct--calculated; non-evaluative (pure)--evaluative; and simple--modified. Sapir derived the notion of totality from two kinds of psychological experience: the feeling of rest or inability to proceed after a count, formal or informal, has been made of a set or series or aggregation of objects; and the feeling of inability or unwillingness to break up an object into smaller objects.

Sapir's paper, "Grading: A Study in Semantics" (1915), also stands as a landmark in semantic theory. Hymes (1964a:13-14) points to Sapir's "Sound Patterns in Language" (1925) as "crucial and classic" in the development of cognitive anthropology's appreciation of ethnography and validity in ethnographic description. In "Culture, Genuine and Spurious" (1924), Sapir promoted among other things his notion of culture as a world outlook.

Alfred E. Kroeber, perhaps the most famous student of Boas, anticipated the modern concern with semantic analysis of kinship terminology in his article, "Classificatory Systems of Relationships," published in 1909. Working with the kinship terminologies of twelve North American tribes, Kroeber distinguished certain components which would serve to define componentially terms in some or all of the twelve terminological systems. Kroeber employed the dimensions of generation, marriage, degree of collaterality, sex of relative, sex of speaker, relative age in generation, and vital condition of connecting relatives. The reason Kroeber inferred the impossibility of a structural analysis of kinship terms, according to Levi-Strauss (in Hymes, 1964c:43), was because linguistics at that time was still confined to phonetic, psychological, and historical analysis.

Kroeber's 1909 paper has given rise to some disagreement by those who attempt to trace the various currents of work and thought which form the background for contemporary cognitive anthropology. The 1909 paper is inevitably mentioned in terms of its priority as a componential analysis of kinship terminology. The "etic/emic" nature of the dimensions utilized by Kroeber is the point which has stimulated most disagreement in evaluating "Classificatory Systems of Relationships" in terms of recent trends in cognitive studies. Sturtevant (1964:102-103) refers to this paper as "The basic paper on the etics of kinship." Harris (1968:577), on the other hand, reduces Kroeber's dimensions to Berreman's (1966) "anemic" status; i.e., Harris claims that Kroeber admitted units which were simultaneously emic and etic.

Harris (1968:577) notes that the whole point of Kroeber's article was to replace Morgan's sociological treatment of kinship with a linguistic treatment. In the original paper Kroeber was adamant in his position that terms of relationship reflect psychology, not sociology. Many years later, Kroeber (1952:172) admitted that it would have been more correct if he had stated "that as part of language, kin term systems reflect unconscious logical and conceptual patterning as well as social institutions."

Robert Lowie reflected the Boasian concern with emics. In "Religion in Human Life" (1963:534), he writes:

> The field worker's business is always and everywhere to understand the true inwardness of the beliefs and practices of the people he studies. He is not content to record that infants are suffocated, aged parents abandoned, or enemies eaten. Unless he can also recover the accompanying sentiments he has failed in his task.

The trend in American anthropology, notably between 1930 and 1950 and as associated with the work of Benedict, Opler, and Kluckhohn, has been described by George and Louise Spindler (1963:517-518) as representing

the first steps toward a psychocultural approach. The conceptions arrived at by these pioneers share certain features in common with each other and with recent cognitive anthropology. Benedict's configurationalist notion of "patterns" and Opler's more refined conception of "themes" share a common concern with what might be called "world view" (Barnouw, 1963:56, 104). Benedict sees cultures in terms of dominant patterns or systems of largely unconscious "attitudes" which permeate all society and provide the members of a culture with an integrated means of looking at the world around him. Opler shares this notion in his position that themes, as they interact with and balance one another, structure the nature of reality for the bearers of a culture.

Harris (1968:574) notes that Colby (1966:28) rejects the idea that Benedict and Opler are relevant in a discussion of the development of ethnographic semantics, an ethnographic approach which I am considering under the general term cognitive anthropology. I feel that Benedict and Opler should be considered relevant here because their early conceptions shared with contemporary cognitive anthropology an interest in patterning of cultural behavior and the attempt to seek this integration at levels which are not ordinarily verbalized by the members of a culture. They are also very much emically oriented. They of course did not share the linguistic approaches nor the stress on discovery procedures, appropriate anticipation, and semantic arrangement portrayal in logical space which characterizes the modern cognitive anthropologist.

Clyde Kluckhohn states his stress on emic orientation in his famous Mirror for Man (1949:300) when he states: "The first responsibility of the anthropologist is to set down events as seen by the people he is studying." Kluckhohn perhaps stands closer than Benedict and Opler to the

recent current in anthropology being discussed because of his contact and assimilation of the work of the Prague School of linguistic theory through the work of Jakobson. Both Kluckhohn and Levi-Strauss came to define culture in terms of structural contrasts (Hymes, 1964b:15). Wescott (1966:26) notes that the quest for meaningful elements of culture analogous to the phoneme was first enunciated by Kluckhohn in Mirror for Man. Kluckhohn also stands as important in the line of thought under discussion because of his utilization of the concept of covert culture, or the recognition of basic cultural phenomena which is hidden, rarely verbalized, and implicit.

The pervading influence of Edward Sapir again appears as decisive in the view of the language and culture relationship expressed by Benjamin Lee Whorf, Sapir's student. As noted previously, Sapir argued that a language as a cultural system more or less faithfully reflects the structuring of reality which is peculiar to the group that speaks it (Hoijer, 1962, in Tax, 1962:264). Whorf's "The Relation of Habitual Behavior and Thought to Language" (1941) represented the first important documentation of Sapir's thesis. David French (1963:392) has noted that in view of the numerous people who have approximated and formulated the hypothesis that thought is influenced or determined by language a more valid name for the "Sapir-Whorf Hypothesis" might be the "Humboldt-Boas-Cassirer-Sapir-Whorf-Lee Hypothesis."

In "The Relation of Habitual Thought and Behavior to Language," Whorf compared the language patterns of Hopi and Indian languages of Arizona with those of modern European languages in order to seek answers to the following questions.

> (1) Are our own conepts of 'time,' 'space,' and 'matter' given in substantially the same form by experience to all men, or are they in part conditioned by the structure of particular languages? (2) Are

there traceable affinities between (a) cultural and behavioral norms and (b) large-scale linguistics patterns (Spier, Hallowell, and Newman, 1941:78)?

Whorf (in Spier, Hallowell, and Newman, 1941:92-93) concludes:

> Concepts of 'time' and 'matter' are not given in substantially the same form by experience to all men but depend on the nature of the language or languages through the use of which they have been developed. They do not depend so much upon any one system (e.g., tense, or nouns) within the grammar as upon the ways of analyzing and reporting experience which have become fixed in the language as integrated 'fashions of speaking' and which cut across the typical grammatical classifications, so that such a 'fashion' may include lexical, morphological, syntactic, and otherwise systematically diverse means coordinated in a certain frame of consistency...As for our second question...There are connections but not correlations or diagnostic correspondences between cultural norms and linguistic patterns...There is a relation between a language and the rest of the culture of the society which uses it.

With regard to Whorf, Hymes (1964b:26) writes:

> A predecessor with special interest for transcultural studies in cognition is Whorf. A theory of the nature of any sector of linguistic structure has import for cognition, as a theory of something users of language acquire and use, but semantic structure is especially salient; and it was in the framework of semantic description that Whorf broached problems now being developed in the framework of transformations...He explored the cognitive implications of Hopi structure through a test essentially like that for generative grammars, trying out Hopi sentences implied by his understanding of the grammar, and investigating the reasons when sentences proved unacceptable to his informant.

Robbins Burling in his influential article, "Cognition and Componential Analysis: God's Truth or Hocus-Pocus" (1964:26), has stated that Whorf's ideas have fallen into disrepute. Bright and Bright (1965:258) deny this and point to the numerous recent papers which are all sympathetic to the Whorfian hypothesis (e.g., Hymes, 1961; Kluckhohn, 1961; Fishman, 1960; Mathiot, 1962). Carrol (1964:12) offers a statement of what may be called the neo-Whorfian position.

> Insofar as languages differ in the ways they encode objective experience, language users tend to sort out and distinguish experience differently according to the categories provided by their respective languages. These cognitions will tend to have certain effects on behavior.

Noam Chomsky's theory of generative grammars has also had its influence on the growth of cognitive anthropology and has been influenced in turn by many of the same men and ideas that have left their mark on contemporary anthropological studies of cognition. Sturtevant (1964:10) notes the great similarities between Goodenough's criterion of cultural analysis and Chomsky's criterion for grammars. This similarity can be seen in King's (1969:10-11) statement concerning the goal of generative theory.

> Our goal in linguistics is the construction of a grammar: the correct account of the linguistic competence of the native speaker-hearer of a language.

"Competence" refers to " the intrinsic, largely unconscious knowledge underlying our ability to speak and to understand what is spoken" (King, 1969:7). Generative grammars aim at delivering a formal account of competence which will serve in formulating and testing theories related to the actual linguistic performance of the native speaker-hearer of a language. The similarity of this program with the general cognitive approach to discovering cognitive systems or formal accounts of cognitive systems which will enable the analyst to anticipate appropriate responses is obvious.

A further similarity between the general positions represented by Chomsky on the one hand and Goodenough on the other, related to the nature of the "rules" they seek, is that both men feel, like Levi-Strauss and others, that the "rules" and/or "models" they generate are in some way related to real physical actualities of the human organism. Chomsky has stated that linguistic competence is instinctive in man (1971). King (1969:14) states that grammars represent an "immensely abstract and complex knowledge contained in the human organism."

The theory of generative grammars has doubtless served to support and augment the theoretical programs of cognitive anthropology. It cannot in anyway, however, be considered as a predecessor to the field. Sturtevant (1964:10) states that Goodenough's statement of the criterion of cultural analysis, though heavily indebted to linguistics, was independently parallel to Chomsky's criterion for grammars.

In bringing to a close the discussion of American linguistic and ethnological tendencies which have momentarily coallesced as cognitive anthropology, note must be taken of the role of Leonard Bloomfield in recent developments. It should be remembered that Bloomfield in his first book, An Introduction to the Study of Language (1914), showed concern with cognitive categories. Between the publication of Introduction and the publication of his Language (1933), he had become converted to a narrow version of behaviorism (Hymes, 1964c:11). "An aggressively 'scientific' approach, a rejection of 'mentalism,' and a focus on descriptive method per se pervaded American linguistics for almost a generation, with Bloomfield as its patron saint" (Hymes, 1964c:11). Bloomfield felt that "signals can be analyzed, but not the things signalled about," and that "this reinforces the principle that linguistic study must always start from the phonetic form and not from meaning" (1933:162). This position is stood on its head by the contemporary advocates of cognitive anthropology. Bloomfield's positive influence in the present development of cognitive anthropology is probably best stated in terms of the rigorous scienticity and descriptivism with which he injected the field of linguistics in America. This stance, coupled with the stimulating conceptions concerning the language and culture question produced here and abroad, has made cognitive anthropology an indeed compelling sub-field of anthropology.

Thus far, I have attempted to trace what I consider to be the major lines of influence which have formed a background for the emergence of cognitive anthropology. I have discussed these influences in terms of the development of French structural sociology and anthropology, the development of structural linguistics and the Prague School, and developments in America stemming in the main from the theoretical positions of Boas and Sapir with regard to the language and culture problem. Extending the search for the most important historical bases of cognitive anthropology, I will note men, ideas, and events in British anthropology and in the fields of psychology and philosophy which can be considered important, and sometimes crucial, to the growth of cognitive anthropology.

According to Hymes (1964c:5), the British point of view concerning the relation of language and culture is to view language as a mode of action, not a countersign of thought. This view was presented, for example, by Malinowski in his "Meaning in Primitive Languages" (1923). The important point which the British have championed, and which is also a basic fact of American cognitive anthropology, is that native language must be utilized in fieldwork.

The standard of emic description is upheld by Malinowski (1922:25) in the following quotation from his Argonauts of the Western Pacific in which he stresses the "final goal of which the ethnographer should never lose sight."

> This goal is, briefly, to grasp the native's point of view, his relation to life, to realize his vision of his world. To study the institutions, customs, and codes or to study the behavior and mentality without the subjective desire of feeling by what these people live, of realizing the substance of their happiness--is, in my opinion, to miss the greatest reward which we can hope to obtain from the study of man.

E. B. Tyler can be understood in the same sense when he warned that

the ethnologist "must avoid the error which the proverb calls measuring other people's corn by one's own bushel" (1881:410, quoted in Sturtevant, 1964:100). In terms of early British anthropological activity relating to the later development of cognitive anthropology, the anthropological expedition to Torres Stratis and New Guinea just before the turn of the century must be noted. "On the initiative of Haddon and under his direction, Rivers, Seligman, Myers, and McDougall studied vision, hearing, smell, taste, 'cutaneous sensations,' and 'muscular sense,' as well as various phenomena not related to sensation and perception" (French, 1933: 390).

The emic position with regard to the study of kinship was early ennunciated by the British anthropologist W. H. R. Rivers (1912:119) in the following quotation from his "The Genealogical Method" (1912):

> In acquiring a knowledge of the pedigrees, the inquirer learns to use the concrete method of dealing with social matters which is used by the natives themselves and is able to study the formation and nature of their social classification and to exclude entirely influence in civilized categories.

With respect to the effects of certain trends within the field of psychology and their relationship to cognitive anthropology, David French (1963:402-415) has noted the impact that Gestalt theories of perception have had on cognitive studies in anthropology. It is interesting to note that in psychology the Gestalt school was in opposition to the Behaviorist school with regard to the nature of perception and cognition. The analog in anthropology is the opposition of the behaviorist oriented Bloomfieldian "school" (basing their studies on verbal behavior) and the Gestalt oriented cognitive anthropologist who seeks the organizational and semantical principles which lie behind verbal behavior.

In the following characterization of Gestalt notions of perception

(drawn from Chaplin and Krawiec, 1968:141-147), certain similarities with cognitive anthropology's position on the nature of cognition and perception can be seen. Gestalt psychology is "form" psychology. According to its proponents, our perceptual experiences arise as "gestalten," "forms," or "molar configurations" which are not mere aggregations of sensation, but organized and meaningful wholes. The determinants of organization and meaning are related in turn to certain fundamental laws of Gestalten, the most important of which is "isomorphism." The principle of isomorphism states that there is no one-to-one relationship between stimuli and percepts, but that the form of experience corresponds to the form or configuration of the stimulus patterns.

Gestalt psychology looks upon the world as psychophysical. Gestalt psychologists are in the habit of referring to the "psychological field" to represent the perceiver's view of reality. In contrast the world of the physicist is referred to as the "physical situation."

The general Gestaltist law of isomorphism has certain sub-principles: these are the well-known Gestalt principles of perceptual organization, sometimes referred to as laws of primitive organization. The most fundamental of these principles of primitive organization is that of figure-ground. Figure-ground is the familiar principle which states that every perception is organized into a figure which stands out from a background. The figure not only stands out but also has well-defined contours, depth, and solidity. It must be emphasized, however, that these figural characteristics are not properties of the physical stimulus-object, but are characteristic of the psychological field. It should be noted that traditional accounts of perception emphasized the role of experience as an explanatory concept. The Gestalt psychologists, on the other hand, emphasize figure-

ground as a spontaneous and native organization which does not depend upon learning but is an inevitable consequence of man's perceptual apparatus.

Among the basic laws of the classic Gestalt school is the law of transposition. This principle states that because Gestalten are isomorphic to stimulus patterns they may undergo extensive changes without losing identity. Thus, a tune transposed to another key remains the same tune even though the elements (notes) making up the melody are all different. Perception, then, is flexible; and, just as a map can be expanded, shrunk, or presented in different types of geographic projections and remain recognizable as the same map, so the elements of our perceptions may be changed--often markedly--and still yield the same perception. Naturally, there is a limit beyond which change in elements may not go without producing a complete transformation in the precept. The structuring of the elements may be changed without destroying the Gestalt only so long as the relative spatial and temporal relationships are preserved.

Though the Gestalt psychologists never emphasized adaptation to the environment as a systematic theme, their psychology is nonetheless functionalistic in spirit, for, to the extent that the laws of Gestalten are valid, they make for stability and constancy in an ever-changing world. It will be remembered that this was Goodenough's position concerning cognitive structures in his Cooperation in Change cited in Chapter II.

The forgoing outline of the basic tenets of Gestalt psychology's position concerning perception opens the door for a brief exploration of the place of certain European and American philosophical traditions in the history of cognitive anthropology. An examination of the major ideas

and assumptions generated by German Idealism, American Pragmatism, Analytic Philosophy, and Logical Positivism is essential in understanding the various developments leading to and justifying the present field of cognitive anthropology and anticipating its future direction. Certain systems of ideas (a philosophy) relating to the nature of reality, truth, and man's means of access to these realms lie at the foundation of the more readily visible theories, methods, and techniques which mark the progress of individual men in individual academic disciplines at a specific time in a specific place. Because of the almost diagnostic historical shortsightedness of cognitive anthropology, it is difficult to specifically relate certain philosophical positions to the cognitivist literature from overt and self-conscious indications in that body of literature. At least, it can be suggested that the various philosophical periods and positions to be discussed "informed" parallel trends in other areas of intellectual endeavor which can be more readily understood in terms of their historical relationships.

The bipolar opposition of Behaviorist Psychology and Gestalt Psychology with respect to attitudes concerning the nature of perception has been noted, as well as the analogy applicable in this case between descriptivist approaches to language and culture and the cognitive and structural approach of cognitive anthropology to the same problem. This opposition is sometimes phrased in psychology and in philosophy in terms of Nativism vs. Empiricism. Gestalt psychology holds a Nativist position with respect to perception; while Behaviorism can be characterized as representing the Empiricist tradition. Nativism originated in philosophy as a parallel concept to Rationalism, and like Rationalism, it is frequently employed as a bipolar opposite to Empiricism.

The Empiricist, notably represented by the English philosophers Locke, Berkeley, and Hume, maintained the view that all our knowledge ultimately derives from experience which reaches us through our senses. The Rationalists, such as Descartes, Spinoza, and Leibniz, held the position that the mind is fitted initially with certain faculties of reason, and that reason, operating in accordance with the laws of logic, can attain knowledge of truth which owes nothing to sense experience. Knowledge obtained in this way is called a priori knowledge.

The debate between Rationalist and Empiricist philosophers held the center arena in European philosophy in the seventeenth and eighteenth century. This is significant because this debate shadowed the emergence of the first attempts to formulate a science of society and, in various way, entered the streams of thought concerning man in his social and cultural aspects. The presence of this basic dichotomy, I feel, is today very much evident in the tenor of anthropological theory. Further, an understanding of the broadest perspectives relating to the position of cognitive anthropology in contemporary anthropological theory must include a consideration of these issues.

The Idealism of Immanuel Kant which constituted a one sided synthesis of British Empiricism and Continental Rationalism, with the weight on the Rationalist position, is most often considered relevant in a discussion of the philosophical milieu of present trends in cognitive anthropological theory (Vachek, 1966:159; Harris, 1968:600). Indeed, for Harris (1968:568ff), the entire field of cognitive anthropology is discussed in terms of Idealism vs. Materialsim, a slightly different phrasing of the Rationalist vs. Empiricist problem.

Kant's Critique of Pure Reason, published in 1781, initiated a new

era of critical philosophy, namely German Idealism (Sahakian, 1968:169). Kant felt that the concepts of time and space were modes of mental experience prior to sensations, but that these concepts had to act with and combine with sensation in order for thought to occur. However, even though Kant injected the necessity of sensibility into his scheme, he insisted that in the first stage in the attainment of knowledge, a stage which he called "The Transcendental Aesthetic," the concepts of time and space are prior to and independent of the senses: they are ideal, internal creations of the mind. It is also significant that for Kant the mind is by its nature logical. The belief in the priority of mind and the belief that the mind operates logically are notions that are fundamental in cognitive anthropology today. These notions are sometimes blatantly stated and at other times buried in the assumptions which guide the particular methodology.

For Kant, ultimate reality, though it exists, is unknowable. The mind, however, is driven to reproduce what it believes to be the real world. The real world is an ideal reconstruction in the mind of man, a replica of what he believes the real world is like, according to Kant. "The world is my representation," wrote Kant (quoted in Sahakina, 1968:173). Kant's notion of "representation" finds kinship with the "collective representations" of Durkheim and Mauss, the structures of Levi-Strauss, and the cognitive structures of cognitive anthropology in general. It is also interesting that Kant's notion of the real world as an "ideal reconstruction" in the mind is very similar to the often used concept of models in the literature of contemporary cognitive anthropology.

Harris (1968:268) states that Kant's influence in American anthropology can be first identified in the neo-Kantian orientation of Franz Boas.

> Boas' involvement with the neo-Kantian movement dates at least from his last four semesters at Kiel when he studied philosophy under Benno Erdmann, a leading contemporary authority on Kant. Another prominent neo-Kantian was Rudolf Lehman, with whom Boas corresponded on the eve of his arctic expedition.

The influence of another famous German Idealist, Georg Wilhelm Fredrich Hegel, is most evident in cognitive anthropology and structural linguistics in relation to such conceptions as "contrast set," "distinct features," "binary opposition," "dimensions of meaning," and "phoneme." The facet of Hegel's general philosophy which is most relevant here is his dynamic logic, the Hegelian dialectic. Sahakian (1968:191) writes:

> The Hegelian dialectic...find truth through a series of triads: thesis, antithesis, and synthesis. Every thesis, if it is to have any meaning, will find it in its antithesis: every fact will be understood when related to its opposites, to those things which the thesis is not. Only by pointing out the many relationships of any one object to another object can we establish the truth about that object...Hegel was agreeing with Spinoza's dictum that 'all determination is negation.'

Pragmatism, essentially an American philosophy, developed in the latter half of the nineteenth century. Pragmatic notions concerning the nature of thought are seen in the purpose and action oriented assumptions of cognitive anthropology concerning the function of cognitive structures. Equating culture with cognition, Goodenough (1963:258-265), for example, speaks of "grammatical principles of action" and argues that experience is organized in man in order to facilitate the accomplishment of "recurring purpose." Further, for Pragmatists, especially Charles Peirce, and for cognitive anthropologists, the mental features which guide action and upon which action is built are part of man's constitutional make-up.

The Pragmatic criterion of truth also holds interesting parallels with the criterion of descriptive validity advocated by cognitive anthropology. For both, truth is based upon the workability of any theory or

proposition. What does not work cannot be true. If a cognitivist theory can anticipate appropriate behavior, it is "true" or valid: if it cannot, it is false, or invalid.

Cognitive anthropology also exhibits the influence of a brand of philosophy variously called Logical Positivism, Neopositivism, Logical Empiricism, and Scientific Empiricism which appeared in Vienna in the early twentieth century. The three features of Logical Positivism which are most reflected by contemporary cognitive anthropology are a stress on rigorous scientific procedure, the pursuit of meaning, and the emphasis on formal analysis in the pursuit of meaning.

The influence of another kind of philosophy, British Analytic philosophy, is witnessed chiefly in the rationale for the eliciting heuristics in anthropological studies of cognition. These philosophers felt that most difficulties in philosophy are caused by the attempt to answer questions without first discovering what question it is which one wishes to answer. The work of R. G. Collingwood is particularly relevant here. Tyler (1969:141) states:

> It is noteworthy that as early as World War I, the British philosopher R. G. Collingwood was formulating a new 'logic of question and answer,' insisting that the basic unit of thought was not the proposition (as was then held), but 'propositions... together with the questions they were meant to answer.' Collingwood wrote, 'A logic in which the answers are attended to and the questions neglected is a false logic.' Note that he is referring to the questions, usually unstated, that the proposition is intended to answer. The question is implicit, as far as the speaker is concerned: he assumes it is known to the hearer. It was Collingwood's thesis that the question may actually be unknown or mistaken by the hearer and, if so, he is incapable of understanding and responding appropriately... For ethnography this principle has a special relevance. Whereas the ordinary speaker normally assumes knowledge of his implicit question on the part of his hearer, the ethnographic approach used here (cognitive anthropology) assumes lack of knowledge of the question on the part of the anthropologist, who must proceed systematically to learn them from informants.

In conclusion, Sturtevant (1964:114-115) notes the specific influences which lead to the emergence of the field of cognitive anthropology in America with the simultaneous publication in Language (1956) of articles by Floyd G. Lounsbury and Ward H. Goodenough:

> In each case the breakthrough was the result of training by Murdock in the etics of kinship, plus thorough knowledge of descriptive linguistics (where componential analysis was then used in phonology), plus an acquaintance with the philosopher Charles W. Morris' work on the theory of signs. Both shared also some exposure to mathematics and learning theory.

The effects of Goodenough and Lounsbury's exposure to the field of learning psychology has been little recognized and explored in the majority of brief survey of the history of the field of cognitive anthropology. The "purposive behavior" conceptions of Edward C. Tolman, expounded in a series of works between 1932 and 1959; the book A Study of Thinking written by J. S. Bruner, J. J. Goodnow, and G. A. Austin and published in 1956; and the continuing study of concept formation in children by Jean Piaget of the Universities of Paris and Geneva can be singled out as three major influences on Goodenough and Lounsbury's formulation of the theoretical orientation that I am calling cognitive anthropology.

The conceptions of "cognitive map" and "field cognition modes" as mental structures are the inventions of Tolman. Tolman's work was mainly involved with the study of learning in animals. The conception of "sign Gestalts" as cognitive processes which are learned relationships between environmental cues and the animal's expectations was borrowed from Gestalt psychology. Tolman felt that sign Gestalts form a pattern which he referred to as a cognitive map. In studying how a rat learns to run a maze to the reward point, Tolman demonstrated that the animals learn a cognitive map of the maze and not merely a set of motor habits.

He conceived of these cognitive maps as complex mental structures which were employed in learning and, in general, acting purposefully. It is also significant that Tolman felt that cognitive maps were actual cortical mechanisms.

In Tolman, many old notions are encountered again, though with a more scientistic dressing. Further, many ideas which are a part of the cognitivists' scheme appear in Tolman's formulations--cognitive maps, real mental structures, emphasis on purposive behavior as part of explanation of human thinking and learning, stress on importance of expectations and anticipation of individual in learning process, and a general Gestalt view of perception and thought.

The work of Jean Piaget can also be considered as part of the general current of thought from which cognitive anthropology was fashioned. Piaget's position is that the study of conceptual thinking in children, especially the origin and development of basic concept and systems, should not only show how concepts are formed but should also show what work a concept does in shaping thought (Thomson, 1959:88). Piaget's work is, in part then, a continuation of the old evolutionary notion that the study of thought processes in children and "savages" would illuminate the nature of thought at more advanced developmental stages.

Piaget's work is very much modeled after the Rationalist tendencies of French Structuralism. The idea that the mind is logically structured is doubtless reflected in Piaget's work in constructing a new type of logic which he calls "psycho-logic," based on mathematical logic, and which he believes is best suited to describe and reflect the nature of thought. Piaget further maintains that thought processes are best analyzed in terms of groups or systems which are the interiorization of actions. Piaget's

conception of "group" is very similar to the notion encountered previously in this chapter of "mental structure." Piaget also exhibits a trend which has been noted previously when he states that higher psychological functions grow out of biological mechanisms (Thomson, 1959:90).

Much of Goodenough and Lounsbury's claim for originality in certain of their formulations first published in 1956 can be challenged by a review of the book by Bruner, Goodnow, and Austin, A Study of Thinking, published the same year as Goodenough and Lounsbury's ground breaking articles in the journal Language. Bruner and his associates were interested in how adult subjects who have already formed and developed a complex repertory of concept attain new class concepts of various kinds. Bruner saw the chief problem for his work in designing experiments which would serve to externalize the thought processes of his subjects. Similarly, Goodenough (1965, in Tyler, 1969:257) notes that he developed the method of componential analysis in order "...to make objective something about Trukese kinship..." With respect to componential analysis, Bruner and his associates utilized paradigmatic representations and componential analysis in their 1956 study of concept attainment in adult subjects. Further, Bruner similarly to the general methodological program of cognitive anthropology, focused on class concepts and classification on the basis of discriminable attributes. Thomson (1959:64) notes:

> There are many varieties of concepts, but the class concept is the type which has been selected for scrutiny by experimental psychologists. It may be that this, almost exclusive, attention paid to classification as an exemplification of conceptualizing derives from Aristotle's logic, which is based upon relations of class inclusion and exclusion, and from the fact that few people are aware of the revolutionary changes in logic in the present century.

The similarity between Bruner and Goodenough is also reflected by comparing Goodenough's idea of the culture concept and the nature of

ethnography with Bruner's model of the problem situation in concept attainment and learning. Goodenough (in Hymes, 1964c:36) states that a society's culture consists of whatever it is one has to know or believe in order to operate in a manner acceptable to its members, and do so in any role that they accept for any one of themselves. He then states that a valid ethnography would be on the order of a kind of guide to appropriate behavior in a particular culture. Thomson (1959:69) writes:

> Bruner noted that if a subject has to acquire a new concept by learning its defining attributes he is really in a problem situation requiring a number of decisions. He gave the example of a stranger being shown around a town by one of its inhabitants. In the course of his introductions several citizens are pointed out as being influential. The stranger is set the task of finding out what makes a man influential in this particular community. What are the attributes which define this class--wealth, occupation, education level, age, religion? The stranger has a problem. What is his task? Which attributes are relevant? How many or how few are needed for a reliable definition of "influential'?

In this chapter I have attempted to demonstrate the antiquity of the conceptions and objectives which are now considered basic to cognitive anthropology. The "newness" of cognitive anthropology resides in its synthesis of old ideas and methods with new manners of inquiry, objectives, and means of demonstration gleaned from psychology, semantics, mathematics, biology, philosophy, and sociology. The basic novelties which permit the field's existence as such include the explicit conceptualization of culture as a whole in terms of cognition (Sturtevant, 1964:100), the position that ethnography should be thought of as concerned with the discovery of the conceptual models with which a society operates, and the proposition that validity of an ethnographic description should be judged in terms of anticipation of appropriate cultural behavior.

IV

PERCEPTION, COGNITION, AND THE CONCEPT OF MIND

In this chapter I will deal with the most recent studies in psychology which deal with perception and cognition. This direction is necessary in order to illustrate that the assumptions concerning perception and cognition which are part of the cognitivist strategy are not necessarily supported by the special fields of perceptual and cognitive psychology. That this fact is realized, overtly or covertly, by cognitive anthropologists is witnessed by the relative lack of notice given to the men and ideas generated by the psychological specialist in the fields of perception and cognition. A conclusion that can be drawn is that the cognitivists notions of perception, cognition, and mind are drawn more from a general faith based on an internalized Rationalistic and Idealistic conception of such facets of human activity, than from contemporary disciplines that specialize in the rigorous study of these human processes. The survey that follows will, I think, demonstrate why researchers in the field of cognitive anthropology might understandably prefer to ignore the conclusions reached by modern perceptual psychology. The studies of perceptual psychology challenge a basic assumption of cognitive anthropology (the belief, advocated or implied, in formal cognitive structures of the mind) as well as the S-R (stimulus-response) model utilized as the basis of discovery and validating procedures.

Similar to the cognitivists' tendency not to cite recent psychological studies of perception and cognition is the tendency not to define precisely

what they intend by the concepts of "perception" and "cognition." Once again this is apparently based on the notion that the meaning of these conceptions is self-evident. A brief consideration of the long history of these ideas in psychology and philosophy, as well as anthropology, would certainly demonstrate this position to be in error.

As a necessary preface to the following discussion, I will deal briefly with definitional statements as to the nature of perception. Stagner and Karwoski (1952:207) define perception in the following manner:

> Perception is the process of obtaining knowledge of external objects and events, by means of the senses. William James put it well when he wrote, 'Perception is of definite and probable things.' In other words, man takes his sensations (about which he is sure) and reaches conclusions about real objects (about which, actually, he is less sure if questioned).

McConnell (1961:185) writes:

> ...a percept (is) an experience composed of a core sensations plus certain images which have become associated with this core through past experience. The sensations supply the 'raw data' of experience, while the images supply the context which gives the sensations meaning. Perception is the process whereby we learn the meaning of stimuli in the external world, the process by which images are built up and attached to sensations.

Sartain (1962:233) defines perception:

> Perception may be defined as the process by which sensory imput is interpreted...Perception is a process that mediates between stimulation and response. As a mediating process perception cannot be directly observed, but must be inferred from observable behavior and a knowledge of the stimulus situation.

Carmichael (1957:90), in his Basic Psychology,..writes:

> Perception (is) an awareness of external qualities, relations, or objects dependent at least in part upon present sensory stimulation... It is always based on the way the organism 'works over' the data of the senses. The interests, needs, and socially determined expectations of the individual influence perception.

The preceeding definitions are of course heavily tinted by their authors particular theoretical bias. However, taken together, they give

a general understanding of perception which is sufficient for the purposes of this section.

The fundamental problem for classical perceptual psychology was to account for the orderly arrangement of objects in space and time in the world of the perceiver (Chaplin and Krawiec, 1968:123). The Scottish philosopher, Thomas Reid (1710-1796), was the first to formulate the distinction between sensation and perception.

> He referred sensations to the activities of the sense organs as these are experienced in consciousness. Perception he held to be dependent on sensation but different from the former in that the perceiver is aware of objects or events in his environment and not merely sense impressions (Chaplin and Krawiec, 1968:125).

Reid evaded the problem of attempting to formulate a rigorous explanation for perceptual meaning. He simply attributed the quality of perception to an instinctive tendency in the human constitution. He held that the perception of time and space was intuitive. In a crude fashion Reid's position is similar to the position of cognitive anthropology. However, as Chaplin and Krawiec (1968:125) state, "Reid's own solution was simple and forthright but not very satisfying to the modern reader." If Reid is considered one of the first to deal with the problem of perception, it can be seen that one of the first explanations of perception in Western philosophy was nativistic and couched in terms of the Rationalist faith.

The counter position in classical perceptual theory, the Empiricist position, is early exemplified by the work of Berkeley who attempted to deal with the problem of perceptual meaning in terms of associationism. He held that any perception is meaningful only in the light of past perceptions, whose meaning in turn is carried into the present in the form of ideas (Chaplin and Krawiec, 1968:126). The empiricist tradition rejected

appeal to the mind as a means to explain the mechanisms of perception.

The emphasis on the study of the perception of space and time variables, guided by the nativism or empiricism paradigm, lasted until the end of World War II. At that time perceptual psychologists began to view perception as a meeting ground for motivational, attitudinal, and personality variables. This break in research direction was something of a "scientific revolution" in Kuhn's terms. The postwar psychologists argued and powerfully demonstrated the incredible degree to which the meaning of experience is contributed by the observer. Modern perceptual psychology has gained from its long history the knowledge that the traditional S-R model is inadequate to the task of formulating a clear explanation of perception. Contemporary perceptual psychology has placed the "O" (organism) in the S-R model and, with this S-O-R orientation, has been able to reach some interesting conclusions concerning perception. The modern S-O-R formula is known generally as directive state theory because it places emphasis on such "O" factors as sets, values, needs, attitudes, etc. In other words, contemporary perceptual psychology has turned from a focus on the structural determinants of perception, such as the nature of the physical stimuli, to behavioral determinants of perception, such as past experience, unconscious assumptions about "objects," needs, attitudes, etc.

It is somewhat difficult to arrange a discussion of the various experimental categories dealing with behavioral determinants of perception. Different authors arrange presentation of the significant experimental findings in different ways. Stagner and Karwoski arrange a multitude of studies under the general heading of the "effects of perceptual assumptions on perception." Chaplin and Krawiec use such headings as "sets"

(or temporary states of motivation) and "attitudes and values" as perceptual determinants. Both of these organizational frames could be subsumed under Stagner and Karwoski's "effects of perceptual assumptions." Sartain (1962:244) utilizes an even more inclusive heading: "Personal factors in perception."

In this chapter I will organize the presentation of significant experimental findings in recent perceptual psychology in the following order: social influences on perception; the effects of sets on perception; the effects of needs on perception; the effects of emotion on perception; the effects of values and attitudes on perception; and the effects of learning on perception. These headings overlap, of course, but I feel that they emphasize certain significant factors reflected in the subsumed experimental statements. Also, the various key concepts in each heading, e.g., needs, emotions, etc., are intended in a very general sense.

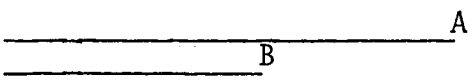
James McConnell (1961:303) notes that social pressures cannot only change the effect associated with a percept, but if the pressure is great enough, the percept itself can be changed. Stagner and Karwoski (1952: 240) write:

> Suggestions may come from the actions or words of others in an indirect manner. One's past experience leads him to assume that, when others attend to an object, it may have importance for him. We also accept the interpretation as to size, value, etc. held by others.

Two experimental studies are of particular interest here--Sherif's 1935 study and the experiment reported by McConnell (1961). Sherif (in Stagner and Karwoski, 1952:240) devised an experiment to measure the social effects on perception. He first had his subjects tested individually on the autokinetic phenomena (in a dark room one watches a point of light, actually fixed, and it seems to move). Each subject developed

his own estimate of the distance in inches which the light moved. Next, three subjects worked together on this problem. Sherif found that the estimations moved together, i.e., persons who had made high estimates lowered them, and persons with low estimates raised them. "Basically, this means that we will modify our own perceptions of size, movement, and value to conform with the judgments expressed by people around us" (Stagner and Karwoski, 1952:240).

McConnell (1961:303) reports an experiment in which a large number of subjects were shown two lines like the following, one line after the other,



and then were asked to judge which was the longer. However, before the subjects reported their judgments, four or more "stooges" reported first. When the stooges reported that line "B" was longer than line "A", a large number of the subjects yielded and reported that they too thought "B" was longer. Some subjects, when questioned later, reported that they just went along with the group, while others insisted that the perceived "B" as longer.

The effect of "sets" on perception is an important area of study. McConnell (1961:273) defines "set" as a readiness to respond in a certain way. Chaplin and Krawiec (1968:176) write that sets are temporary states of motivation which alert the subject to perceive or respond in accordance with the set. With regard to sets in Hebb's theory, McConnell (1961:273) writes:

> Sets play a powerful role in controlling our experience. One of the basic laws of psychology, derivable from Hebb's theory, is this: We see what we expect to see. Since perception generally precedes and influences action, it follows that a corollary to this law is this: We react as we are set to react.

Hastorf (1950, in Stagner and Karwoski, 1952:239-240) gave his subjects the task of adjusting a circle of light so that it looked to be the same distance away as a target. When they were told, i.e., provided with the set, that the target was a ping-pong ball, they responded differently than when told that it was a billard ball. When simply shown a disk and asked to judge its distance, the same disk was seen farther away as a ping-pong ball than as a billard ball.

Cartwright (1949, in McConnell, 1961:272) made a study of war bond sales during World War II. At first war bond sales were very low. Cartwright writes that the population perceived the slogan "Buy War Bonds" simply as a request for money which carried no implication that any money would be ultimately returned. At this point the sale of bonds went poorly. When the populace was convinced, i.e., its set was altered, that their money would come back to them, the sale of bonds went up. Cartwright concluded that in each case the people reacted as their expectancies made them set to react.

Sipola (1935, in Chaplin and Krawiec, 1968:176) required his subjects to respond to a list of words presented tachistoscopically at 0.10 second. The stimulus words were: horse, baggage, chack, sael, wharl, monkey, pasrort, berth, dack, and pengion. One group was told beforehand that it would see words dealing with animals or birds: the other group was told that it would be responding to words dealing with travel or transportation. Since all the words except 1, 2, 6, and 8 are ambiguous, the hypothesis was that the responses of the subjects would correspond to the sets with which they were provided. For example, the first group might perceive "dack" as "duck," while the second group might perceive "dack" as "deck" or "dock". The results confirmed the hypothesis.

Subjects in the first group perceived six times as many animal-bird words as did the subjects in the second group, who, perceived five times as many travel-transportation words as the first.

Adams (1923, in Stagner and Karwoski, 1952:239) altered the odor on theree artificial flowers before submitting them to her subjects. An artificial pink rose was perfumed with violet scent, a red rose with lily scent, and violets with lily scent. Ten of the twelve subjects tested reported that the perfume was the characteristic odor of the flower, although when tested with eyes closed they all identified the scents correctly.

With respect to studies dealing with the effects of needs on perception, the underlying assumption seems to be that when certain needs or drive states are active the organism is most likely to give its attention and perceptual priority to potential satisfiers of the particular need, or drive. The major studies here deal with hunger's effect on perception.

Sanford (in Chaplin and Krawiec, 1968:176) demonstrated that hungry subjects completed word stems in such a way as to make more food-relevant words than did nonhungry subjects. For example, the word stem "me--" was more likely to be completed as "meat" or "meal" by hungry subjects than by nonhungry subjects.

McClelland and Atkinson (1948, in Stagner and Karwoski, 1952: 241-242) showed blurred pictures (ambiguous stimuli) to their subjects one to sixteen hours after eating. As the amount of presumed food need increased the perceptions reported showed a great increase in the number of food related objects reported.

In studying the effects of emotion on perception, Leuba and

and Lucas (1945, in Chaplin and Krawiec, 1968:177-178) hypnotized three subjects and by means of suggestion induced three different moods in each subject--"happy," "critical," and "anxious." While in each mood, the subjects were presented with six pictures which they were asked to describe following a brief observation period. In general the descriptions corresponded to the induced mood.

Values and attitudes have also proven to be crucial in perception. Bruner and Goodman (1947, in Stagner and Karwoski, 1952:242) asked children to adjust the size of a circle of light to match the size of coins. In another series they asked the children to adjust a circle of light to match the size of gray cardboard disks, the sizes being the same as the coins. All the children overestimated the size of the coins. In the same experiment two groups of ten-year old children, one group from "rich homes" and one group from "poor homes," were asked to estimate the size of coins using the same circle of light adjusting technique. The poor children overestimated the size of every coin to a greater degree than the rich children (Chaplin and Krawiec, 1968:178).

Postman, Bruner, and McGinnies (1948, in Stagner and Karwoski, 1952:242) used a standard test to identify people who had high religious values, high economic values, and so on. To these people words were presented in a tachistoscope at very short exposure. In the majority of cases words which related to the person's high value were perceived at much shorter times (faster perception) than when the words related to values on which the subjects made a low score.

> The experimenters claim that two processes are involved: 'perceptual selectivity' (more efficient use of cues which fit in with motives or values) and 'perceptual defense' (active resistence to certain perceptions which were contrary to the person's values) (Stagner and Karwoski, 1952:242).

Experiments which deal with the effects of learning on perception have produced some startling results. I include here studies which others have discussed under the headings of "motives," "conditioning," and "subception." However, in my opinion all of the following studies relate to learning, in the broadest sense, and its effect on perception.

Senden (1932, in Stagner and Karwoski, 1952:207) collected information in sixty-six instances of persons who had undergone cataract operations permitting vision at a delayed age. It was hoped that these reports would enable a determination of what a person can see when presented with a visual environment for the first time. One conclusion stands out clearly from these observations: the patient is aware of being visually stimulated, but he does not identify objects as such. Even though these people knew objects from tactual experience and had the use of language, they could not answer questions because the words simply did not relate to vision. Such a man could distinguish visually between a ball and a block, but he did not know which was which until allowed to handle them. Two strips of cardboard, 10 cm. and 20 cm. in length, were perceived as different, but the individual could not say which was shorter--although he knew the meaning of this word in terms of touch. The perceived object is a totality involving vision, touch, smell, etc., but there is no inherent connection between these cues: the relationships must be learned. Senden's data may be of some help in understanding the development of perception. Naming colors was apparently easiest. Motion, size, and distance were acquired fairly soon, but it took months to identify common shapes.

Riesen (1947, in Stagner and Karwoski, 1952:208) reared two chimpanzees in total darkness. He found that the animals did not learn to use visual cues in any manner, although it was clear from their behavior that

they did see "something" in their environment. They showed no eyeblink to an object approaching the eye and were incapable of forming a fear response to a visual stimulus (a distinctive visual pattern paired with electric shock).

Stagner and Karwoski (1952:208) write:

> The data collected by Senden and Riesen seem to prove conclusively that learning plays an important part in our ability to identify objects and their characteristics.

Chaplin and Krawiec (1968:176-177) describe the experiment of Schafer and Murphy (1943). The experimenters devised drawings in such a way that either half of the drawing could be seen as a face. The faces were then cut out so that either could be presented separately. A training series was then initiated in which members of one group of children were 'rewarded' with small sums of money every time they were shown one face and 'punished' by losing a few pennies every time they were shown the alternate face. The faces were then combined and presented tachistoscopically at exposure time short enough to prevent the perceptual alternation which usually occurs if ambiguous figures are fixated for relatively long intervals. A significant difference was found in the direction in which directive-state theory would predict, namely, the rewarded face was seen--the punished face unnoticed.

Lazarus and McCleary (1949, in Stagner and Karowski, 1952:242-243) had subjects study a list of nonsense words. Certain syllables always were accompanied by shock. Such syllables, when seen later, gave rise to the GSR. Now, McCleary and Lazarus presented the syllables in a tachistoscope, at very short exposure times. Before the syllable was recognized, the GSR measures showed a sizeable difference between shock and non-shock syllables.

> (Such) studies suggest that some kind of basic recognition of a stimulus as 'good' or 'bad', rewarding or threatening, occurs faster, at a more primitive level, than conscious identification of the stimulus. Neurologically it may be that subception (unconscious perception) is based upon an emotional response mediated through the thalamus, to which sensory impulses go before they reach the cerebral cortex. Much experimentation is now being done along these lines, and in a few years psychologists expect to be much better informed about this aspect of perception (Stagner and Karwoski, 1952:243).

The effect of behavioral determinants on perception is powerfully demonstrated by noting several examples from the history of science. Kuhn (1962:115-121) offers the following examples when discussing scientific revolutions as changes in world view. Sir William Herschel's discovery of Uranus demonstrates the influence of sets on perception. Between 1690 and 1781, on at least seventeen different occasions, a number of astronomers had seen a "star" in positions which were occupied at that time, according to modern astronomy, by Uranus. One of the best observers in this group had actually seen the star on four successive nights in 1769 without noting the motion that could have suggested another identification. Herschell, when he first observed the object twelve years later with an improved telescope of his own manufacture, noted both the celestial body's disk-size, highly unusual for a star, and its motion among the stars, and announced that he had seen a new comet! Herschel's "star set" had effectively blinded him to the perception of the diagnostic planetary characteristics of the body. Kuhn (1962:116-117) writes:

> Can it conceivably be an accident, for example, that Western astronomers first saw change in the previously immutable heavens during the half-century after Copernicus' new paradigm (set) was first proposed? The Chinese, whose cosmological beliefs did not preclude celestial change, had recorded the appearance of many new stars in the heavens at a much earlier date. Also, even without the aid of a telescope, the Chinese had systematically recorded the appearance of sunspots centuries before these were seen by Galileo and his contemporaries. Nor were sunspots and a new star the only examples of celestial change to emerge in the heavens of Western astronomy immediately after Copernicus. Using traditional instruments, some as simple as a piece

> of thread, late sixteenth-century astronomers repeatedly discovered that comets wandered at will through the space previously reserved for the immutable planets and stars. The very ease and rapidity with which astronomers saw new things when looking at old objects with old instruments may make us wish to say that, after Copernicus, astronomers lived in a different world.

The history of the study of electricity offers further examples (after Kuhn, 1962:117-118). During the seventeenth century, when their research was guided by one or another effluvium theory, electricians repeatedly saw chaff particles rebound or fall from the electrified bodies that had attracted them. At least, that is what seventeenth-century observers said they saw, and, as Kuhn humorously notes, we have no more reason to doubt their reports of perception than our own. Placed before the same apparatus, a modern observer would see electrostatic repulsion rather than mechanical or gravitational rebounding. Similarly, researchers, after the assimilation of some of Franklin's notions, saw something different when looking at a Leyden jar than they had previously seen. And again, where Lavoisier had seen oxygen, Priestly has seen dephlogisticated air, and others had seen nothing at all.

Use of the verb "to see" in the above brings the point home. The differences being noted above are differences in perception and not differences in stimuli or sensations. "Reality" becomes difficult to pin down. What was "really" in the container that both Priestly and Lavoisier "saw," oxygen or dephlogisticated air? An even more interesting question is what will "really" be in that container ten years from now?

This line of thought raises issues which will be dealt with in more detail in the next chapter. What is the nature of an ethnography as a culture description? How should we evaluate what the ethnographer "sees" and describes?

At first glance the information so far discussed in this chapter would suggest support for the cognitivist position that language is a kind of theory or system of rules which structures perception or the manner in which the world is meaningfully ordered for the members of some society sharing a given culture. However, the above most forcefully demonstrates variables which differentially order experience rather than variables which consistently offer a static structure to experience. The "material" experience of the members of a culture is as variable and continually changing in terms of criterion of meaning as are the structural and behavioral determinants which are "brought to experience" by the bearers of some culture. At least with regard to the conclusions reached by contemporary perceptual psychology, the products of cognitive anthropology present an absurdly over-simplified version of meaningful, i.e., perceptual, organization in man. Language may be considered as one of the behavioral determinants of perception but by no means the only or major one as cognitive anthropology seems to imply. If cognitive anthropology had as its objective certain self-consciously limited goals, the information gained from a survey of perceptual psychology would not be that damaging. However, as has been noted, cognitive anthropology aims at total and real description of particular cultures, and it is at this point that it is open to the above criticism.

The notion, stated and implied, in the literature of cognitive anthropology, that the mind is logical and is thus reflected in the logic of experiential orderings as discoverable by the methods of cognitive anthropology is also questionable from the standpoint of perceptual psychology. There is no structured and integrating logic to motives, emotion, values, attitudes, sets, drives, and needs; and as the experiments

performed since World War II demonstrate, these variables can and do radically affect perception. The key here is "demonstrate." The studies in perceptual psychology are characterized by the self-conscious and rigorous demonstration of their contentions, whereas the opposing contentions of cognitive anthropology are left to be assumed as self-evident.

A difficulty arises in attempting to utilize data from perceptual psychology in a critique of cognitive anthropology because of the differences of levels which are involved--psychological on the one hand and cultural on the other. The significance of the comparison, however, maintains when the informant-centered emic thrust of cognitive anthropology is considered. The data of cognitive anthropology stems, by the avowed intent of cognitive anthropology, from the informant. In this sense psychological considerations must be applicable. The degree to which logic-less determinants affect perception is also applicable in a profound way to the ethnographer. This facet will be considered at greater length in Chapter V. In this instance it amazes me that cognitive anthropologists seem to be unaware of the "tyrannical hold" that their culture has bequeathed to them in viewing and in arguing for the "tyrannical hold" that other cultures exert on their members.

The significance of the language dependence of cognitive anthropology is also questioned by recent findings in perceptual and cognitive psychology. Few of the behavioral determinants of perception discussed above are demonstrably language dependent, yet all of them can be demonstrated to radically change the world as we see it. In a book dealing with cognitive psychology, Thomson (1959:164-165) writes:

> Thought cannot be simply identified with using language...Again the study of speech disorders due to brain injury or disease suggests that patients can think without having adequate control over their

> language. Some patients, for example, fail to find the names of objects presented to them and are unable to describe simple events which they witness; they even find it difficult to interpret long written notices. But they succeed in playing games of chess or draughts. They can use the concepts needed for chess playing or draughts playing but are unable to use many of the concepts in ordinary language. How they manage to do this we do not know. Yet animals such as Kohler's chimpanzees can solve problems by working out strategies such as the invention of implements or climbing aids when such animals have no language beyond a few simple warning cries. Intelligent of 'insightful' behavior is not dependent in the case of monkeys on language skills: presumably human beings have various capacities for thinking situations which are likewise independent of language.

Cognitive anthropology's position on thinking or cognition as a logical process based largely on logical contrasts and discriminations is also revealed to be simplistic by Thomson's (1959:207) conclusion to his historical survey of the field of cognitive psychology:

> It is necessary to keep in mind the fact that 'thinking' is a polymorphous concept which applies to a considerable range of different sorts of activity. Whether or not these different types of activity are related to each other, and, if so, in what specific ways, is a question which must be always kept in view. When a person is thinking, it is usually the case that several of these distinguishable sorts of activity are involved within the same situation--visual and other types of imagery; verbal contents; insights; performances which are the result of the evocation of prior learning; strugglings which are the steps towards the acquisition of new skills or concepts; goal directed behavior which conforms to well-established rule-following models and goal directed behavior which is exploratory in the means it adopts toward what may be an unfamiliar goal; operations which conform to strategies of a strictly logical form and leaps in the dark which appear unrelated to any other part of a long series of activities.

As I have continually noted, the field of cognitive anthropology operates with a series of assumptions pertaining to the concept of "mind." As Brown (1964:251) notes:

> For ethnoscience the mind seems to be a categorical grid imposed on reality, rendering some things equivalent and others nonequivalent. Since the cells of the grid are usually named, the design of the grid should be discoverable from inquiries about the meanings of words.

Brown (1964:251) opposes the mind conception of cognitive anthropology to the concept of mind developed by Piaget.

> For Piaget, intelligence is an activity; to think is to operate... The mind, for Piaget, is more transformer than template...To study the mind as a transformer, you have to ask subjects to do more than denote and define. Piaget...has asked them to solve problems, operate apparatus, and invent experiments...The methods of ethnoscience have, thus far, stayed close to the methods of descriptive linguistics and especially to the methods for deriving 'emics' from 'etics.'

Thomson's (1959:84) critique of Bruner's work with the formation of "simple class concepts based on the discrimination of certain easily identifiable attributes" is apt for the "class concept" orientation of cognitive anthropology generally.

> ...his (Bruner's researchers confine themselves to what is only part of a much wider field of conceptual behavior--namely, the case in which a simple class concept is formed on the basis of the discrimination of certain easily identifiable attributes. There is no doubt that human beings do attain and use such concepts in their normal thinking, and that classification, on this basis, plays an important part in the organization of perceptual data. But this is not the only type of concept which we use in our everyday thinking, nor is it even the only variety of class concept. Not all classification is based upon our ability to discriminate perceptual cues, and not every empirically grounded concept is a class concept... Accordingly, our knowledge of concept attainment cannot yet throw much light on 'thinking' in general.

The most powerful contemporary "school" in psychology dealing with perception is represented by "transactional functionalism" developed by Ames and his associates at the Hanover Institute for Associated Research. Ames and his group have devised some of the most remarkable perceptual experiments in the entire history of experimental psychology (Chaplin and Krawiec, 1968:170). This approach runs counter to the perceptual and cognitive assumptions of cognitive anthropology by emphasizing the interactive nature of perception. Transactional functionalism is specifically in opposition to Gestalt-like approaches in the manner in which it de-emphasizes inherent organizing factors in perception, while demonstrating

the degree to which perceptions vary depending on the particular nature of the transactions of man with his environment.

Proponents of cognitive anthropology have argued that the "stimulus saturation" type of experiment in psychology serves to support their contention that the human mind is composed of a finite number of structures which can handle only a limited quantity of in-put. This serves to explain, for them, the typical blocking reaction of over-stimulated subjects. I would simply like to note that another manner of explaining the reaction of the stimulus saturated subject is by noting that "rate of in-put" is more experimentally equal to the task of explanation as a crucial variable than is appeal to the "black-box" of the mind.

In this chapter I have attempted to illustrate the alternatives, presented by perceptual and cognitive psychology to cognitive anthropology's position on perception, cognition, and the concept of mind. What I think this chapter demonstrates is that the various positions which are in opposition to cognitive anthropology's understanding of these concepts argue from a stronger base--a base composed of a long history of particular specialization in the fields of perceptual and cognitive study, and a base which is experimentally rather than speculatively grounded.

In a famous passage the philosopher Whitehead (1932:68-69) writes:

> Thus nature gets credit which should in truth be reserved for ourselves: the rose for its scent: the nightingale for its song: and the sun for its radiance. The poets are entirely mistaken. They should address their lyrics to themselves, and should turn them into odes of self-congratulation on the excellency of the human mind. Nature is a dull affair, soundless, scentless, colourless, merely the hurrying of material, endless, meaningless.

V

GENERAL CRITIQUE

This chapter presents a "general" critique in the sense that it will deal with a limited range of problems raised by the assumptions, objectives, methods, and the overall self-conception of cognitive anthropology. Matters of technique competence in cognitive anthropology are beyond my abilities and, if the cognitive literature bears witness, beyond the abilities of the majority of commentators on the field. However, a highly specific knowledge of symbolic logic, Boolean algebra, set theory, and calculus, as well as a detailed presentation of the ethnographic data utilized in the cognitive literature is not necessary in order to discuss the bedrock foundation upon which cognitive anthropology rests. Choice of techniques is subsequent to, and dependent upon, the presuppositions inherent in cognitive anthropology; and the nature of the data presented in the cognitive literature is a reflex of specific techniques guided by objectives which are condoned and indicated by the particular assumptions and presuppositions of cognitive anthropology concerning the nature of the universe in which they are operating.

Beginning at the beginning, then, leads directly to a consideration of the ontological position of cognitive anthropology. Ontology is here construed as that branch of metaphysics dealing with the nature of being or reality. The French philosopher of science, Emile Meyerson (in Brody & Capaldi, 1968:59), notes:

> Science is not positive and does not even contain positive data in the precise meaning which Auguste Comte and his adherents have given to this term--that is, data 'stripped of all ontology.' Ontology is

> of a piece with science itself and cannot be separated from it. Those who pretend to separate them are unconsciously using a current metaphysical system, a common sense more or less transformed by science of the past, which is familiar to them.

Ironic as it may seem, I feel that the position of cognitive anthropology with regard to the nature of reality is best described as a kind of Realism. This appears ironic when cognitive anthropology's position concerning the nature of language, thought, and the mind is considered. As was noted when discussed, cognitive anthropology's position that knowledge of reality is variable and dependent upon the particular mental structures of the bearers of some culture as discoverable through language is most adequately described and understood in terms of Rationalism and Idealism. It is as if the cognitive anthropologist were arguing that everyone else in the world lives in accord with their particular culturally provided mental fiction except cognitive anthropologists who deal only with pure, demonstrable, and reproducable fact. This kind of confusion is reflected in cognitive anthropology in a number of ways. The emic/etic distinction and the empirical/formal distinction, with respect to the nature of the kind of science which cognitivists feel anthropology should be, all mirror the kinds of confusion that occur when consistency is not self-consciously sought at the level of basic assumptions.

The naive realism of cognitive anthropology is exemplified by the field's zealous concern with the discovery and description of facts, and the implicit belief that truth will somehow appear from a large enough body of properly collected facts. With the famous philosopher of science, Norwood Hanson (in Brody & Capaldi, 1968:150), the cognitive anthropologist would agree that "...scientists do not start from hypothesis, they start from data." As this chapter will demonstrate, I reject this position.

Further, I feel that this combination of inductive discovery procedures and formal or deductive validating and demonstration procedures only serves to cloud the proper appreciation of precisely what cognitive anthropology is about, both from the standpoint of the cognitive anthropologist and from the standpoint of those who attempt to evaluate his product.

When commenting on B.B. Colby's discussion of ethnographic semantics, G.L. Trager (1966:25) writes:

> The 'new phase in descriptive ethnography,' that Colby notes, is not new--it is merely a long-neglected continuation of the old, tried and true methods of Boas and all the founding fathers of our field. True, they were followed by a generation of much lesser stature, with a kind of inbred fear of language and its uses, and the 'great thinkers' of that generation went in for theory, or what they thought was theory, without bothering with troublesome data. There are now once again some anthropologists who insist on recording data, and who don't care if the theory doesn't fit the data. In this sense there is a new interest, but it is renewed rather than new.

The notion that fact is prior to theory is echoed by Stephen A. Tyler in the introduction to his book Cognitive Anthropology. He (Tyler, 1969:1) states, "When the descriptive facts of science no longer fit the older explanatory models, it becomes necessary to discover new theories which will more adequately explain the accumulated data." The emphasis on the priority of fact is further demonstrated by Tyler in the following quotations:

> Rather than attempt to develop a general theory of culture, the best we can hope for at present is particular theories of culture. These theories will constitute complete, accurate descriptions of particular cognitive systems. Only when such particular descriptions are expressed in a single metalanguage with known logical properties will we have arrived at a general theory of culture (Tyler, 1969:14).

> Comparisons between systems can only be useful if the facts compared are truly comparable, and we cannot know what facts are comparable until the facts themselves are adequately described. When this is achieved, the units of comparison will be formal features rather than substantive variables (Tyler, 1969:15).

Goodenough (1956:173) also fancies himself an explorer in the real world when he writes, "The great problem for a science of man is how to get from the objective world of materiality...to the subjective world of form as it exists in what, for lack of a better term, we must call the minds of our fellow man." Goodenough further indicates his Realist stance when he derives human cognition from the "modal clustering of events" in nature. He (1963:253) states:

> No part of the real world, of course, is perfectly stable, though it may appear to hang in a state of balance for some time, especially when viewed macroscopically. But the repetition of events within it is never exact, merely a modal clustering of tracks. These modal clusterings, however, are essential to human cognition, for in their absence people would be unable to discern discontinuities in their surroundings by which to discriminate categories of phenomena...

The above quotation from Goodenough seems curiously at odds with the view of cognitive anthropology in that what is considered relevant material phenomena in a particular culture is a cultural product. If discernment of discontinuity in human cognition is dependent on modal clustering of events repeated in the real world, as Goodenough claims, what does this say for the raison d'etre of cognitive anthropology?

Goodenough (1963:253-254) continues:

> ...our concern is to record everything that is really happening, on the assumption that we are all-seeing observers and that, as scientists, we are interested only in observable facts. Not knowing what the facts that we observe mean, we count them and sort them statistically and invent theories to rationalize the results.

The notion that static facts exist in our environment to be perceived in a constant and pristine state was seriously challenged by the previous discussion of recent conclusions drawn by experimental psychologists working in the field of perception. As was seen, the acquisition of meaning from what we choose to call sense data was by no means a simple process. Indeed, the majority of studies dealing with perception has,

since World War II, tended to focus on the ways in which perception is determined and influenced by sets, expectations, attitudes, values, motivation, emotion, etc. The majority of contemporary philosophers of science, and scientists as well, would challenge Goodenough's contention above that "observable facts" are directly related to what is "really happening," i.e., that observable facts automatically explain the truth. Further, Goodenough's concern "to record everything that is really happening" is both naive and absurd, as well as impossible.

Kaplan (1964:131-132) writes:

> ...no human perception is immaculate, certainly no perception of any significance for science. Observation is already cognition, not just material for subsequent knowledge, and the possibility of error is as ever-present in this cognitive process as in the more obviously inferential ones. Seeing is believing because we do not just see something: we see that something is the case...An observation is made: it is the product of an active choice, not a passive exposure... Data are always data for some hypothesis or other; if, as the etymology suggests, they are what is given, the observer must have hypotheses to be eligible to receive them. In his Theory of Data Clyde Coombs proposes that the term 'data' be used for observations already interpreted in some particular way. I am saying that there are no other works of observations, though often the interpretation at work is far from explicit and clear.

Cohen and Nagel (1934:199) add:

> It is an utterly superficial view...that the truth is to be found by 'studying the facts.' It is superficial because no inquiry can even get under way until and unless some difficulty is felt in a practical or theoretical situation. It is the difficulty, or problem, which guides our search for some order among the facts, in terms of which the difficulty is to be removed...Facts must be selected for study on the basis of a hypothesis. In directing an inquiry, a hypothesis must of necessity regard some facts as significant and others as not.

Mach (in Brody & Capaldi, 1968:33) when discussing explanation in physics states:

> In the investigation of nature, we always and alone have to do with the finding of the best and simplest rules for the derivation of phenomena from one another. One fundamental fact is not at all more intelligible than another: the choice of fundamental facts is a matter of convenience, history, and custom.

Popper (in Brody & Capaldi, 1968:184) writes:

> Observation is always selective. It needs a chosen object, a definite task, an interest, a point of view, a problem. And its description presupposes a descriptive language, with property words; it presupposes similarity and classification which in terms presupposes interests, points of view, and problems. 'A hungry animal,' writes Katz, 'divides the environment into edible and inedible things. An animal in flight sees roads to escape and hiding places...Generally speaking, objects change...according to the needs of the animal.' We may add that objects can be classified, and can become similar or dissimilar, only in this way--by being related to needs and interests. This rule applies not only to animals but also to scientists. For the animal a point of view is provided by its needs, the task of the moment, and its expectations; for the scientists by his theoretical interests, the special problem under investigation, his conjectures and anticipations, and the theories which he accepts as a kind of background: his frame of reference, his 'horizon of expectations.'

Therefore, when G.L. Trager (1966:25) supports the aims of "anthropologists who insist on recording data...and who don't care if the theory doesn't fit the data," he is missing the point that recording data, and particularly scientific data, depends on a problem orientation or hypothesis which guides the recorder in selecting what is significant and what is not significant in the data. Popper (in Brody & Capaldi, 1968:184) offers the following comment concerning this point:

> Twenty-five years ago I tried to bring home the same point to a group of physics students in Vienna by beginning a lecture with the following instructions: 'Take pencil and paper; carefully observe, and write down what you have observed.' They asked, of course, what I wanted them to observe. Clearly the instruction, 'Observe!' is absurd. It is not even idiomatic, unless the object of the transitive verb can be taken as understood.

Tyler (1969:1) makes an error comparable to Trager's when he states, "When the descriptive facts of science no longer fit the older explanatory models, it becomes necessary to discover new theories which will more adequately explain the accumulated data." Once again, Tyler's "descriptive facts of science" are actually specific data which represent in a fundamental sense the answer to a specific question which was phrased

in terms of a body of previously unrelated facts, and which ordered these facts in terms of the particular criteria of the hypothesis, problem, or question. Simple, facts do not order theory, as Tyler assumes: theory orders facts.

A similar criticism can be directed toward Tyler's statement, previously quoted, that a general theory of culture "will arrive" only after a significant number of accurate culture descriptions are made. The only sense to this statement would be conditioned by an operation which posited a particular theory of culture to direct and guide the descriptions. The descriptions would then serve as one of the means to evaluate the theory originally presented.

Again, Tyler (1969:15) states, "Comparison between systems can only be useful if the facts compared are truly comparable, and we cannot know what facts are comparable until the facts themselves are adequately described." Like Popper's "Observe!" and Trager's "record," "compare" is equally pointless without an hypothesis which directs attention to which facts and to which features of which facts are to be compared and for what reason. The comparability of facts is not necessarily dependent on some conception of the intrinsic reality of the particular fact. Furthermore, any notion of what constitutes an "adequate" description in relation to comparison is dependent on some idea on what the point of the future comparison will be, an hypothesis which would serve to place limits on the problem of adequacy of description, an hypothesis which states what "description" will mean in a particular case. Similarly, the notion of "useful" in Tyler's statement above is ambiguous and suggests that it too is dependent on still further presuppositions and particular problem orientations which do not automatically spring from the facts.

Goodenough's previously quoted material in this chapter also exhibits what I consider to be cognitive anthropology's erroneous appreciation of the nature of fact and theory. With regard to Goodenough's two quotations cited earlier one could ask: What does repetition mean in this instance? Repetition of what? What is an event? How is modality calculated in nature? How does one decide what is included in a particular modal cluster? How can one record everything that is really happening? What is the basis for assuming that the scientist is an all-seeing observer? How does one define an observable fact? How does one acquire facts if they have no meaning? How can one possible count and statistically sort facts which have no meaning? The answers to all these questions depend upon a certain series of presuppositions and not, as Goodenough apparently assumed, upon self-evidence. The basis for the discovery procedures in cognitive anthropology as the general character of the preceding statements by Trager, Tyler, and Goodenough illustrate, is inductive in nature, even though the most loudly touted discovery methods of the field are formal and deductive.

The problem of how the cognitivist arrives at the delineation of a certain domain or universe of discourse to which he will apply his formal analytic techniques is crucial particularly because the product of his analysis will be claimed to have emic significance or perhaps psychological reality. It is at this level that the formal and deductive drive of cognitive anthropology breaks down. All analysis is dependent on this basic first step, and it is at this point that the cognitivists' strategy is most vague. The general opinion of the cognitivist seems to be that the domain will somehow appear upon inductive observation. Regarding the domain problem, Goodenough (1956:198) writes:

> Determining the universe: The first step in analysis is to gather together all expression whose denotata make it appear on inspection that there may be some common element in their significata; which is another way of saying that they appear to relate to the same subject matter.

All of the important steps in Goodenough's plan are necessarily etic; i.c., the observer is inspecting in the observer's terms, and it is he who decides that a common element is or is not present.

Pelto (1970:70), referring to Frake's article, "The Ethnographic Study of Cognitive Systems" (1963), notes:

> As a first step the anthropologist must identify particular 'segregates'--the meaningful behavioral items that are grouped together as sets of contrasting responses. He suggests that such forms will be found by observing verbal behavior, particularly bounded sociolinguistic contexts.

Frake, like Goodenough, seems to assume that the emic universe of discourse will simply appear upon observation of verbal behavior. It is also an etic decision as to when one can decide that he is observing a "bounded sociolinguistic context." The bounding is done by the investigator. Frake, however, offers no suggestion concerning how he arrives at his bounded contexts.

An oft cited attempt to solve the problem of domain discovery is the work of Metzger and Williams (1963a) which is based on programmed learning techniques. The basic notion is that the ethnographer learns correct word usage in a specified domain by referring to previously recorded native materials. "The process, in the form of verbatum statements of both ethnographer and informant, is presented as evidence so that the reader can judge for himself" (Colby, 1966:11). The inductive problem of infinite regression rears its head in this case too. What is the basis for ethnographer "A" accepting the emic validity of the eliciting frames utilized by ethnographer "B." Certainly temporal priority is not

sufficient. Furthermore, when the reader "judges for himself," he once again has escaped the emicist ideal.

To temporarily digress, another very real concern that can stem from the eliciting procedures suggested by such cognitivists as Metzger and Williams (1963a) and Frake (1964) relates to the delicate problems of ethnographer/informant rapport and informant fatigue. When discussing how to test if "discovered queries" are appropriate with respect to particular topics, Frake (1964, in Tyler, 1969:135) writes:

> As a test, the reader with a chair and an English speaking informant handy might try to detect which of the following queries are inappropriate:
>
> What kind of a chair is it?
> What does a chair taste like?
> What is a chair used for?
> What sex is that chair?
> What part of a chair is this?
> How fast is this chair?

A half hour of questions like that could push the most committed and sympathetic informant to the breaking point. It would be extremely difficult to utilize such techniques if instead of such neutral topics as firewood, color categories, sandwiches, or kinship terms, the topic was more emotionally loaded.

The major consideration at this point then is the emic/etic distinction and the nature of induction, for clearly the domains analyzed by the cognitive anthropologist are inductively generated to be deductively analyzed. What then of emic results? Can emics be derived from etics? These questions lead to consideration of the results of defining "emic" in two different manners. One emphasis would view emic statements as psychologically real presentations of the native's viewpoint. Another tendency is to stress that emic statements represent a calculus, a model, or an accurate approximation of the native's categorization of a certain

realm which can be tested by the ethnographer's ability to appropriately anticipate behavior.

Goodenough (1956), for example, repeatedly states in his paper on Trukese terminology that the purpose of componential analysis of kinship terms is to provide psychologically real definitions. Regarding this article, Wallace and Atkins (1960, in Tyler, 1969:363) note:

> He (Goodenough) speaks of people having 'certain criteria in mind by which they make the judgement that A is or is not B's cousin'; he alludes to the method as a means of learning about 'human cognitive processes'; he discusses 'concepts' which exist in 'the Trukese cognitive world'. In his earlier monograph on Truk he justifies the choice of components by characterizing them as 'criteria' or 'rules' valid in 'Trukese thinking,' by which the Trukese 'appraises his relationship with another individual'.

Anthony F.C. Wallace also argues that psychological reality is the goal of formal analysis (Hammer, 1968:527; Colby, 1966:8-9). Hymes (1964b) and Romney and D'Andrade (1964) also uphold this position. For all, the test that psychological reality has been achieved is based around the test by appropriate anticipation stratagem.

The hopes of those authors who believe in the possibility of arriving at psychologically real statements via formal methods of analysis is seriously challanged by Burling (1964) in his paper "Cognition and Componential Analysis: God's Truth or Hocus-Pocus." Burling denies that cognitive studies provide insight into the cognitive processes of peoples. He bases his opinion on the fact that there are a great number of logically poss ⌐le alternatives for grouping sets of even a very few items. He (1964:23) states, for example, that there is "a total of 124 ways in which a set of four terms can be discreetly but nonredundantly apportioned into cells by the application of components. Clearly with five or more items the possibilities would rapidly become astronomical." He further notes that the componential analysts have a whole series of

other logical problems to cope with, including homonomy (splitting a single term into two different meaning clusters), nonbinary components, and redundancy (Pelto, 1970:73). Burling (1964:27) concludes by stating:

> It is always tempting to attribute something more important to one's work than tinkering with a rough set of operational devices. It certainly sounds more exciting to say we are 'discovering the cognitive systems of the people' than to admit that we are just fiddling with a set of rules which allow us to use terms the way others do.

The problem that Burling raises for those who claim to be delineating psychologically real cognitive systems of a people is this: of the many logically complete and coherent deductive orderings of a specific domain, how does the analyst arrive at the decision that his particular analysis is psychologically valid for the bearers of a certain culture.

Hymes (1964d:116) and Frake (1964b:119) have responded to Burling's paper. Hymes claims that the superfluous possible logical solutions of ordering are eliminated by the field worker who carefully elicits the appropriate terms from informants in relevant sociolinguistic contexts. In this way, according to Hymes, "God's truth" can be approximated (Colby, 1966:12). However, two points can be made at Hymes' expense. The emic goal is questionable when, as previously noted, the few statements of eliciting procedures by cognitivist demonstrate their inductive or etic nature. Further, how can a psychologically real statement of cognitive process be approximated if the analyst admits that he is attempting to discover precisely that statement in the first place? If the analyst does not know the logico-semantic characteristics of a particular domain, how can he possibly know when he has approximated them?

In Frake's (1964:119) reply to Burling, the cognitivist's major validating criteria, appropriate anticipation of informant response, is

cited as an answer to Burling's criticism. Burling (1964, in Manners and Kaplan, 1968:521) in his "Rejoiner" to Hymes and Frake writes:

> My only justification for writing yet another programmatic article was that I have tried to analyse various sets of terms, and I have not found the proposed methods adequate. I have faced horrendous problems of alternative possibilities, and I felt that part of the difficulty stemmed from the logical problems raised in my paper. My colleagues may well be more clever and successful in these analyses than I. But when Goodenough suggests an intricate distinction between 'lineal' and 'ablineal,' and 'colineal' to help in ordering English kin terms, I am not persuaded that he is approaching anyone's cognitive system though he is certainly proposing a scheme that works. When Frake confidently tells us that for a Subanun 'A case of nuka may eventually develop into one of 23 more serious diseases' (no 'about 23' or 'over 20' but just '23), I suspect the imposition of a spurious precision.

I feel that Burling's criticism stands well against the feeble attempts to debunk it. The cognitivist attempts to decide between two equally complete formal accounts of a particular domain as being the psychologically real one must ultimately rest with certain criteria that are brought to bear from an etic standpoint, i.e., in terms of distinctions appropriate to the analyst, and not to the native.

Romney and D'Andrade (1964) attempted to maintain the validity of the quest for psychologically real definitions by asserting that supporting evidence to argue the psychological reality of one formal account over another can be achieved by "further behavioral measures." It could then be asked: "How does one establish the emic validity of a test which tests for emic validity?"

That Burling had made his point in the debate with Hymes and Frake can be seen in the fact that there was a general "drawing-in-of-horns" about that time with regard to the question of the psychological reality of so-called cognitive descriptions perporting to represent a native speaker's cognitive world. Wallace (1965, in Tyler, 1969:399) writes:

> ...awareness of the problematic nature of psychological validity sprang historically from the apparent indeterminacy of the results of componential analysis. The publications on componential analysis to date, and no doubt unpublished experiences of others like myself, suggest that the application of its procedures to lexical and denotative data does not automatically yield a unique description of a native cognitive (or semantic) system. This is not to say that such descriptions are inaccurate in predicting the usage of a native speaker.

My suspicion is that the cognitivists have not abandoned their belief that it is possible to "get inside the native's head." As the relevant literature since 1964 demonstrates, they have merely become more cautious in how they talk about their goal. Terms like "calculus" and "model" increasingly replace "psychologically real" in the cognitivist literature.

The continuing primary means, however, by which cognitivists claim the descriptive validity of their analyses is simply informant response. This aspect of cognitivist strategy is highly susceptible to criticism. Colby (1966:12) states when discussing Hymes:

> When Hymes speaks of prediction, he means mainly an affirmative informant response to the correct naming of objects in the environment showing that the meaning has been attained by the investigator. The drawback to such a criterion is that the various semantic principles and components applied by the investigator when he decides whether an object is designated by a specific lexical unit may not always be conscious to him...Even if the investigator is fully conscious of all the semantic criteria he uses in testing word usage, an affirmative informant response does not necessarily mean the criteria are those used by native speakers.

Pelto (1970:85) notes:

> The test of 'correctness' in...field research strategies is the same--its empirically determined productivity. The complete outsider's set of survey questions put to a sample of the local population is often empty of meaning and devoid of predictability...But the local people are not invariably better than the ethnographer in categorizing their own social reality for the simple reason that (1) they are not social scientists, and (2) their (arbitrary) categorizations were not constructed for the purposes of cross-cultural study of behavior systems...Neither the 'insider' nor the 'outsider' in the cultural

> scene has the answers for appropriate categories and definitions of behavioral facts. The appropriate categories depend on their predictive consequences in research.

With regard to the cognitivists' stress on "appropriate anticipation" rather than "prediction," Harris (1968:572) remarks: "Since no attention has been devoted to the question of how to proceed with this operation, it cannot be taken at once both literally and seriously." At another point Harris (1968:575) remarks:

> There are certain options which in their most subtle form defeat the emic/etic distinction. Thus it is a commonplace of psychoanalytical research and practice that the actor is regarded as a poor observer of his own inner states.

Some general comments from various philosophers of science may be brought to bear on this problem--theory and its validation. Kaplan (1964: 315) notes:

> ...theories cannot be validated as though they were wholly self-contained. It is simply a mistake...to suppose that validation consists in confronting 'the' theory with 'the' observation. Other theories and facts are always involved--for instance, those bearing on the instruments of observation.

Particularistic cognitive theories are entirely self-contained involving no other theories or facts but those present when "the" theory confronts "the" observation in anticipating word usage for example. Also, as has been noted, cognitivists typically pay little attention to problems involving the ethnographer as "the instrument of observation," preferring to stress matters of technique.

When discussing the structure of theories in the social sciences, Walter Wallace (1971:92) writes:

> ...in the sciences we want to know not only how things 'have worked' in the past, not only how things 'will work' in the future, but both--more than that, we want to know both in one statement. In short, we want to know how things '<u>must</u> work,'...this simultaneous backward and forward reach of theory <u>may</u> be considered the primary manifestation of the way science pursues necessity...

The theories of cognitive organization in a particular domain as developed by cognitivists in terms of appropriate response are, however, entirely static. That in itself is no crime except that the manner of validation implies that language is a static phenomenon, a presumption that could hardly be defended. If correct informant response and appropriate anticipation argue the correctness of the theory in general, the correctness of the theory in terms of future application would also depend on future informants giving the same set of responses. This would seem most unlikely especially given a goodly amount of time between the first instance of validation and subsequent testing of the theory of cognitive organization by means of language. Further, there would be no way to argue the retrodictive power of the theory, unless an assumption concerning language as a static phenomenon could be supported.

A problem related to cognitivist validation procedures is suggested by Weatherall (1969:176) when he states: "...hypotheses become acceptable if they are supported sufficiently often and never refuted." In other words, it is not enough that a theory be supported in observation unless the clear possibility of its refutation also exists and is stated. It cannot be known how a theory is right if it is not also possible to say how it could be wrong. Popper (1959:40-41), in his The Logic of Scientific Discovery, writes:

> I shall not require of a scientific system that it shall be capable of being singled out, once and for all, in a positive sense; but I shall require that its logical form shall be such that it can be singled out, by means of empirical tests, in a negative sense; it must be possible for an empirical scientific system to be refuted by experience. Thus the statement 'It will rain or not rain tomorrow' will not be regarded as empirical, simply because it cannot be refuted; whereas the statement 'It will rain here tomorrow' will be regarded as empirical.

A particular delineation of some cognitive organization can be refuted, according to the cognitivists, if it fails to allow the ethnographer to make appropriate anticipations of verbal behavior. However, the basic assumption that cognitive processes are discoverable in language cannot be refuted using cognitivist validation criteria, because of the many alternative logical orderings which can allow the ethnographer to make the same anticipation. There is nothing in any formulation of cognitivist "theory," nor in related validation techniques, that states how the ultimate cognitivist orientation (cognition through language) could be falsified. Its immunity to potential falsification is antithetical to the scienticity which the cognitivists claim. Cognitivist validating procedures merely allow them to state that they have or have not formulated a set of rules which permit them to use words in a sense "equivalent" to native word use. Nothing can be said concerning cognitive processes or their approximations! Where, then, is the "cognitive" in cognitive anthropology? The only cognitive processes that are even vaguely outlined by cognitive methods are the ethnographers.

A further criticism of the extravagant claims of cognitive anthropology is summed up in Harris' (1968:591) phrase, "The Science of Trivia." Berreman (1966:351) when discussing the accomplishments of cognitive research writes:

> None of these descriptions, whatever their virtues, can in themselves be called significant...they remind of Mills' warning that many sociologists have gotten to the point where they overlook what is important in their search for what is verifiable...many have worked so hard on what is trivial that it comes to appear important.

Kaplan (1964:406) makes a similar kind of statement when writing of the future of behavioral science.

> Many behavioral scientists, I am afraid, look to methodology as a source of salvation: their expectation is that if only they are willing and obedient, though their sins are like scarlet, they shall be as white as show. Methodology is not turned to only as and when specific methodological difficulties arise in the course of particular inquiries: it is made all encompassing, a faith in which the tormented inquirer can hope to be reborn to a new life. If there are such illusions, it has been my purpose to be disillusioning. In these matters, the performance of the ritual leaves everything unchanged, and methodological precepts are likely to be as ineffective as moral exhortations usually are...There are behavioral scientists who, in their desperate search for scientific status, give the impression that they don't care what they do if only they do it right: substance gives way to form. And here a vicious circle is engendered; when the outcome is seen as empty, this is taken as pointing all the more to the need for a better methodology. The work of the behavioral scientist might well become methodologically sounder if only he did not try so hard to be so scientific!

Pelto (1970:74) adds: "Given that the field work was carefully done, and the results are sound, one wonders what theoretical use other anthropologists will make, for example, of descriptions of Tzeltal firewood or ingredients for making beer among the Subanun." In a similar vein, Harris (1968:592) writes:

> It would appear on balance, however, that the net contribution to substantive theory is less than what usually results from equivalent labor in-puts. For example, Conklin's conclusion that the Hanunoo's terminological specialization in cousin terms reflects payment of fines in accordance with degree of incest scarcely requires the elaborate descriptive apparatus with which it is juxtaposed.

Hammer (1968:523) continues:

> I am pleased when Conklin tells us that the decisive division among American units of monetary exchange is between 'coins' and 'bills' or when Haugen shows us that Icelandic noror, austir, suor, and vestr do not always mean the cardinal directions north, east, south, and west, but may also mean 'in the direction leading ultimately to the north (east, south, west) quarter of the island,' but we hardly need the elaborate terminology and complex methodological apparatus which has grown up around componential analysis to tell us these things.

The product of the cognitivists is indeed trivial when it is considered that the champions of the field have been active for the last ten to fifteen years and have agreed fervently with Sturtevant's (1964:101)

statement; "ethnoscience shows promise as the New Ethnography required to advance the whole of cultural anthropology."

The question of cultural homogeneity, generally assumed by the cognitivists, has drawn criticism from Pelto and Harris. Pelto (1970:75) writes:

> It is assumed that there is one 'right' description of, or logical organization of, a given semantic domain, and that all or most of the members of a given society 'know' that particular system. The componential analysts do not generally explore the significance of variation from person to person in organization or cognitive domains... The ethnoscientists, therefore, are seldom concerned with details of sampling and representativeness in the cultures they study. Their published descriptions are set forth as the single 'best' unitary system derivable from the given culture. In the area of nonverbal behavior the assumptions of cultural homogeneity and unambiguity become strained even more than they were in the matter of folk taxonomies.

Harris (1968:590) continues this theme:

> If permitted to develop unchecked, the tendency to write ethnographies in accord with emic rules of behavior will result in an unintentional parody of the human condition. Applied to our own culture it would conjure up a way of life in which men tip their hats to ladies; youths defer to old people in public conveyances; unwed mothers are a rarity; citizens go to the aid of law enforcement officers...television repairmen fix television sets.

As Pelto and Harris point out, the cognitivist faces the problem of intracultural variations. By what criteria, and how, do the cognitivists conclude that a particular informant is representative of his group? This question is never raised, and it is significant! As contemporary perceptual psychology has demonstrated, there is a mass of variables affecting differential responses from the same sample to perceptual and cognitive cues. There is a vast body of literature in psychology which flatly denies the assumption of cultural homogeneity made by the cognitivists.

The excessively idealist theory of human behavior filtered through a computer model has also led the cognitivist to assume unambiguity as a

basic feature of human cognitive processes. Logical exhaustiveness of a domain is a sought after feature of formal modes of analysis but this does not argue that it is a necessary feature of cognition. With regard to his analysis of American kinship terms, Schneider (1965:291) refers to the "fuzzy boundary and fadeout principle" of this domain in American culture. Harris (1968:587) has noted; "None of the attempts to define basic cognitive features of American kinship terminology has thus far made concessions to the possibility that ambiguity is one of the salient characteristics of this domain." It should also be noted that ambiguity, paradox and contradiction are basic in such human endeavors as art, poetry, and philosophy.

Arch-eticist, Marvin Harris (1964;1968), has been a major critic of cognitive anthropology. Some of his criticisms appear well founded. Harris (1968:570) states:

> The pairing of structural results with emics and nonstructural results with etics accords with the history of linguistics. But there is no reason to suppose that this equation must hold for non-linguistic phenomena. There are structures in an atom, a molecule, a cell, and an organism, the description of none of which depends upon emic operations. Why should we not also assume that there are sociocultural systems whose structures can be exposed independently of procedures modeled after phonemic analysis.

Harris has also pointed out the implications for cognitive anthropology of the rare instance when several cognitive oriented researchers independently attacked the same domain with cognitivist methods and assumptions. American kinship terminology was analyzed by Wallace and Atkins (1960), Romney and D'Andrade (1964), and Goodenough (1965). None of the three analyses completely agreed. If intersubjective testing can be utilized as a means to assess the scientific validity of a research method, cognitive anthropology in this case failed. Harris (1968:588) notes:

> ...the failure of American ethnologists to agree on the analysis of their own native terminology, the evident tendency among ethnosemanticists to accept the cognitive expertize of the well-informed informant, and the failure to accomodate the possibility of functional ambiguity suggests that the ethnosemanticists must take a more critical look at their basic assumptions...At the very least, the difficulties and shortcomings of the treatment of American terminology provide a firm basis for skepticism regarding the attempt to push the study of major terminological systems into numerous refinements which are possible if terms for kin types representing all shades of ambiguity and psychological and social significance are jammed into the same frame.

Pelto's (1970:74) general critique of cognitive anthropology relates to the fact that the cognitivists offer "...no comprehensive statement of theory in connection with which their methodological practices are invoked...." Pelto (1970:74) states:

> ...we are struck with the question 'But what theoretical problems are the componential analysts attempting to solve?' It would appear that the method has some pertinence to the general problems of describing folk taxonomic systems. However, as proponents of a new kind of taxonomic system for analysis of <u>all</u> cultural problems, the componential analysts (and other formal semantic analysts) have provided little in the way of a general theoretical framework, and they appear to believe that accurate description of semantic domains is a useful end in itself. (Most of the works in componential analysis are programmatic papers on the new research methods, and provide only partial descriptions of the quite limited semantic domains with which they concern themselves).

Sturtevant (1964:123) notes another difficulty with the cognitivists' program:

> Ethnoscience raises the standards of reliability, validity, and exhaustiveness in ethnography. One result is that the ideal goal of a complete ethnography is farther removed from practical attainment. The full ethnoscientific description of a simgle culture would require many thousands of pages published after many years of intensive field work based on ethnographic methods more complete and more advanced than are now available.

I would only note that a more useful program of ethnographic methodology should make the ideal of a complete ethnography more attainable instead of rendering it "farther removed from practical attainment."

Further, the notion that "Ethnoscience raises the standards of reliability, validity, and exhaustiveness in ethnography" is based on the highly questionable validating techniques of the field.

In my study of cognitivist literature I find the emic/etic distinction, a major conceptual underpinning of cognitive anthropology, most problematic. Cognitivists attempt to derive emic descriptions of certain domains and to eliminate ethnographer bias. The problem is that all observation is biased, i.e., etic. Without etic distinctions and a priori perceptual sets, an ethnographer would not recognize a native, let alone his mind, or his cognitive processes. To perceive that this quantity is a man and not a tree, to perceive that this man rather than that man is a native, to perceive that he demonstrates behavior, to perceive that his behavior illustrates cognitive processes, to perceive that these processes may be logical, to perceive that a certain technique rather than some other technique will demonstrate the logical structure of his cognitive mode, and to perceive that a particular cognitive delineation is valid involves etic distinctions, i.e., "...distinctions judged appropriate by the community of scientific observers" (Harris, 1968:575). It would be logically and epistemologically impossible to derive "emics" from the above basic series of perceptual operations. To claim that emically valid descriptions are achieved by cognitivist techniques is an affirmation of faith and mysticism rather than sound rational demonstration.

The frustrating irony of cognitive anthropology is that it contains in its basic tenets the stuff of its own destruction. It follows Sapir in basing its program on the assumption of the hold language exhibits over class perception and discrimination, and then ignores that premise by considering "etics" to be culture-free features of the real world. Cognitivists

would view as "stifling to ethnoscientific analysis" (Sturtevant, 1964: 105), Murdock's (1945:125) assumption that "cultures...have in common... a uniform system of classification...a single basic plan," and then utilize Aristotlean logic as the basis for a common descriptive language of all human systems of classification. Cognitivists acknowledge the question-dependent-answer thesis of Collingwood and others (Tyler, 1969: 141) but ignore the entire basic thesis when they claim emic validity for an analysis the source of which is founded in an etically delineated domain.

The overall theoretical framework of cognitive anthropology is difficult to assess because as Pelto (1970:74) notes above, it is largely nonexistent. It would be most accurate to evaluate cognitive anthropology as a "point-of-view" immune to falsification and verification, couched in a Newtonian conception of nature and science, which has been dated in terms of scientific thought and endeavors since the 19th century.

VI

CONCLUSION

Arthur S. Eddington (1881-1944) was an outstanding English astronomer whose major scientific achievements were in the fields of astrophysics and relativity theory. He was Einstein's chief assistant on a trip to West Africa in 1919 to observe the solar eclipse. An experiment carried out during that eclipse proved that light from a distant star would be bent as it passed the sun. This experiment helped to confirm the general theory of relativity. "Eddington's name is well known to the general public because of his attempts to reconcile relativity with more traditional beliefs held by man" (Brody & Capaldi, 1968:37).

Eddington's account of his two tables offers an elementary exposition by a genius of theoretical physics of a basic problem faced by men doing science. Simple, it is the problematic relationship between "social reality" and "scientific reality." In his The Nature of the Physical World (1928:ix-xii), Eddington writes:

> I have settled down to the task of writing these lectures and have drawn up my chairs to my two tables. Two tables! Yes, there are duplicates of every object about me--two tables, two chairs, two pens...One of them (tables) has been familiar to me from earliest years. It is a commonplace object of that environment which I call the world. How shall I describe it? It has extension; it is comparatively permanent; it is coloured; above all it is substantial... It is a thing; not like space, which is a mere negation; nor like time, which is--Heaven knows what!
>
> Table No. 2 is my scientific table. It is a more recent acquaintance and I do not feel so familiar with it. It does not belong to the world previously mentioned--that world which spontaneously appears around me when I open my eyes. There is nothing substantial about my second table. It is merely all empty space--space pervaded,

> it is true, by fields of force, but these are assigned to the category of 'influences', not of 'things'.
>
> I need not tell you that modern physics has by delicate text and remorseless logic assured me that my second scientific table is the only one which is really there--where ever 'there' may be. On the other hand I need not tell you that modern physics will never succeed in exorcizing that first table--strange compound of external nature, mental imagery and inherited prejeduce--which lies visible to my eyes and tangible to my grasp.

Eddington, at the outset of his introduction to theoretical physics, presents the social/scientific reality problem. For him it is a basic foundation for the understanding of high level scientific theory and theory building. Cognitivists acknowledge this distinction in one version of the emic/etic differentiation, then loose it totally with their emic theoretical emphasis: "Yes there are at least two distinct reality levels which the anthropologist must deal with," and "No, there is only one." Logically the cognitivists cannot have both without running the risk of continued contradiction. As was noted in the preceeding chapter, internal contradiction must be cited as one of the major negative characteristics of cognitive anthropology.

Heisenberg (in Brody & Capaldi, 1968:53) has noted that "...science is not concerned with nature itself, but with nature as man describes and understands it." The cognitivists stress the study of how man describes and understands nature, but what they apparently do not see is that for the anthropologist man is the natural phenomenon which is scientifically studied. It may sound like a cold and Byzantine statement, but for the anthropologist doing science the "real" nature of man is not the central goal of inquiry but rather how anthropology as a science can better describe and understand man in anthropological terms. For anthropologists interested in the progress of theory in the field, man and his behavior are manipulatable data, no more, no less. To be concerned with formally

and scientifically delineating the "real cognitive structures" of a given people, for example, is to confuse two different universes of discourse. Science does not deal with the real, it deals with the valid; it does not deal with facts, it deals with data. I see resistence to this notion (man as data, not fact) as very similar to the popular outcry raised against Darwinian theory in the 19th century. The code of "social reality" could not tolerate the grave status change which Darwin's theory (science reality) dictated. Man was supposed to be special in the realm of nature, not just another animal. Of course, in "scientific reality," man is data, not social fact; he does not have status problems. According to Darwinian theory, man is just another animal.

In terms of the social reality/scientific reality distinction, however, the humanistic face of anthropology retains its place along with the cool face of anthropology as a science. The anthropologist as humanist and teacher is a translator. He must be capable of rendering the language of the scientific reality of anthropology into the social reality in which the anthropologist finds himself. This is what the humanistic anthropologist actually does, though he may not realize it. Realizing it, and controlling it, however, is the essence of the anthropologist's art.

The major problem here, and I find it a glaring one, is that the anthropologist typically has little interest in research into the scientific aspects of the social reality in which he finds himself. Too often, he, like ordinary mortals, accepts the social reality into which he was born with his mother's milk and rarely considers what must stand as one of anthropology's major contributions to human thought, the recognition of the special and restrictive lens socially transmitted as a culture in

a particular time in a particular place. Simply, the anthropologist is a cultural product too!

The effect of personal bias in anthropological literature has been noted only in those instances in which intersubjectivity has occurred in a particular area of study. Two such classic encounters happened in the independent studies of the Mexican village of Tepoztlan by Robert Redfield in the 1920's and Oscar Lewis' study some seventeen years later; and with the differing versions of Pueblo culture offered by Ruth Benedict, Laura Thompson, Esther Goldfrank, and Dorothy Eggan.

Regarding the Lewis/Redfield differences, Lewis (1951:428-429) writes:

> The impression given by Redfield's study of Tepoztlan is that of a relatively homogenous, isolated, smoothly functioning and well-integrated society made up of a contented and well-adjusted people. His picture of the village has a Rousseauan quality which glosses lightly over evidence of violence, disruption, cruelty, disease, suffering and maladjustment. We are told little of poverty, economic problems, or political schisms. Throughout his study we find an emphasis upon the cooperative and unifying factors in Tepoztecan society.
>
> Our findings, on the other hand, would emphasize the underlying individualism of Tepoztlan institutions and character, the lack of cooperation, the tensions between villages within the municipio, the schisms within the village, and the pervading quality of fear, envy and distrust.

Redfield (1960:135) in reply notes:

> The greater part of the explanation for the differences between the two reports on this matter of Tepoztecan life and character is to be found in differences between the two investigators...I think that it is simply true that...I looked at certain aspects of Tepoztecan life because they both interested and pleased me.

The differential accounts of Pueblo life began in 1934 with Ruth Benedict's Patterns of Culture. In that book the Pueblo people were characterized as restrained and moderate in all things. According to Benedict, they attempted to avoid violence, quarrels, and warfare. Laura Thompson rendered a similar portrait of the Pueblo people. In later studies

of the Pueblo lifeways (particularly Zuni and Hopi), Esther Goldfrank and Dorothy Eggan portrayed the Pueblo culture as traumatic, violent, and repressive.

Bennett (1956:211-212) studied both versions of Pueblo culture and concluded:

> The differences in viewpoints...cannot be explained entirely either on the basis of scientific goodness or badness, nor on the basis of publication differentials. Underneath both these factors lies what I have already suggested may be a genuine difference in value orientations and outlook in the feeling about the reaction toward Pueblo society and culture in the light of values in American culture brought to the scientific situation by the anthropologist... Scientific anthropology is...implicated in an on-going process in our culture, and from this level of observation, it is nonobjective and culturally determined.

The preceeding examples cited instances when the ethnographer, cool purveyor of the native life, was caught with his/her humanity showing. Since relatively few ethnographic studies are, or can be, intersubjectively assessed it is staggering to contemplate the nature of the biases which have produced the great libraries of ethnographic accounts which form the basis of ethnological theory.

The implication of the above to a consideration of cognitive anthropology is seen mainly in the cognitivists' naive attempt to eliminate ethnographer bias. This is a futile direction! A model of a human as a social creature could be ideally constructed upon the system of his perceptual sets of biases. A more practical attack to the question of ethnographer bias would be one in which bias is acknowledged as the norm of perception and which sets out to expose bias and structure it in terms of rational thought and description. Contrary to what cognitivists seem to think, the real cannot be formally, rationally, or scientifically "bottled" for the edification of future generations of anthropologists.

Cognitivists in their search for descriptive purity and "reality" have boxed themselves into a paradigm based on a Newtonian conception of the universe; a universe which contains an inherently rational order discoverable by scientific techniques. This is the popular reality-view of the culture of the cognitivists. What the cognitivists do not realize is that Newton's universe was effectively blown away by the first quiver of relativity theory in the late 19th century. Contemporary science no longer seeks the really real: it seeks the theoretically valid.

The cognitivist stance is set in a social reality which is based in a Newtonian naive realism. Nature and mind are intrinsically rational and orderly, according to this view, this paradigm, this methodology. The cognitivist then views his subject matter through the lens of his social reality's version of ontology and the result is the measuring of another's corn by one's own bushel. This is a minor crime to the cognitivist but in my opinion absolutely inevitable in view of the fact that all scientists are human.

The most insideous and pervasive error of cognitivist methodology is the search for and the fascination with the "real." Reality, it seems to me, is a major concern in the social realm and therefore where ever it is defined it is a culture trait marked by spatial/temporal features. Contemporary physics, the mother of Western man's ontology, on the other hand, has begun to escape the emotion evoking call for reality finding. For anthropologists to follow the cognitivist methodology and paradigm would be to regress instead of progress in anthropological theory generally.

What factors can be suggested to account for the rise of cognitive methodology in American anthropology? In no special order of importance, several factors can be mentioned. Colby (1966:6) notes one such factor.

> The current swing away from behaviorism and back to an interest in cognition is undoubtedly influenced by the place computers have in our society...The great interest in generative grammars and the associated terminology...may indicate the same influence among anthropologists.

In reply to comments on his "Ethnographic Semantics: A Preliminary Survey," Colby (1966:27) adds:

> One might characterize the new American emphasis as a conservative revolution...(a) limited outlook is also indicative of a conservative view...Conservative also is the view of culture as a mental code or set of rules. Though this view is attractive to me, I am not sure it is the most fruitful one. I think one reason it has caught on is that it is mentally assuaging. It reduces everything to a set of logical relationships that are clear and unambiguous.

In "The Psychic Unity of Human Groups," Wallace (1968:504) offers his whimsical appraisal of the persistence of interest in cognition in the social sciences: "Indeed, one may suspect that the social sciences have nourished the idea of cognitive sharing for so long, just because the world would seem rather a lonely place if the wistful dream of mutual identification is abandoned."

A more practical reason for the appeal of cognitive anthropology may relate to the often noted rapid "disappearance" of exotic cultures around the world. A native language, the chief avenue of the cognitivist, may remain while the more dramatic extrinsic traits of a people have disappeared. Thus, the Comanche Indians of southwestern Oklahoma may drive pick-up trucks and shop at the supermarket but retain their native language. For a theoretical position that equates language with the core of culture, the apparent disintegration of native cultures would not be quite so traumatic for the anthropologist so oriented.

A further source of appeal for cognitive anthropology, and one which I feel is valid, is that it at least raises issues dealing with the upgrading of standards of ethnographic description. Though I believe that

their efforts have been misdirected, I think that the introduction of new methods of analysis borrowed from many diverse areas can only be applauded.

Several further sources of the rise of cognitive anthropology relate to what Colby above has referred to as a "conservative revolution." The emphasis on rigorous studies of particular cultures; the stress on emic studies which attempt to "get inside the skin of the native;" a focus on the study of culture defined in ideational terms; all of these points are venerable and traditional in American anthropology from Boas through Sapir, Kluckhohn, Opler, and Benedict.

In conclusion I would like to answer a major question which I raised in the first chapter: Is cognitive anthropology ultimately detrimental to the growth of viable theory in anthropology? The only answer is a resounding "Yes"! If the ultimate goal of anthropology as a science is to build a theoretical system relating its intermeshed subject matter, the methodology of cognitive anthropology could only stifle the growth of many major areas of anthropological research. Pelto (1970:83) notes:

> Given the 'timeless' nature of ethnoscientific methodology, those anthropologists whose main concerns are the study of acculturation, migration, urbanization, adaptation to national cultures, complex societies, or other aspects of changing cultural patterns are seldom able to make extensive use of the techniques of the New Ethnography in their work. Also, those studies that concentrate on intra-cultural differences and conflicts--such as research in factionalism, decision-making processes, 'marginal subgroups,' etc.--find the ethnoscientific paradigm unsuitable for coping with the heterogenieties and ambiguities of cultural behavior.

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