Cognitive Anthropology

Readings edited by

STEPHEN A. TYLER
Tulane University

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Contents

Introduction

THE OLD AND THE NEW[1]

The history of all scientific disciplines is marked by periods of intense theoretical innovation followed by relatively quiescent periods of consolidation and refinement. 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. Anthropology is currently in one of these periods of innovation. On every hand, the various subdisciplines of anthropology are astir with new formulations challenging and supplementing established concepts and methods. The very lexicon of anthropology reflects this ferment. The journals are full of articles on *formal analysis*, *componential analysis*, *folk taxonomy*, *ethnoscience*, *ethnosemantics*, and *sociolinguistics*, to list but a few. Nearly all of these topics have appeared in the brief span of approximately ten years, with increasing frequency in the last three or four years.

Assessment of such new departures is always difficult. What are their 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?

Enough has been presented in symposia and journals for us to feel that we are witnessing a quiet revolution in anthropology—

quiet because the new departures are firmly rooted in the past. Formal analysis derives in part from the work of such anthropological titans as Radcliffe-Brown, Lévi-Strauss, and Nadel. Folk taxonomies are foreshadowed in the writings of Mauss, Boas, and Evans-Pritchard. That great ethnographer Malinowski would have been no stranger to recent developments in sociolinguistics. The concern for psychological validity is congruent with much of Sapir's work and, to a lesser extent, with some of Kroeber's. And, perhaps, most relevant of all is the work of Bateson.

Yet, these developments constitute more than a disconnected reworking of disparate themes from out of the past. These new formulations contrast sharply with many of the aims, assumptions, goals, and methods of an earlier anthropology. Previous theoretical orientations in anthropology can in a very general way be classed into two types—those concerned primarily with change and development and those concerned with static descriptions. Thus, the evolutionists and the diffusionists concentrated on patterns of change, while the functionalists eschewed this work as mere "speculative history," and focused on the internal organization and comparison of systems, hoping thereby to discover general laws of society. Some culture and personality studies attempted to characterize whole cultures with such concepts as "national character" and "modal personality type," while other culture and personality studies utilized a comparative approach in an attempt to correlate psychological and cultural features.

These formulations were attempts to construct universal organizational types which were linked either by similar processes of change or by similarities of internal structure. In order to achieve this goal, only certain kinds of information were accepted as relevant, and concrete ethnographic data had to be elevated to more abstract forms such as index variables and typological constructs. Consequently, abstract definitions of these features were necessary, and much of the discussion in books and journals concerned the adequacy of these definitions. Once a corporate lineage, for example, had been defined in a particular way, it was only a matter of time before some fieldworker returned to his desk and elatedly reported that his tribe did not conform to the received definition. One way around this problem was to construct more types and subtypes, and broader, more abstract definitions. It was generally accepted that neither the types nor the definitions actually corresponded to anything in the "real world." They were merely convenient methods of ordering the data at hand. Proliferation of types, however, was dangerous, for as the types proliferated, so did the processes linking the types and their constituents. Contrary to expectations, anthropology became more and more particularistic rather than more general and universal.

This concern with typology and definition is an index to another feature characteristic of this period in anthropology. Anthropologists were really

much more concerned with discovering what anthropology was than, for example, what an Eskimo was. In a sense anthropologists were studying only one small culture—the culture of anthropology.

Aside from the diffusionists, these earlier theories can be characterized as attempts to construct monolithic, unitary systems which purported to either explain cultures or their development. Such concepts as cultural core, cultural norm, structure, modal structure, pattern, and others were used to describe these systems. These ideas are symptomatic of a quest for the typical, the normal, the usual, for those definitely bounded phenomena which would systematically differentiate one culture from another. In fact, the very concept of culture is but another of these labels for some arbitrarily bounded unit within which certain types of behavior, norms, artifacts, and emotions are typical (cf. Sapir 1932:515; 1934:593–595). The atypical, especially as expressed in patterns of variation, were either simply dismissed or artifically worked into the scheme as indices of change, diffusion, survival, innovation, dysfunction, abnormality, cultural disintegration, opportunities for the exercise of social control and the like. The only important variations were variations between cultures.

In contrast to these approaches, cognitive anthropology constitutes a new theoretical orientation. It focuses on *discovering* how different peoples organize and use their cultures. This is not so much a search for some generalized unit of behavioral analysis as it is an attempt to understand the *organizing principles underlying* behavior. It is assumed that each people has a unique system for perceiving and organizing material phenomena—things, events, behavior, and emotions (Goodenough 1957). The object of study is not these material phenomena themselves, but the way they are organized in the minds of men. Cultures then are not material phenomena; they are cognitive organizations of material phenomena.[2] 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. Such descriptions may tell us something about the way an anthropologist thinks about a culture, but there is little, if any, reason to believe that they tell us anything of how the people of some culture think about their culture.

In essence, cognitive anthropology seeks to answer two questions: What material phenomena are significant for the people of some culture; and, how do they organize these phenomena? Not only do cultures differ among one another in their organization of material phenomena, they differ as well in the kinds of material phenomena they organize. The people of different cultures may not recognize the same kinds of material phenomena as relevant, even though from an outsider's point of view the same material phenomena may be present in every case. For example, we distinguish

between dew, fog, ice, and snow, but the Koyas of South India do not. They call all of these *manču*. Even though they can perceive the differences among these if asked to do so, these differences are not significant to them. On the other hand, they recognize and name at least seven different kinds of bamboo, six more than I am accustomed to distinguish. Similarly, even though I know that my cousin George is the son of my mother's sister, while my cousin Paul is the son of my mother's brother, this objective difference is irrelevant to my system of classification. They are both "cousins." If I were a Koya, however, this difference would be highly important. I would call my mother's brother's son *baaTo* and my mother's sister's son *annaal*. Even though the same material phenomena are objectively present, they are subjectively perceived and organized differently by Koyas than they are by Americans.[3] Furthermore, there is no apparent over-all integrative pattern which relates the classification of bamboo to the classification of relatives. These are separate classes of phenomena with distinctive and unrelated principles of organization.

Not only may the same phenomena be organized differently from culture to culture, they may also be organized in more than one way in the same culture. There is, then, *intracultural* variation as well as *intercultural* variation. Some intracultural variations may be idiosyncratic, but more important from the anthropologist's point of view are those variations which are used by different classes of people and/or occur in different situations and contexts (cf. Goodenough 1963:257–264). For example, if we are interested in describing the way people classify colors we may discover that there are variant patterns dependent upon the sex or age of our informant as well as his general experience with colors. Thus, females in our culture can generally discriminate and name more colors than males. Or, to take another example, the classification of relatives may be partially dependent on the social statuses of the people talking about relatives, the relationship between them, and the social context in which they are conversing. A Telugu refers to his younger sister as *celli* when talking to another member of his family, but when speaking to a person outside his family group, he uses the term *cellelu*, which may mean younger sister, or mother's sister's daughter, or father's brother's daughter.

A consequence of this interest in variation is the idea that cultures are not unitary phenomena, that is, they cannot be described by only one set of organizing principles. For each class of relevant phenomena there may be several alternative organizations. The realization or choice of one alternative to the exclusion of some other is dependent upon a variety of factors. For example, some people have more or less knowledge of some phenomena, or certain alternatives may be acceptable only in particular contexts (cf. Hymes 1964b:41). If these variants are used only in certain identified situations, or if there is a hierarchy of choice so that variants are ordered on

the basis of their relative desirability, we can say that they are in complementary distribution and do not conflict with one another. In such a situation it is possible for a large number of variants to coexist. But, if these variants conflict in their organization and the situations in which they occur, there must be some means of harmonizing the contrast. This can be achieved by some change in the principles of organization or in the situations in which they occur. For example, among the Koyas, the pig is classed as an edible animal, but among neighboring Muslims the pig is classed as inedible and defiling. Suppose a Koya woman were married to a Muslim man. While in her husband's home she could not act on her classification of the pig as an edible by eating pork; while visiting her parents in the absence of her husband she could. So long as the two systems of classification can be realized in these isolated contexts there is no necessary conflict between them, and both may persist. If these contexts were not in complementary distribution, some rearrangement of the two contrasting systems of classification would have to take place if the marriage were to persist.[4]

In fact, this is an argument for a different kind of unitary description which sees unity as emerging from the ordered relations between variants and contexts. Variants are not mere deviations from some assumed basic organization; with their rules of occurrence *they are the organization*. (Wallace 1961:29–41; Hymes 1964a:386–387). It must be emphasized, however, that such a unitary description can be achieved only by the anthropologist. It is highly unlikely that the members of a culture ever see their culture as *this kind of* unitary phenomenon. Each individual member may have a unique, unitary model of his culture, but is not necessarily cognizant of all the unique, unitary models held by other members of his culture. He will be aware of and use some, but it is only the anthropologist who completely transcends these particular models and constructs a single, unitary model. This cognitive organization exists solely in the mind of the anthropologist (cf. Bateson 1958:294). Yet, to the extent that it will generate conceptual models used by the people of a particular culture, it is a model of their cognitive systems.[5]

The "theory" here is not so much a THEORY OF CULTURE as it is *theories of cultures*, or a theory of descriptions. The aim of such a theory is to provide answers to 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? Answers to these questions are provided by an adequate description of the rules used by the people in that culture. Consequently, 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.

ORDER OUT OF CHAOS

In a sense, cognitive anthropology is not a new departure. Many anthropologists have expressed an interest in how the natives see their world. Yet, there is a difference of focus between the old and the new. Where earlier anthropologists sought categories of description in their native language, cognitive anthropologists seek categories of description in the language of their natives.[6] Ultimately, this is the old problem of what do we describe and how do we describe it? Obviously, 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 of the environment. Naming is seen as one of the chief methods for imposing order on perception.[7]

In a very real sense, the anthropologist's problem is to discover how other people create order out of what appears to him to be utter chaos. Imagine, for a moment, a being from another planet equipped with all our sensory apparatus who perceives for the first time the infinite variety of sight and sound in which we live. Suppose further that he is attempting to describe this world in a scientific report for his colleagues at home. At first, everything would be chaotic. Each sound and object would seem to be unlike any other. His experience would be similar to what we feel the first time we hear a language we have never heard before. But, with infinite time and patience, let us assume that he is able to describe everything he perceives—that is, the total environment of earth. Probably he would eventually be able to organize his report around concepts acceptable to his world or devise new ones as he saw fit. Yet, would anyone of us accept his report as an accurate account of the world as we see and live in it? If he in fact describes everything, we would not. Nor would we accept his organization of the things he perceived, for they would almost certainly not fit our own system of organization. Unlike this mythical creature, we do not live in a world in which we discriminate among all the possible sensory stimuli in our environment, nor do we react to each stimulus as if it were totally new and foreign. In effect, we choose to ignore many of those perceptual differences which make each object unique. In large part, we do this by naming. By naming we classify and put objects which to us are similar into the same category, even though we can perceive differences among them (cf. Boas 1938:208–214). For example, the chair in which I sit has a nick in the left leg, yet I class it as a "straight chair" no different from others like it in the room.

We classify because life in a world where nothing was the same would be intolerable. It is through naming and classification that the whole rich world of infinite variability shrinks to manipulable size and becomes bearable. Our methods of classification are entirely arbitrary and subjective. There is nothing in the external world which demands that certain things go together and others do not. It is our perception of similarities and differences together with a set of hierarchical cues that determine which things go together. We not only react to certain discriminable stimuli as if they were the same, we name them and organize them into groupings. Thus, for example, there are objects with a seat, a back and four legs which we label *chairs*, even though no two of these objects are exactly alike. The word *chair* then stands as a sign for a whole class of objects with a seat, a back, and four legs. This sign, too, is arbitrary—we might as well call these objects *argoboos*. Just as there is no inherent quality in an object that forces us to perceive it in exactly one way, neither is there an intrinsic characteristic associating an object with its name. Consequently, with the passage of time, a class of objects may be renamed; but the class of objects denoted by this name does not change, or, conversely, the class of objects denoted by a name may change, but the name does not.

Thus, 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 hierarchically arranged by a process of inclusion, they form a *taxonomy*. To continue the example of chairs, there are other objects in our homes which are not chairs. There are sofas, tables, desks, cabinets, and the like. Each of these constitutes a separate class, some with many subclasses. For example there are end tables, dining tables, and coffee tables, but each of these is also a member of some more inclusive class—the class of things called "furniture." A portion of this taxonomy is shown in Fig. 1.

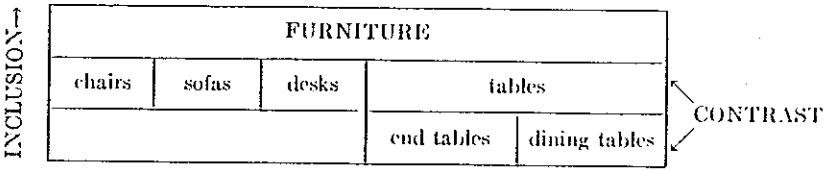

Figure 1. Taxonomy of furniture.

Figure 1 illustrates 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. Thus, end tables are kinds of tables as tables are kinds of furniture; end tables are not the same as dining

tables just as tables are not the same as chairs. These relationships could also be represented in a branching diagram as in Fig. 2.

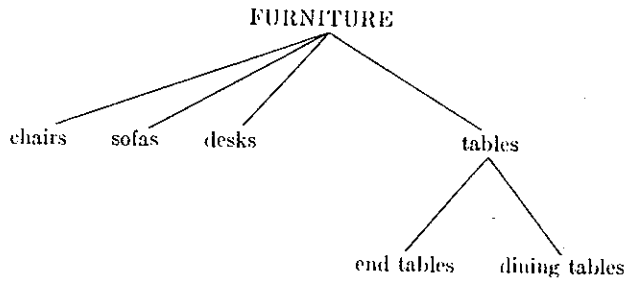

Figure 2. Branching diagram.

This particular taxonomy constitutes one *semantic domain* in our culture. A sematic 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*.

Note that Fig. 2 tells us nothing of the things which distinguish a chair from a table. It tells us only that they are different. Suppose you had to tell someone how you know that one object is a chair and the other is a table. In the process of doing this, you might describe certain underlying features, some of which both chairs and tables share and some of which they do not. For example, you might say a chair has four legs, a seat, and a back, but a table has four legs and a top. Chairs would thus differ from tables by the presence of two features—a seat and a back, and the absence of one feature—a top. These underlying features are *components* or *features* of meaning. They are some of the dimensions of meaning underlying the general domain of *furniture*. That these are not the only dimensions is apparent in the contrasts between desk and table. Both pieces of furniture have four legs and a top. Using only the two features you have isolated, it is not possible to say how a table differs from a desk. Should you wish to show how each of these items differs from the other you would have to discover other features of meaning.

Semantic features, like labels, are also organized. A part of the taxonomy of "animals" in American English consists of the following lexemes: cow, bull, heifer, calf, steer, mare, stallion, filly, foal, colt, gelding, sow, boar, gilt, barrow, shoat, piglet, ewe, ram, wether, lamb, livestock, cattle, swine, horse, sheep. This taxonomy is arranged in Table 1.

On even casual examination the items occurring in the lowest level of Table 1 seem to be related in some way. Closer inspection reveals that

Table 1. Taxonomy of "Livestock"*

ANIMAL			
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

* cf. Lamb 1964:68.

similar distinctions are made under each major category of livestock. The contrast between cow and bull, for example, is the same as the contrast between boar and sow; ram and ewe; stallion and mare. We can readily identify this contrast as one of sex or gender, male versus female. Similarly, there is an identical contrast between bull and steer; ram and wether; stallion and gelding; boar and barrow. Again, we would identify this as a contrast between male animals versus neutered animals. In addition to this sex contrast there is a further contrast between mature and immature animals. A calf is an immature cow or bull and a heifer is an "adolescent" cow. All the lexemes in the lowest level of Table 1 reflect the two semantic features of sex and maturity. Each of these has three values: sex (male, female, neuter); maturity (adult, adolescent, child). Note, however, that horse and pig have an additional feature of maturity denoting "newborn" or "baby" (piglet and foal).

Using symbols: ♂—male; ♀—female; ∅—neuter; M^{-1}—adult; M^{-2}—adolescent; M^{-3}—child; M^{-4}—baby; H—horse; P—swine; C—cattle; S—sheep; the distribution of features for each label can be stated in formulae as follows:

stallion	H ♂ M^{-1}	boar	P ♂ M^{-1}
mare	H ♀ M^{-1}	sow	P ♀ M^{-1}
gelding	H ∅ $M^{-1}M^{-2}$	barrow	P ∅ $M^{-1}M^{-2}$
filly	H ♀ M^{-2}	gilt	P ♀ M^{-2}
colt	H ♂ ♀ M^{-3}	shoat	P ♂ ♀ M^{-3}
foal	H ♂ ♀ M^{-4}	piglet	P ♂ ♀ M^{-4}

The first formula reads: a stallion is a horse, male, adult, or more appropriately, a stallion is an adult male horse. Such formulae are simply expressions of the distribution of features for each separate label. A box figure shows how these features distribute across the whole set of labels.

Reading from the diagram, a stallion is an adult male horse and a mare is an adult female horse. The features "adult" and "male" *intersect* at the space containing the label "stallion," while the features "adult" and "female" intersect at the space containing the label "mare." Since this diagram has two major features (maturity and sex) which cut across (intersect) one another, it is a *paradigm*. Features are paradigmatically arranged when they are: (1) multiple; (2) intersect.[8]

Paradigms and taxonomies are different kinds of semantic arrangements. In contrast to a paradigm, a taxonomy orders its labels by contrast and inclusion. A taxonomy typically asserts that items in lower levels are *kinds of* items in higher levels. A horse, for example, is a *kind of* livestock. A paradigm makes no such assertion. In Fig. 3 for example, a shoat is not necessarily a kind of boar.

		SEX		
		male ♂	female ♀	neuter ø
MATURITY	adult M-1	stallion boar	mare sow	gelding barrow
	adolescent M-2		filly gilt	
	child M-3	colt shoat		
	baby M-4	foal piglet		

Figure 3. Paradigm of features for "horse" and "swine."

For cattle and sheep the contrast between baby and child would be omitted. Sheep also omits the adolescent distinction. There is however an archaic form for newborn sheep *viz.* "Yeanling."

In addition to taxonomies and paradigms semantic features may be arranged on a branching diagram called a *tree*. Features in a tree are ordered by sequential contrast of only one feature at a time. Trees are thus based on successive choices between only two alternatives. Such a semantic arrangement is most frequently encountered in zoological or botanical texts. Figure 4 is a simplified example of a tree.

A reading of Fig. 4 would be: Are the flowers spurred? If yes, are the flowers regular? If they are regular, then this is a delphinium. Unlike a paradigm, the features of a tree do not intersect, and unlike a taxonomy items at lower levels are not included in higher levels. Consequently, paradigms, taxonomies and trees are fundamentally different kinds of semantic arrangements. Each semantic domain of a culture may be ordered by one or more of these arrangements.

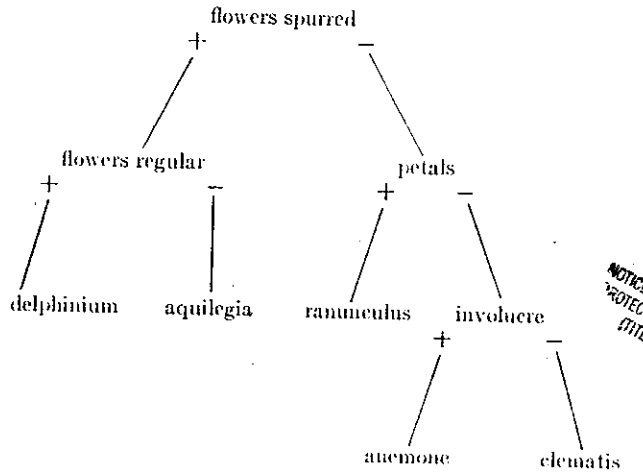

Figure 4. A tree arrangement. (Adapted from Porter 1967:83)

Plus (+) indicates presence of the feature, minus (−) its absence. Thus, if a flower is not spurred, has no petals, and no involucre, it is a clematis.

A culture consists of many semantic domains organized around numerous features of meaning, and no two cultures share the same set of semantic domains or features of meaning, nor do they share the same methods of organizing these features. The problem for the anthropologist is to discover these semantic domains and their features, for an anthropologist in the field is much like our interplanetary visitor. There is no familiar order to the way these strange people organize their world. But, unlike our visitor, the anthropologist must avoid imposing his own semantic categories on what he perceives. He must attempt to discover the semantic world in which these people live. There are, then, two ways of bringing order out of apparent chaos—*impose* a preexisting order on it, or *discover* the order underlying it.[9] Nearly all of earlier anthropology was characterized by the first method. By contrast, cognitive anthropology seeks to develop methods which can be used for discovering and describing these principles of organization.

Since such semantic systems are implicit in our use of language, they constitute one of the most significant features of human communication. Yet, what can be communicated and how it is communicated is not solely determined by this kind of semantic feature. Other semantic features deriving from the context of communication are equally important. Context includes the manner of communication (for example, verbal and written), the social setting, and the linguistic repertories of speaker and hearer.

Contextual semantic features and their mutual interdependence are as much a part of the cognitive system as taxonomies and semantic domains.

There still remains the question of how we discover features in cultures other than our own. If you will attempt to complete the statement of semantic features for the taxonomy of furniture, you will see that the discovery of these features is difficult enough in your native language. It is even more difficult in a strange language. As a consequence, new fieldwork techniques and methods have had to be devised. Most important among these are techniques of *controlled eliciting* and methods of *formal analysis*.

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. Suppose you are a foreigner attempting to learn something about American culture. On seeing an object for which you do not know an English term, one possible sequence of related questions and responses might be:

Q. What is this?
A. This is a sow.
Q. Is that a sow, too?
A. No, that's a boar.
Q. Is a boar a kind of sow?
A. No, a boar is a kind of livestock.
Q. Is a sow a kind of livestock?
A. Yes.
Q. How many kinds of livestock are there?
A. There are pigs, horses, mules, sheep, goats, and others.

This sequence indicates that sows and boars are conceptually linked and that there are numerous other things grouped with them in the taxonomy of livestock. Note that decisions concerning the inclusion of items within this taxonomy are made by the informant, not by the investigator. Contrast this procedure with a familiar questionnaire technique derived from handbooks on social science methodology. Is the cow __very like; __somewhat like; __only a little like; __not at all like a god (check one). Aside from the spurious scaling, this question would be meaningful only in societies of English speakers in which there were: (a) cows, (b) gods, (c) some evidence that gods and cows were conceptually linked, or (d) sociologists. In this technique, the investigator has already made all the decisions about conceptual relevance. The informant's responses can only be replications in one way or another of the investigator's judgments about conceptual relevance. In a sense, such a method merely tells you what you already know. Controlled eliciting, on the other hand, 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 (Collingwood 1929:29–43). 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.

Formal analysis is simply one method of stating the results of such controlled eliciting. It differs from other methods in its emphasis on internal consistency, completeness, and form. A particular set of data relating to some semantic domain must be explained by the relationship between units comprising that domain--not by determinants outside it. The problem of external determinants is delayed until internal determinants are analyzed. For example, the question of whether I call my mother's sister's son "cousin" because he is outside my nuclear family cannot be determined until I know the system of relations between cousin, brother, and all the other kin terms in the English system. A formal analysis is complete when the relations among all the units comprising a semantic domain are described.

THE NEW ORDER

The aims and methods of cognitive anthropology have important implications for cultural anthropology. They entail a rethinking of the culture concept, the comparative method, and of ethnography.

In this discussion culture has been identified with cognition. This must strike some cultural anthropologists as a truncated version of the culture concept, for it neglects many of their traditional interests. They might well ask, What about process? What about behavior? What about motivation? Implicit in these questions is an assumption that in addition to cognitive systems a theory of culture must explain cultures as systems emerging from patterned frequencies of observed behavior and processes of development and change (cf. Goodenough 1964). *As a general statement of anthropological goals, these are relevant considerations, but they are not relevant to a theory of culture.* There is no necessity to assume that the cognitive order is either systematically a derivative of or a predictor of substantive actions. Just as the grammar of a language provides no information on what an individual speaker will say on any given occasion, so too a cognitive description of a culture does not pretend to predict the actual behavior of any individual. The formal analysis of culture, like a grammar, is concerned only with what is expected and appropriate. And just as an adequate grammar is neither contingent upon prior assumptions concerning developmental processes nor necessarily explains them, a grammar of culture need make no assumptions about nor attempt to explain these processes. So construed, neither prediction of actual events nor specification of developmental process is a necessary

component of a theory of culture.[10] To paraphrase Collingwood (1956:217), cultural anthropology is not a description of events or an account of change. The cultural anthropologist is only concerned with those events which are expressions of underlying thoughts. His aim is to penetrate beyond mere material representation to the logical nexus of underlying concepts.

Culture, conceived as the totality of human behavior, ideas, history, institutions and artifacts has never been particularly useful as a meaningful method of explaining ethnographic facts. Such a conception merely asserts that culture is equivalent to the whole of human knowledge. As a device which purports to explain all of man's learned behavior, motivations, prehistoric record, ecological adaptations, biological limitations, and evolution it attempts too much. 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 cultures. 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. Such a general theory will be equivalent to the language in which we describe cultures (Kay 1965:112). In effect we already have a pseudometalanguage. It is for this reason that nearly all ethnographies have similar chapter headings. The problem with this metalanguage is that it assumes universality without prior demonstration. Its universality inheres in the language of description and not necessarily in the object being described.[11]

At issue here are two contrasting views of cultural anthropology. The central issue is, Is cultural anthropology a *natural* or a *formal* science? 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.[12] Each particular culture consists of a set of logical principles which order relevant material phenomena. To the cognitive anthropologist these logical principles rather than the material phenomena are the object of investigation. For the cognitive anthropologist cultural anthropology is a formal science. It seems likely that the logical operations underlying principles of ordering are finite and universal, but capable of generating an infinite number of possible specific orderings (cf. Lévi-Strauss 1966:268). In this limited sense, cognitive anthropology constitutes a return to Bastian's search for the "psychic unity of mankind."

The implications for the comparative method follow directly from the above. The central issue in comparative analysis is, What is the unit of comparison? There have been many attempts to specifically delimit the unit of comparison. Yet most so-called cross-cultural comparisons have really been nothing more than cross-tribal or cross-community comparisons. Obviously, if a culture is the unit of comparison, then we must compare whole systems which are bounded in space and time or demonstrate that the parts of systems we are comparing are justifiably isolable (cf. Boas 1940:275). Since most ethnographies are not sufficiently complete for either of these possibilities, the whole comparative approach based on substantive variables must be abandoned if our aim is indeed cultural comparison. Those who insist that no fact has meaning except by comparison are right, but the implication that comparison can occur only between similar facts from different systems does not follow. It is much more pertinent to compare similar, but not identical facts within the same system. This is not so much a total abandonment of the comparative method; it is a matter of priorities. 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.

PROBLEMS AND PROSPECTS

A major indication of vitality in any new orientation is its capacity to generate new problems, and new ways of looking at old problems. Although cognitive anthropologists have made great strides in the study of semantic organization and formal analysis there still remains a vast and only partially explored territory. How current procedures and assumptions will be modified in the attempt to explore these areas is an open question. Clearly, methods and procedures are not adequate for all of the problems that will be encountered. A few of these problems and some of the potential means of looking at them are outlined in the following section.

Arrangements

Taxonomies, paradigms, and trees do not exhaust the domain of semantically significant cognitive orderings. In fact, they represent only a small portion of cognitive processes, and probably occur only when the number of properties or the relations among properties are few. In more complex domains properties are only partially ordered. The structure of such domains is characterized by discontinuous and partial combinations of a large number of features. These are probably best represented as discontinuous arrays of features in a matrix, each row uniquely defining a single lexeme. Although some of the features of such a domain may be dichotomously opposed, intersect paradigmatically, or be related by inclusion, it is not possible to order the entire domain by any one of these principles of ordering. It seems

likely that most semantic domains will display this kind of partial ordering and will not strictly conform to any one of the three preceding kinds of arrangement. If this is true, such domains should probably not be analyzed in terms of features and feature organization. The multiplicity of relations and properties probably signifies some other type of ordering unrelated to the isolation and organization of features.

Perception and Conception

The problem of partially ordered domains discussed above is partially a reflex of a related problem—the problem of the relation between perception of attributes or features ("identification") and conceptual knowledge. So far anthropologists seem to have assumed a direct connection between attribute perception and conceptual ordering. In many semantic domains this assumption is probably unjustified. Some semantic domains (for example, the classification of deities) may lack obvious perceptual attributes. Other semantic domains may be conceptually ordered without reference to perceptual attributes. Even though it may be possible to isolate perceptual attributes in such cases they are not semantically relevant. It is also possible that the organization of attributes and the organization of concepts ultimately refer to different semantic domains. Where this is the case both conceptual and perceptual orderings may merely be surface structures deriving from even more complex underlying forms.

Discourse Analysis

In part the difficulties inherent in the perception-conception problem arise from the cognitive anthropologist's enthrallment with the lexeme as a basic unit of analysis. As yet, few anthropologists have attempted to investigate either larger linguistic units or nonlinguistic units. What semantic information, for example, is transmitted by the occurrence of a lexeme in sequences of discourse larger than a sentence (cf. Harris 1952)? In connected discourse speakers and authors deliberately manipulate semantic features in order to convey nuances of meaning often quite opposed to the overt content of individual lexemes. Essentially, discourse analysis is one more aspect of the problem of context.

Propositional Analysis

When lexemes occur in a sentence, it is obvious that some aspect of meaning is conveyed by the sentence as a whole. The meaning of the sentence is not simply the sum of the meanings of its constituent lexemes. One convenient approach, which has a long-standing usage, is to assume that the sentence is an assertion about the relationship among the semantic components of its constituent lexemes. The sentence "germs cause colds" asserts a relation between germs and colds—in this case a causal relation. It

can also be seen as a statement about disease *beliefs*. Underlying this sentence is the prior semantic information to the effect that there are things called colds and there are things called germs. Such sentences can be succinctly stated in logical form.

(1) $(\exists x)(\exists y)(xCy)$

Where x stands for germs, y for colds and C for the relation of causality, this formula reads, There is an x (germs) and there is a y (colds) such that x causes y. In this same domain of analysis, the sentence "germs cause disease" also occurs, symbolized as follows (z stands for disease):

(2) $(\exists x)(\exists z)(xCz)$

Except for the substitution of z this formula is identical with (1). Note however that there is a special relation between z and y. A cold is a kind of disease. Using the notation for set inclusion ($\in$) this taxonomic relation can be symbolized as:

(3) $(\exists y)(\exists z)(y \in z)$

This formula asserts that there are colds (y) and there are diseases (z) and that colds are a kind of (are included in the set of) disease. Since (3) is equivalent to a statement of taxonomic categorization it is apparent that all taxonomies are derived from propositions of this type.[13] A more interesting feature is that because y is included in z we might infer without prior knowledge:

(4) $(\exists x)(\exists y)(\exists z)(xCz) \ \& \ (y \in z) \rightarrow (xCy)$

That is, if there are germs, diseases and colds, and germs cause diseases, and colds are a kind of disease, then germs also cause colds. If we did not already know that germs cause colds, on the basis of this inference we would be prompted to ask our informant if this were the case. As a matter for further investigation we would want to discover whether or not the relation of germ causation holds for all lexemes included in the category of diseases. In other words, we are interested in the domain of the relation C. It would also be interesting to know if C holds for all lexemes included in the category of "illness." Since mental illness is a kind of illness (is included in the set of illnesses) and we would not normally assert that germs cause mental illness, one of the chief semantic contrasts between illness and disease is the belief that germs cause disease but not all illnesses. Put another way, the domain of C does not include cases in which the ordered couples are germs and illness.[14]

An important point is implicit in the preceeding illustration. Since germs are not an attribute of disease we would probably not have arrived at this statement of contrast between illness and disease if we had remained

at the level of identifying those perceptual attributes which differentiate diseases from illnesses or one kind of disease from another. Perceptual attributes are irrelevant to beliefs (conceptual knowledge) about diseases—at least in this example (cf. D'Andrade and others 1966). Recognition of propositional analysis and its importance for understanding cognitive orderings is recent, but it seems certain that it will play an important role in the development of cognitive anthropology (cf. Kay 1965).

Metamessages

A much more difficult area of research is the analysis of what I shall here refer to as "metamessages." In a common sense way, metamessages are what we are talking about when someone says, How are you? and we say to ourselves, Now I wonder what he *really* meant by that? Metamessages communicate semantic information which does not seem to be at all related to the overt content of an utterance. Under these circumstances an apparently simple sentence like "it's cold in here" could mean "bring me my coat," "turn up the heat," or "you've had enough to drink and its time to go home." The pioneering work of Bateson (1956) remains the most important contribution to this kind of analysis.[15] As a speculative aside (deriving from Bateson's work) it is possible that all such metamessages ultimately derive from a restricted set of propositions in the imperative mood expressing dominance, dependency, aggression, and submission (Love me! Hate me! Don't hurt me! Be nice!). If this is true, then it is in this area that studies of animal communication can make a significant contribution to cognitive anthropology.

Historical Linguistics

Application of formal semantic analysis to problems of linguistic reconstruction should yield significant results. Historical linguistics has made great progress in the reconstruction of phonology and grammar, but its attempts to reconstruct meanings are generally unimpressive. Too often semantic reconstructions are either based on the notion that the most frequently occurring glosses represent basic meaning or that certain glosses are derivative extensions of other glosses. The fallacy in much of this stems from a failure to recognize the arbitrary nature of the relation between linguistic signs and their denotata. Attempts to reconstruct the meanings of words are symptomatic of a preoccupation with the lexeme rather than the organization of semantic domains. Recent work in the semantic reconstruction of kinship terminologies indicates that semantic domains can be reconstructed without reference to their constituent lexemes (cf. Voorhees 1960; Tyler 1965). The basic procedure consists of a comparison and reconstruction of semantic categories. The structure of the rules which transform one genetically related system into another constitutes a description of historical process. An interesting feature emerging from these studies is that the semantic structure of such systems display a remarkably conservative nature. The parameters of the system are relatively impervious to change despite the fact that individual lexemes denoting semantic categories frequently undergo rapid and dramatic change. Changes occur in the variables of the system, not in its parameters (cf. Bateson 1958:292). Preliminary reconstructions of Dravidian, Athapascan, Yuman, and Indo-European kinship systems all agree in manifesting this trait. Such evidence, preliminary as it may be, is certainly sufficient to challenge many of our assumptions about change in kinship as a result of changes in other features of social organization. Whether such persistence is characteristic of other semantic domains remains to be seen, but it seems apparent that structural semantics can make an important contribution to linguistic reconstruction. And, if the structure of transform rules linking the semantic domains of separate languages are in fact a description of historical process, this should be of enormous significance to lexicostatistics and glottochronology.

Semantic Ontogenesis

Finally, we need research on the development of cognitive categories. How does the child acquire semantic features? What is the significance of rote versus rule learning in semantic analysis? What is the relation between the derivation of semantic features and their order of acquisition in the learning process? A common assumption in philosophy (cf. Quine 1960:80-124) and psychology (cf. Church 1963:68-78) is that the child first acquires concepts for concrete objects and gradually expands its semantic domain by extension and generalization to include more abstract conceptions. There is little real evidence to support this developmental sequence and despite its obvious appeal to intuitive notions, it seems suspect. Given the primacy of spatiotemporal orientation over language acquisition, an equally good a priori argument could be made for the prior learning of relational concepts. It is quite likely that the ontogenesis of semantic categories will bear little significance in the analysis of semantic domains among adult speakers. Since different individuals probably arrive at similar semantic structures by widely variant ontogenetic pathways the relation between ontogenesis and semantic structure will probably remain indeterminant. Disparity of ontogenetic sequences may, however, be related to the occurrence of multiple formal solutions in some indirect fashion.

These comments are intended as speculations on the possible areas of future development in cognitive anthropology. They do not pretend to be exhaustive nor even representative. Nonetheless, they do indicate that cognitive anthropology has moved into a secondary stage of development. We have a few tentative answers, some new questions, and a host of old questions still unanswered. Fresh ground has been broken and new areas

occupied, but still more remote territories have opened up for further research.

Explicit here is a view of culture derived from a kind of ethnography in which the methods of description are public and replicable, and the results predictive of expectations of appropriate behavior. Implicit is the cognitive reorganization of our categories of description and analysis. Cognitive anthropology entails an ethnographic technique which describes cultures from the inside out rather than from the outside in. Categories of description are initially derived from relevant features in a culture rather than from the lexicon of anthropology.

Cognitive reorganization is a familiar process in the history of anthropology—in fact of any scientific discipline. The history of science is but the record of constant reexamination of assumptions, methods, and data. Such new developments in science do not take place in a vacuum. Innovations in one branch of science are complemented by convergent developments in other branches.

The psychologist's renewed interest in cognition, the linguist's rediscovery of semantics, the biologist's recent emphasis on taxonomy and species specific behavior, and the sociologist's concern with the presentation of the self all reflect a set of recent developments complementary to one another and to those in anthropology. To be sure, among these disciplines there are differences in emphasis and method, yet each shares with the other a common orientation—the discovery of the organizing principles used by individuals, cultures, and species in manipulating and adapting to their particular life-space.

NOTES

[1] I wish to thank the following people who commented on previous versions of this paper: Brent Berlin, Mary Black, Charles Frake, John Gumperz, Dell Hymes, Paul Kay, Floyd Lounsbury, Ronald Rohner, George and Louise Spindler. I hope it is evident that the views in this chapter are those of the editor and do not necessarily represent a consensus of opinion among the above nor among those whose papers comprise subsequent chapters.

[2] In this, and in much of what follows, there is a pronounced neo-Kantian flavor.

[3] For an instance of a similar distinction between "objective environment" and "perceived life-space," see von Uexküll (1957).

[4] The processes involved in this example are related to theories of cognitive dissonance (cf. Festinger 1957).

[5] The line of argument here derives mainly from Russell (1929:92-98), but see also Bateson (1958:294) and Sapir (1932:515–519).

[6] The most notable exception to this statement is Boas.

[7] It is probably not true that all named things are significant, just as it is not the case that all significant things are named. Yet, as a point of departure, named categories are of primary importance.

[8] Note the two empty spaces in Fig. 3. These indicate that this is not a perfect paradigm. The empty spaces are the result of incomplete combination of semantic components. The combinations ♂ M^{-2}; ∅ M^{-3}; ∅ M^{-4} do not occur. In a perfect paradigm all possible combinations would be realized. Perfect paradigms occur less frequently than imperfect paradigms. It should also be noted that in some contexts the lexemes "boar" and "stallion" denote not only ♂ M^{-1} but ♂ $M^{-1}M^{-2}$. For "horse" the lexeme "yearling" may sometimes denote ♂ M^{-2}; ♀ M^{-2}; ∅ M^{-2}. Historically it is interesting to note that all the lexemes denoting "newborn" (♂ ♀ M^{-4}) except piglet are derived from verbs denoting "to give birth to." A cow "calves," a mare "foals," a ewe "lambs," or "yeans," but a sow "farrows." As might be expected the archaic term for a newborn pig is "farrow." The lexeme piglet is recent. Also relevant is the fact that the "wild animal" category denoted by the lexeme "deer" has, except for the neuter category, the same semantic features as the category denoted by cattle. Formerly deer denoted "animal." Finally, the features male, female, neuter correspond to the generalized Indo-European classification of nouns as masculine, feminine, neuter.

[9] This distinction between a priori and a posteriori models is difficult to maintain, for it impinges directly on the philosophical problem of "other minds." If the mind imposes its own order on the disorderly happenings of the universe, then the investigator of necessity imposes his own logical constructs on the world he believes he is exploring. From this point of view there is no evidence for a belief in the existence of other minds except by analogy. Yet, granted the existence of other minds it is possible to assume that since the logical constructs of an informant and of an investigator are both products of a mind, these constructs are knowable insofar as they are communicable. Hence, the cognitive anthropologist's emphasis on language as both a method of discovery and an object of investigation (cf. Russell 1929:99–103).

[10] This is not to imply that such a theory is incompatible with the study of change and development. The point is that a theory of description constitutes a different order of theory than that required for processes of change (for a discussion of this point, see Bateson 1958:296-300). A theory of change emphasizing cognitive organization would probably demonstrate that most anthropological data on change relate not to cultural change, but merely to epiphenomenal fluctuations.

[11] This is misleading. Scientific laws are of necessity statements of universals in the language of description. All talk of "objects" and an hypothesized relation between "objects" and the language of description is symptomatic of a pernicious kind of dualism. The point is that our current language of description is inadequate either for the description of particular cultures or the development of universals simply because its assumptions are implicit and its operations (when specified) are contradictory. The continuing argument in descent "theory" is a classic example of the inadequacy of our current "metalanguage." The misleading statement reflects my own vacillation between an "intuitionist" (conceptualist) and "logicist" (realist) point of view (cf. Quine 1952). In general, this chapter is intuitionist with occasional logicist lapses. The lapses create problems like the one referred to above.

My assertion that the description of a culture is really a description of the anthropologist's cognitive ordering is pure intuitionism which does not square directly with the Lévi-Straussian quest for a universal pan-human logic expressed in other sections. In a sense, the psychological reality problem is a confrontation between intuitionism and realism or perhaps formalism (nominalism). My belief in the relevance of relevance as an aspect of cognitive anthropology is probably creeping realism.

[12] Leach (1961:6–21) makes a similar point, but with different emphasis. For two discussions of the distinction between formal and factual or natural sciences, see Carnap (1953) and Quine (1960:270–276). The distinction may be somewhat overdrawn, but this should not obscure the fact that cultural anthropology has traditionally emulated a model of scientific method derived from a rather naive nineteenth century scientific materialism. Harris's (1962) quest for elementary units of cultural "matter" is a recent example of this attitude.

[13] For more complete formalization of taxonomies, see Gregg (1954), and Woodger (1952).

[14] These assertions are illustrative. Whether they hold true for disease and illness is a matter for research. The main point is that the domain of a relation may be of greater importance in establishing semantic contrast than the distribution of features.

[15] For additional research of this kind, see the bibliography under "expressive language" in Hymes (1964a).

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