**Studies in Ethnoscience**

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# PART THREE
# ANTHROPOLOGICAL APPROACHES

## *Studies in Ethnoscience*[1]

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THIS paper is a survey and explication of a new approach in ethnography—of what one might well call "the New Ethnography" were it not for that label's pejorative implications for practitioners of other kinds of ethnography. The method has no generally accepted name, although one is clearly required. "Ethnoscience" perhaps has the widest acceptance, in conversation if not in print, and has the advantage of freshness. However, some of this word's undesirable implications should be disavowed: "The term 'ethnoscience' is unfortunate for two reasons—first, because it suggests that other kinds of ethnography are *not* science, and second because it suggests that folk classifications and folk taxonomies *are* science" (Spaulding 1963). Although the name may have been chosen partly because of the first of these implications, it would be impolitic if not impolite to insist on it; in any case, the method should stand or fall on its own merits. To dispose adequately of the second implication would require a discourse on the definition and philosophy of science. It is perhaps sufficient to remark that the most appropriate meaning to assign to the element "science" here (but not necessarily elsewhere) is, essentially, "classification." This restricted implication has been well expressed by G. G. Simpson in a somewhat similar context:

> The necessity for aggregating things (or what is operationally equivalent, the sensations received from them) into classes is a completely general characteristic of living things. . . . Such generalization, such classification in that sense, is an absolute, minimal requirement of adaptation, which in turn is an absolute and minimal requirement of being or staying alive. . . . We certainly order our perceptions of the external world more fully, more constantly, and more consciously than do any other organisms. . . . Such ordering is most conspicuous in the two most exclusively human and in some sense highest of all our activities: the arts and sciences. . . . The whole aim of theoretical science is to carry to the highest possible and conscious degree the perceptual reduction of chaos . . . . the most basic postulate of science is that nature itself is orderly. . . . All theoretical science is ordering (Simpson 1961:3–5).

"Ethnoscience" is appropriate as a label because it may be taken to imply one interpretation of such terms as "ethnobotany," "ethnogeography," etc.—although it is important to emphasize that the approach is a general ethnographic one, by no means limited to such branches of ethnography as are often called by the names of recognized academic "arts and sciences" coupled with the prefix "ethno-." This prefix is to be understood here in a special sense: it refers to the system of knowledge and cognition typical of a given culture.

Ethnoscience differs from Simpson's "theoretical science" in that it refers to the "reduction of chaos" achieved by a particular culture, rather than to the "highest possible and conscious degree" to which such chaos may be reduced. To put it another way, 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. Thus, to take an extreme example, the "ethnopornography" of the Queensland aborigines is what *they* consider pornography—if indeed they have such a category—rather than what was considered pornography by the Victorian ethnologist who titled the last chapter of his monograph on Queensland aboriginal culture "ethno-pornography," warned that "the following chapter is not suitable for perusal by the general reader," and described under this heading such topics as marriage, pregnancy and childbirth, menstruation, "foul language," and especially genital mutilations and their social and ceremonial significance (Roth 1897:169–84). Similarly, "ethnohistory" is here the conception of the past shared by the bearers of a particular culture, rather than (the more usual sense) the history (in our terms) of "ethnic groups"; "ethnobotany" is a specific cultural conception of the plant world, rather than (again the more usual sense) a description of plant uses arranged under the binomials of our own taxonomic botany.

It is not a new proposal that an important aspect of culture is made up of the principles by which a people classify their universe. A rather clear statement to this effect was made by Boas (1911:24–26); the notion was hinted at by Durkheim and Mauss (1903:5–6); Malinowski clearly stated that "the final goal, of which an Ethnographer should never lose sight. . . . is, briefly, to grasp the native's point of view, his relation to life, to realise *his* vision of *his* world" (1922:25). Even E. B. Taylor can be understood in the same sense, when he warned that the ethnologist "must avoid that error which the proverb calls measuring other people's corn by one's own bushel" (1881:410). However, the explicit definition of culture as a whole in these terms, and the proposition that ethnography should be conceived of as the discovery of the "conceptual models" with which a society operates, was first stated quite recently in an elegant, brief paper by Goodenough:

> 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 for perceiving, relating, and otherwise interpreting them. . . . Ethnographic description, then, 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 (Goodenough 1957:167–68.)

It has long been evident that a major weakness in anthropology is the underdeveloped condition of ethnographic method. Typologies and generalizations abound, but their descriptive foundations are insecure. Anthropology is in the natural history stage of development rather than the "stage of deductively formulated theory" (Northrop 1947), it is history rather than science (Kroeber 1952:52–78), it has not discovered a fundamental unit comparable to the physicists' atom (a common complaint, variously worded; e.g., Kluckhohn

1953:517, Spuhler 1963). One may try to make the best of this situation by insisting that one prefers to remain a historian or a humanist, or one may look for improvement in ethnography. Taking the latter choice, the best strategy is not, I think, to seek to modify existing generalizations on the basis of intensive field work of the traditional sort in one or two societies (Leach 1961a, 1961b), nor to elaborate *a priori* typologies and apply them to more and more old descriptions by means of fancy retrieval procedures, hoping that the errors and incommensurabilities in the descriptive sources will balance out in the statistical manipulations used to yield generalizations. It is on this latter score that Needham (1962) attacks Murdock's methods (e.g., 1953, 1957), justifiably although intemperately. An interesting methodological contrast of this sort is provided by the exchange between Goodenough (1956b) and Fischer (1958) on Trukese residence rules: Goodenough pointed out the discrepancies resulting from his and Fischer's attempts to apply the usual *a priori* typology of residence in their independent censuses of Truk as a basis for urging that ethnographers should drop this method and substitute the search for the rules significant to the bearers of a particular culture in their own choices of residence. Fischer responded by tinkering with the *a priori* typology to take account of the Trukese peculiarities Goodenough had noted—yet there is no guarantee that the next culture examined will fit his new typology any better than Truk fitted the old one.

What is needed is the improvement of ethnographic method, to make cultural descriptions replicable and accurate, so that we know what we are comparing. Ethnoscience shows promise as the New Ethnography required to advance the whole of cultural anthropology.[2]

The ethnoscientific approach is now about ten years old[3] and has a rapidly growing body of practitioners in general agreement on methods and aims, in close communication with each other, and sharing an enthusiasm for the rehabilitation and revivification of ethnography. There are several excellent programmatic general statements about ethnoscience (Conklin 1962a; Frake 1962; Wallace 1962), which include (usually simplified) examples. However, 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 or "kinship algebra" or both, so that there may now be room for a more informal, less technical characterization.

The sections which follow attempt to present briefly and in rather general terms the main features of ethnoscience as a method, and to indicate some of the areas in which further work is needed. Usually, examples are either not given, or not described in sufficient detail for adequate comprehension of their relevance. The sources cited should be examined for more complete exemplification.

## PRINCIPLES

### 1. *Etics and Emics*

If a folk classification is ever to be fully understood, an ethnoscientific analysis must ultimately reduce to a description in terms approximating cul-

ture-free characteristics. Colors may be among the significant features in a folk taxonomy of plants; but color itself is classified by principles which differ from culture to culture, hence is a domain which must be analyzed ethnoscientifically before the botanical folk taxonomy is translatable into our terms (Conklin 1955). Enough is known about color, and the classificatory features involved are ordinarily sufficiently concrete, so that the color classification of a given culture may be relatable to culture-free physical and physiological features. Obviously there are very few aspects of culture where reductionism of this type is even remotely foreseeable. In domains where such reduction is not yet possible, the local perceptual structure may nevertheless be largely discoverable, even though incompletely translatable (see now Frake 1964:134). In fact, in some domains the very difficulties in observation which prevent the outside observer from analyzing the significant features in culture-free terms also force the bearers of the culture to utilize explicit verbalized defining attributes in learning and communicating about their own folk classification—hence make easier the discovery of attributes on this level—in contrast to classifications where the objects and their attributes are so concrete and frequent that the classifications may be well learned by exemplification rather than description (Frake 1961:124–25). Nevertheless, full understanding of a culture or an aspect of a culture and particularily its full description in a foreign language require the ultimate reduction of the significant attributes of the local classifications into culture-free terms. Lamb's discussion of the relationship between his semantic and sememic strata, and the parallel relationship between the phonetic and phonemic strata (Lamb 1964:75–77), is highly relevant here.

Culture-free features of the real world may be called "etics" (Pike 1954). The label may also be applied to features which are not truly culture-free, but which at least have been derived from the examination of more than one culture, or to the sum of all the significant attributes in the folk classifications of all cultures. Most of ethnography has operated with characteristics of this sort; ethnology has devoted much attention to the accumulation and systematization of features which *might* be significant in any folk classification, but it has given little attention to comparison of folk classifications or their principles as such. These results are by no means wasted from the point of view of ethnoscience: the ethnographer's knowledge of etics assists him in discovering the locally-significant features by guiding his initial observations and formulation of hypotheses.

Pike contrasts an etic approach with one which he calls emic, which amounts to an ethnoscientific one: 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). An emic description should ultimately indicate which etic characters are locally significant. The more we know of the etics of culture, the easier is the task of ethnoscientific analysis. Thus the great attention to kinship in the past, as well as the great amount of knowledge concerning cultural variability in kinship terminologies (the basic paper on the etics of kinship being a half

century old [Kroeber 1909]) is one reason why emic analyses of kinship are easier than those of art, or law, or religon. Better knowledge, at least among anthropologists, of the physiology and physics of color than of taste or smell more readily permits an ethnoscientific analysis of color, even though it is clear (Conklin 1955) that a folk domain including color need not be congruent with what the physicists understand by color. It seems probable that the vast accumulation of anthropological (both ethnological and archeological) knowledge of the etics of material culture will allow the ethnoscientific approach to be quite readily applied in this presently-neglected field. Furthermore, in material culture the objects classified are concrete and easily examined and usually readily observable in many examples during the time available for normal field work—in contrast to diseases, deities, etc. In classifications of concrete but natural, noncultural phenomena such as plants and animals, the range of variation which is classified is both extreme and beyond the direct control of the classifiers, who must select only certain features to which classificatory significance is given (Lévi-Strauss 1962b:73–74). But with cultural artifacts the corpus is smaller and the significant features are largely produced by the classifiers and hence should be more distinctive and more readily recognizable; also the ethnographer can here subject at least some of the features to controlled variation in order to test informants' reactions to their significance (cf. Berlin and Romney 1964 for an illustration of some of these advantages).

The nature of learning and of communication implies that a culture consists of shared classifications of phenomena, that not every etic difference is emic. But it should be emphasized that an emic analysis refers to one society, to a set of interacting individuals. Cross-cultural comparison, if we take culture in Goodenough's sense, is another level of analysis which involves the comparison of different emic systems. There is no reason why one should expect to find emic regularities shared by cultures differing in space or time. Thus Dundes' "emic units in the structural study of folktales" (1962) are not emic units in the sense here intended, insofar as the "system" of which they are analytical units is comparative (an etically defined "motif," "tale," or "tale type," whose actual manifestations in different cultures are treated as "variants"). On the other hand, Lévi-Strauss' brief characterization of some of the defining attributes of the "gustèmes" of the English, French, and Chinese cuisines (1958: 99–100) is a comparison of the emics of different cultures, although the emic analysis of each of the three cuisines is not presented in sufficient detail to be convincing. Even so, "slippage back and forth between individual systems and any and all systems, as context for contrast, recurs in his [Levi-Strauss'] work" also; "the first step in a resolution of the problem . . . is to refer structural contrast exclusively to within the domains of individual systems, where its cognitive basis can be empirically warranted" (Hymes 1964:45, 16).

### 2. *Domains*

One of the most important principles of ethnoscience, and one of those most often overlooked, is the necessity for determining in a nonarbitrary manner the

boundaries of the major category or classification system being analyzed, i.e., for discovering how a domain is bounded in the culture being described rather than applying some external, cross-cultural definition of the field. If this is not done, the description of the internal structuring of the domain is likely to be incomplete if not entirely erroneous, and the utility of the analysis for predicting the classificatory placement of new instances will suffer. (See now Hymes 1964:16–18.)

Any two cultures differ in the way they classify experience. Everyone with any familiarity with more than one realizes that this is true for the lower, more specific classificatory categories, and trivial examples are easily found. But we cannot assume that the higher, more general levels of the folk classifications of different cultures will coincide either; there is no reason to suppose that the total range of a set of categories will match that of the "corresponding" set in another culture even though the ranges of the lower categories in the two sets are different.

Thus every anthropologist recognizes that "uncle" is not a universal category, but most seem to suppose that "relative" or "kinship" is—i.e., that a set of categories defined by consanguinity and affinity is everywhere a "natural" set, that features such as ritual relationships must somehow be always outside the core system; the term "fictive kinship" is significant of the analytical bias. In contrast, Conklin (1964) specifically does not assume that "kinship" is a domain everywhere bounded in the same manner.

It is also customary to assume that everywhere there are just two systems of kinship terms: those used in "reference" and those used in "address." Thus an *a priori* decision is made as to the significant defining features and the number of coexistent systems. Such an analysis of the American kinship system blurs many distinctions: "mother's brother" and "father's brother" are required instead of "uncle" in some referential contexts; different forms of address are often used to differentiate co-resident "grandmothers" or "mothers" (Mo vs WiMo/HuMo); such terms as "father, dad, daddy, pop, old man" are not synonymous.

> The classic distinction between terms of address and terms of reference is not of much help in dealing with the American system. It tends to obscure certain important processes, partly, at least, because it presumes that there is a single term used in all referential contexts . . . . In the contemporary American system the wide variety of alternate forms allows them to differentiate a variety of different contexts (Schneider and Homans 1955:1195–96).

It seems probable that these "alternate forms" would turn out to be quite systematically structured, that several domains could be specified, were the contexts to be analyzed ethnoscientifically. One would expect a higher degree of agreement between informants in the usage of these terms if the contexts were discovered by the observation of natural situations or the asking of natural questions than is the case when informants are asked (e.g. Lewis 1963) to sort the "alternates" into contexts which are supplied ahead of time by the investigator, even though he himself is an American.

Frake (1960:58–59) has made the same point with regard to the Eastern

Subanun. Conklin (1951) described several Tagalog "co-existing sets of relationship terms," with their defining contexts. Swartz (1960) shows the relevance of situational environments to the choice between two Trukese terms. According to Chao's analysis (1956), there are three major sets of Mandarin Chinese kinship terms, which are not entirely synonymous even in their kin-term referents; furthermore, the contexts in which Chinese "terms of address" (pronouns, kinship terms, proper names, and titles) are used can be analyzed in terms of the intersection of seven main categories of hearers and ten main categories of person spoken of or addressed (Chao 1956). Presumably this kind of situation is quite general. Yet Norbeck can still conclude a discussion of the "errors" in Morgan's schedule of Japanese kinship terms by urging "the importance of making clear distinctions between terms of address and terms of reference" (1963:214) when it is clear from his preceding discussion (and from Befu and Norbeck 1958) that there are many more than two systems here, and that some of Morgan's "errors" in fact represent accurate reporting of one of these systems.

The arbitrary delimitation of major domain boundaries persists in kinship studies even though the analytical procedures here are the most developed ones in ethnography. It is an even more obvious fault in other areas. Many of the difficulties, for example, in discussions of "primitive art" are seen in a new light when one ceases to assume that "art" is a universal category. The assumption that "cultures . . . have in common . . . a uniform system of classification. . . . a single basic plan" (Murdock 1945:125) is stifling to ethnoscientific analysis.

There may be domains—perhaps kinship is one of them—which are more nearly universal than others, where cross-cultural comparison would show greater sharing of significant features for higher level taxa than for lower level ones. But this is a significant hypothesis to be tested by the comparison of domains from different cultures, each analyzed without prejudice, rather than being a postulate determining the delimitation of domains to be analyzed. Prior assumption of the universality of domains, as in much work on kinship and other domains (e.g. color), prejudges the case and masks some of the variability the explication of which is a classical task of anthropology.

But procedures for the definition of domains are not yet well worked out—this remains one of the more difficult problem areas of ethnoscience (Conklin 1962a:124, 1964; Öhman 1953; Voegelin and Voegelin 1957). However, the problems do not differ in kind from those involving the identification of categories on lower levels, or the discovery of significant contexts or environments.

### 3. *Terminological Systems*

Research in ethnoscience so far has concentrated on classifications as reflected by native terminology, on "discerning how people construe their world of experience from the way they talk about it" (Frake 1962:74).

> The analysis of a culture's terminological systems will not, of course, exhaustively reveal the cognitive world of its members, but it will certainly tap a central portion of it. Culturally significant cognitive features must be communicable between persons in one of the standard

symbolic systems of the culture. A major share of these features will undoubtedly be codable in a society's most flexible and productive communication device, its language (Frake 1962:75; cf. Conklin 1962a, Goodenough 1957, Lounsbury 1963).

The main evidence for the existence of a category is the fact that it is named. As a result, the analyst faces the problem of locating *segregates* (segregate: "any terminologically-distinguished . . . grouping of objects," Conklin 1962a:120–21; Frake 1962:76). Much work on the "Sapir-Whorf hypothesis" has assumed that any morpheme, word, or grammatical construction labels a category of meaning, that the semantic structure of a language is built up only of these units. But it is clear that contrasting categories within a terminological system, and within a single level of a system, are frequently named with units whose positions in the strictly linguistic system vary markedly—morpheme, word, phrase, etc. (Conklin 1962a; Frake 1961, 1962; Lounsbury 1956:190–92). These labels of classificatory categories, whatever their grammatical status, have been called "lexemes." Alternatively, a lexeme is a "meaningful form whose signification cannot be inferred from a knowledge of anything else in the language" (Conklin 1962a:121; see also Weinreich 1963:145–46; Lamb's use of the term [1964] is nearly equivalent). Thus for example "stool" is a lexeme in English, and kwêi chéi ('stool') is a lexeme in Burmese labelling an approximately equivalent segregate, even though kwêi ('dog') and chéi ('leg(s)') are also nouns occurring independently as labels for other segregates. The analyst must differentiate between lexemes and other linguistic forms of similar grammatical status which do not serve as segregate labels. The solution of this problem depends partly on knowledge of the language, both comprehension of it and technical knowledge of its structure. Comprehension is required because translation prior to semantic analysis causes insuperable difficulties because of the incommensurability of the semantics of any two languages (Conklin 1962a: 125–27 gives a nice example). Furthermore, in practice much of the best data comes from observing linguistic behavior outside the formal eliciting situation with an informant. One task of ethnoscience, in fact, can be viewed as the solution of the old problem of translation.

Knowledge of the linguistic structure is necessary because the category names belong to two systems, one linguistic and one nonlinguistic; or, in Lamb's terms (1964), because lexemes are related by representation to both the morphemic and sememic strata. "While identity between the two planes is incomplete, it is a useful starting-point from which to describe the lack of isomorphism actually found" (Weinreich 1963:117). Lamb (1964:61–66) catalogs the different possible discrepancies between units on different strata.

The many discussions within linguistics of the relevance of meaning to the analysis of phonology and grammar apply here also; even if form and meaning are in principle independent, or at least not isomorphic, and if (as some have maintained) an appeal to meaning is methodologically unsound in linguistic analysis, nevertheless the practice of linguistic field work has established that in order to get the job done within a reasonable time, on the basis of a corpus of practical size, it is essential to appeal to meaning in some manner—by the same

or different test, the pair test, or some less explicit test where the linguist is analyzing his own native language (see Voorhoeve 1961:41–42 on the semantic element in such tests). The converse applies to ethnoscientific analysis: although the two systems are not entirely congruent, the overlap is sufficient so that an "appeal to linguistic form" is a very useful field technique in working out a terminological system. In fact, the development of ethnoscience will certainly eventually assist strict linguistics in handling the "problem of meaning."

Efforts to discover nonterminological systems in such areas as behavior units (Barker 1963; Barker and Barker 1961; Barker and Wright 1955), folktales (I évi-Strauss 1955; Leach 1961c), and values (Kluckhohn 1956, 1958) have not employed rigorous, replicable procedures for identifying units without the application of criteria foreign to the cultures analyzed; in this regard they differ little from many previous ethnographies. These studies attempt to discover classifications without first establishing the communication systems by which they are transmitted.

Nonlinguistic communication systems are also structured. Birdwhistell's work with kinesics (1952) and Hall's with proxemics (the structuring of space in interpersonal relations) (1963a, 1963b) are concerned with establishing the units of the codes, and to some extent with discovering categories of meanings, but both jump to rather anecdotal cross-cultural comparisons before working out the structure of any one system. The nonisomorphism of sememic and lower strata can be expected to hold here also. Other communication systems are also relevant, including paralanguage (voice qualities and nonlinguistic vocalizations; see Trager 1958). Material culture resembles language in some important respects: some artifacts—for example, clothing—serve as arbitrary symbols for meanings (i.e., noniconic signs [Goodenough 1957]) and occur in a limited number of discrete units whose combinability is restricted. Possibly complex phenomena of esthetics would yield to a similar approach. Studies in these areas are potentially of much importance for ethnography, and it seems wise not to restrict the meaning of ethnoscience to the study of *terminological* systems.

4. *Paradigms and Componential Analysis*

A key concept in ethnoscience is that of the contrast set. This is a class of mutually exclusive segregates which occur in the same culturally relevant environment (setting, context, substitution frame, surroundings, situation, etc.). These segregates "share exclusively at least one defining feature"—i.e., that which characterizes the environment in which they occur (Conklin 1962a: 124; cf. Frake 1962:78–79). The domain of the set is the total range of meanings of its segregates.

The notion of contrast is relative to the environment within which it occurs. Thus the mutual exclusion in English between 'ant' and 'ship' (Conklin 1962a: 127) or between 'hamburger' and 'rainbow' (Frake 1962:79) is not contrast in this sense, because the environment which they share is not culturally relevant.

As Frake (1962:79) puts it, "In writing rules for classifying hamburgers I must say something about hot dogs, whereas I can ignore rainbows. Two categories contrast only when the difference between them is significant for defining their use. The segregates 'hamburger' and 'rainbow,' even though they have no members in common, do not function as distinctive alternates in any uncontrived classifying context." Although 'ant,' 'ship,' 'hamburger,' and 'rainbow' are all 'things,' the sub-sets of 'things' to which they belong are so far removed from each other that these four segregates themselves are never distinctive alternates. Any culturally significant partitioning of 'things' would involve contrasts between segregates on a much higher level. Lower level environments are of primary importance—in this case, for example, the environment in which such segregates as 'hamburger,' 'hot dog,' and 'cheeseburger' contrast, and the environments in which such adjacent segregates as 'sandwich,' 'pie,' and 'something to eat' occur (Frake 1962:78–82).

One may conceive of a contrast set containing only one segregate; if, as seems likely, there are no complete synonyms, then every segregate does occur in an environment which no other segregate shares. But "contrast" implies that the set contain at least two segregates, and the term is normally understood in this way. Since these minimal two contrast in the same environment, each must have some unique feature of meaning.

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 (Chafe 1962; Conklin 1962a:132; Goodenough 1956a:197, 202; Lounsbury 1960:127–28, 1962). A set of only two segregates can be considered a paradigm, but normally the term is applied to sets of three or more segregates, so that at least some of the sub-sets consist of two or more segregates sharing some feature of meaning.

It is important to note that while all contrast sets are paradigmatic, not all paradigmatic sets are *complete* contrast sets. A paradigmatic set may not be equivalent to its containing contrast set: it is possible to analyze paradigmatically a collection of items which do not *exclusively* share any feature, which do not exhaust the membership of a class occurring in a single environment (Conklin 1964). Thus Burling (1963a) has made a paradigmatic analysis of a set of "core kinship terms" which however do not form a complete contrast set—there is no culturally relevant environment which differentiates these terms from the other Garo kinship terms. A parallel example from phonology (where a paradigm involves phonetic rather than semantic features) is Chafe's (1962:338–39) paradigm of English consonant phonemes, which excludes some phonemes (*l*, *r*, perhaps also *y*, *w*, *h*) which are included in the relevant contrast set.

This difference between a paradigm and a contrast set is not always recognized in ethnoscientific work. Yet if the analysis is required to reflect the cognitive system of the bearers of the culture, before attempting a paradigmatic analysis one should show that one is dealing with a complete contrast set, that there is a culturally relevant environment in which all and only the segregates

in the set occur. This is the problem of definition of domains seen from a somewhat different angle.

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 1962:83). 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 segregate. It is normally assumed that the number of componential dimensions will be smaller than the number of segregates they define. The paradigm may then be viewed as a multidimensional structure, in which the categories are placed according to the componential dimensions. (Useful references on componential analysis include Conklin 1962a, 1964; Frake, 1962; Wallace 1962; Lounsbury 1956, 1962; Goodenough 1956a; Sebeok 1946; Chafe 1962. Lamb's [1964] sememes are similar to, if not identical with, the semantic components of these authors.)

There are two points of view regarding such componential analyses (Burling 1964). According to one of them, the componential analysis should reflect the classificatory principles utilized by the bearers of the culture, the components should be "cognitively salient"; such an approach has been labeled an aim for "psychological reality" (Wallace and Atkins 1960:64). However, this is a difficult requirement: such features are often not consciously formulated,[4] and furthermore different bearers of the same culture may utilize different features and yet share the same categories and communicate perfectly (Wallace 1962). The other position is what Wallace and Atkins (1960:64) refer to as an aim for "structural reality," and what Lounsbury (1964) calls a "formal account." This position drops at this point the requirement that an ethnoscientific analysis should reflect the cognitive world of the bearers of the culture being analyzed. Having discovered the culturally significant sets and their included units, say these workers, we now try to determine the most economical componential analysis which will define (or "generate") their paradigmatic relationship—we are concerned only with predictability, economy, and inclusiveness, not any longer with cognitive saliency. Others take an intermediate position, and allow the use of hints from the culture in deciding between variant componential solutions which are equally or nearly equally economical—for there will often (if not always) be such variants, and furthermore the criterion of economy (simplicity, parsimony) is not an easy one to define and apply (see Wells 1963:42 on this last point). Romney and D'Andrade (1964) discuss this problem, and illustrate some testing procedures for determining the cognitive saliency of alternate componential analyses of the same set of terms. Cancian (1963) has illustrated another method, which may be used to evaluate a componential analysis of a multi-position classification. If it is possible to determine the position in this classification of some items whose exact position

is not known to all informants familiar with the classification, the correctness of the components used in setting up the classification can be tested by means of the magnitude of informants' errors in placing items unknown to them. If errors are extreme, the classification is shown to be erroneously understood by the ethnographer. "When an informant makes an error that results from lack of precise information, he is most likely to approximate the truth in terms that are meaningful to him" (Cancian 1963:1073).

Weinreich (1963:148–49) points out that componential analysis is more appropriate in some domains than in others. In a given culture, some domains will be more highly patterned; in these, "distinguishing components recur in numerous sets of signs, [whereas] the bulk of the vocabulary is of course more loosely structured and is full of components unique to single pairs, or small numbers of pairs," of segregates. While componential analysis is still possible in these latter "non-terminologized fields," Weinreich suggests that the cognitive saliency of components will be greater in the more structured domains and the validity of the componential analysis can be more readily checked by informants' reactions in these domains.

It is important to note that not every componential analysis is ethnoscientific. Semantic and ethnoscientific studies have adapted the method from its use in another area, phonology (e.g. Harris 1944). When semantic componential analysis is applied to paradigms which are not complete contrast sets, the results are not strictly ethnoscientific. Furthermore, the method essentially amounts to focussing on the differentiating features of a classification rather than on its categories or pigeonholes. Hence any classification is amenable to analysis resembling a componential one, and the technique is very useful for extending and elaborating purely etic typologies having nothing to do with ethnoscience. Thus, for example, Pike (1943) was able to improve greatly on existing compendia of articulatory phonetics by attending to the distinctive features of previous phonetic typologies, and extending and recombining them to produce new phonetic types and a more logical classification. Similarly, Balfet (1952) produced the best available typology of basketry techniques by abstracting the components of previous classifications and re-arranging them to produce logical grids with many new classificatory slots, some of them as yet unknown in actual specimens even though fully possible. Malkiel (1962) describes a typology of dictionaries which explicitly borrows from the method of componential analysis.

### 5. *Taxonomies*

Different segregates within a folk classification may be related to each other in various ways: as part to whole, as sequential or developmental stage to stage, as different grades of intensity, etc. (Conklin 1962a:129, 1962b; Frake 1964). 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 classifications. In a taxonomy, there is a series of hierarchical levels, with each segregate at one level included

in (only) one segregate at the next higher level. It is sometimes possible to analyze componentially a contrast set which forms one level of a folk taxonomy, but it is impossible to analyze in this way the whole taxonomy, even though the boundaries of the whole must define a domain: a single contrast set is limited to one taxonomic level (Conklin 1962a:128, 1964; Frake 1962).

A single folk classification may contain sets of segregates interrelated in different ways. From one point of view, any folk classification is a taxonomy since the domain or environment of the whole classification may be taken to define the most inclusive taxonomic level. But if the segregates within such a classification are not further related by inclusion, the taxonomy has only two levels and is relatively uninteresting as such; what is then more interesting is the kind of non-taxonomic relationship between the lower level segregates. A folk taxonomy of more than two levels, interesting as such, may also contain within it segregates which are interrelated in some nontaxonomic way (e.g., as developmental stages) which together form a domain which itself is placed within a taxonomic series.

Some attention has been devoted to folk taxonomies, particularly in ethnobotany, and the prospects are good for comparisons of folk taxonomic principles intra- and interculturally, but much of the methodology still requires attention. Further discussion will be found in Conklin's recent (1962a) excellent general treatment of folk taxonomies.

### 6. *Discovery Procedures*

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. While arbitrary stimuli—i.e., stimuli foreign to the culture—may yield nonrandom responses, the patterning involved derives from the cognitive system of the bearers of the culture, and the principles of this system are not likely to be made clear by answers to the wrong questions. Regularities will appear if one measures continental European manufactured goods with an American or British yardstick, but measuring with a meter stick will much more readily reveal the principles of the system relevant in European culture.

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. Hypotheses must be checked in the field situation, and revised if they turn out not to fit the field data. Thus it is impossible to make a strictly ethnoscientific analysis of data previously collected, by oneself or by someone else, according to different procedures. Any componential or similar analysis made of such old data must be treated as an inadequately checked hypothesis. Structural restatements of even the best old field data may prove impossible. Lévi-Strauss illustrates some difficulties which

> result from our ignorance of the observations (real or imaginary), facts, or principles which inspire the [folk] classifications. The Tlingit Indians say that the wood worm is "clever and neat," and that the land-otter "hates the smell of human excretion." The Hopi believe that

> owls exert a favorable influence on peach trees. If these attributes were taken into account in placing these animals in a [folk] classification of beings and objects, one could search indefinitely for the key, were not these minute but precious indications furnished by chance (Lévi-Strauss 1962b:81; my translation).

Criterial attributes must be investigated in the field.

The general principle here is widely recognized, but only very recently has attention been devoted to making explicit the discovery procedures involved. Discussion and exemplification so far have concentrated on the use of questions in the native language and *chosen from the customary repertory* of the culture being studied. Frake's explication of interlinked topics and responses of queries in Subanun is an excellent example. His general suggestions on distinguishing questions which are appropriate to particular topics from those which are inappropriate (1964:143–144) should be particularly noted. Sarles (1963) describes a related procedure, in this 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. Metzger and Williams, in a series of papers as yet only partly published, have emphasized the discovery, selection, and use of question "frames" appropriate for eliciting specific folk classifications, particularly among the Tzeltal and Ladinos of Chiapas (Metzger 1963; Metzger and Williams 1962a, 1962b, 1962c, 1963a, 1963b). These papers are important particularly in that the frames utilized are explicitly stated, as a means of ensuring replicability and demonstrating the reliability of the analyses. Conklin (1964) has suggested some improvements in the genealogical method applicable to field studies of kinship systems, including the use of question frames, the recording of conversations in native settings,[5] and the use of "ethnomodels" or native metaphors and diagrams of classifications (including diagrams volunteered by informants to aid in explaining to the ethnographer and influenced by observation of the ethnographer's charting attempts).

The emphasis on the classes of responses elicited by appropriate questions is beginning to show the expectable extreme complexity of the cognitive map of any culture, with multitudinous interlocking and overlapping contrast sets. Even so, these papers concentrate on the discovery of categories and their significant environments; as yet insufficient attention has been devoted to the development of reliable techniques for elucidating the further underlying complexities in cognitively salient semantic components.

I have already mentioned the relevance of ethnoscientific methods to material culture, where the possibility of pointing to and manipulating concrete objects may partially replace the use of question frames and the reliance on terminological systems in eliciting significant categories and contrasts. Another area where similar comparison of concrete cultural manifestations may be possible is music. Recent published discussions by ethnomusicologists of their problems in developing appropriate notation systems imply, at least to a nonspecialist, that the etics in this field have developed to the point where the

application of ethnoscientific methods would resolve many difficulties and lead to a true *ethno*musicology (see Bright 1963:28–31).

## EXAMPLES

Despite considerable discussion of ethnoscience in recent years, there have been relatively few applications of the methods in the only context in which they matter: intensive field work. This section provides an annotated bibliography of most but not all of the published or nearly-published ethnographic reports which qualify as ethnoscience, insofar as I am aware of them. I comment also on some publications which will not so qualify, but which are of interest in this connection because of similarities or contrasts in method or theory.

### 1. *Pronominal and Case Paradigms*

Analysis in terms of semantic components was first applied to paradigms of affixes, particularly to sets where the components are at least sometimes overt, i.e., components "with separate phonemic identities" (Lounsbury 1956:161–62; in Lamb's [1964] terms, where there is simple representation between the sememic, lexemic, and morphemic strata). In these instances, the contrast set is defined morphologically, in terms of its linguistic environment. The first development of the methods is due to Roman Jakobson, who applied them in an analysis of the semantic components of the Russian case system (Jakobson 1936). This was followed by Trubetzkoy's (1937) componential analysis of the Slovak case system (with some comparison of Slavic case systems on the same basis), and by Sebeok's (1946) analysis of the Finnish and Hungarian case systems and comparison of the structural principles of the two.

Lotz (1949) followed with an analysis of the Hungarian pronominal suffixes, which included a diagram exhibiting the suffixes in a structure whose dimensions consist of semantic oppositions. Wonderly, pointing out (in effect) that Sebeok's components are much less overt than Lotz's, analyzed the pronominal suffixes of two dialects of Quechua in terms of semantic components which are covert in that each morpheme is associated with two components, but where the distributional classes of the morphemes are associated with the components on a one-to-one basis (Wonderly 1952). In the use of distribution to validate the semantic components, Wonderly's treatment resembles Harris' (1948) analysis of the Hebrew pronominal paradigm—but Harris identified the components solely in terms of the shared linguistic environments of the morphemes, and did not attempt to identify shared features of meaning (Lounsbury 1956:162). D. Thomas (1955) gives a componential analysis of Ilocano pronouns which utilizes both semantic and distributional criteria. A similar analysis for another Philippine language, Maranao, is briefly outlined by McKaughan (1959); but according to his analysis the forms are not affixes, yet he gives no evidence that the paradigm is a complete contrast set. A very similar componential structure is indicated for the pronominals of a third Philip-

pine language, Hanunóo, where the morphemes are also not affixes; but Conklin (1962:134–36) is careful to demonstrate that the paradigm is a complete contrast set. He includes a dimensional diagram of the type introduced by Lotz (1949), as does Berlin (1963) in his componential analysis of Tzeltal pronominals (a more complex system than the Philippine ones), and Austerlitz (1959) in his componential analysis of Gilyak pronouns. In a footnote, Austerlitz (1959:104) notes the possibility of reducing the three pairs of oppositions of his analysis to two, by introducing a rule of order of application of the oppositions. The semantic components involved make clear the implications of an alternative application of forms glossed 'thou' and 'ye' in addressing a single person, and an alternative between 'he, she' and 'thou' in addressing one's spouse.

In a fascinating paper, Brown and Gilman (1960) have analyzed in detail a somewhat similar alternative in European languages (English, French, Italian, Spanish, and German; Slobin [1963] finds the analysis to be applicable to Yiddish also). They describe the rules for selecting, now and in the past, between the two singular pronouns normally glossed 'familiar' and 'polite.' There turn out to be a whole series of correlates of choice between the two, which Brown and Gilman reduce to two basic semantic binary oppositions, power or status (superior-inferior) and solidarity or intimacy (solidary-nonsolidary), discussing their association with features of social structure, political ideology, and affective style. The relationship between these dimensions is complex enough so that it would be difficult to diagram, and further complexities are introduced by ongoing changes which the authors demonstrate both by historical data and by data on individual variation in present usage. In another paper, Brown and Ford (1961) show the relevance of the same dimensions to choices between a variety of forms of address in modern American English which are not grammatically obligatory, unlike the pronouns previously analyzed (the dimensions governing the selection of pronouns are here said to hold for 20 languages of Europe and India, and for Japanese). Brown's emphasis on social correlates and his detailed examination of the semantic dimensions in various behavioral contexts might well be combined with a more explicitly componential analysis and more careful delimitation of domain boundaries. Probably some of Brown's methods are adaptable to the problem of determining the cognitive saliency of semantic components in this and similar domains. Brown alludes to some interconnections between usages of kinship terms and other terms of address. An approach similar to his might elucidate some of the semantic dimensions of choice between alternative or "variant" kinship terms, and it would be particularly interesting to apply similar techniques to the study of the complex and often interrelated kinship, status, and personal address terminological systems of Southeast and Eastern Asia (see Koentjaraningrat 1960:107–14, for an example).

### 2. *Kinship Terminologies*

Componential analysis was first applied to kinship terminologies in simultaneous and independent inventions by Ward H. Goodenough and Floyd G.

Lounsbury in 1947–1949. 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 (Lounsbury, in conversation, November, 1960). Goodenough published first, giving a methodological statement together with a componential analysis of the Trukese kinship terminology he had collected in the field (1951:103–10). In a pair of important papers appropriately published in a 1956 issue of *Language* dedicated to A. L. Kroeber, both authors set out careful theoretical and methodological treatments, Goodenough's illustrated by a revision of his Trukese analysis and Lounsbury's by a structural analysis of the Pawnee terminology collected by an Indian Agent in 1863 for Lewis Henry Morgan (Goodenough 1956a, Lounsbury 1956). Wallace and Atkins (1960) have analyzed and compared the methods of these two papers in some detail. An important difference not noted by Wallace and Atkins—although it is related to the problem of metaphors which they do discuss—is that Goodenough makes plain (more so in his earlier monograph [1951:103–107] than in his methodological paper [1956a]) that the paradigm he is analyzing is also a complete contrast set. He is able to do this because he was concerned while in the field with identifying the boundaries of the domain glossed 'kinship.' Lounsbury (1956), however, is not able to do this, since he is analyzing a set of terms collected long ago with unknown sampling procedures. He goes further, and explicitly excludes some of the terms listed by Morgan, making his selection on the basis of *a priori* criteria (1956:163).

Since these ground-breaking papers, a number of other kinship terminologies have been analyzed componentially, with variations in techniques of analysis, in methods of presentation, and in the extent of discussion of methodological problems. Conklin's unpublished Ph.D. thesis (1954:80) included a brief componential analysis of Hanunóo consanguineal terms of reference, influenced by Goodenough's monograph (1951) and by knowledge of Lounsbury's Pawnee analysis prior to its publication. Romney and Epling (1958) gave a componential analysis of Kariera kinship terminology, pointing out some of the behavioral correlates of the semantic features. Pospisil (1960) gave componential definitions for the Kapauku terminology, and followed this with a classification of the terms in an outline which has some of the characteristics of a key (cf. Conklin 1962b, 1964). Frake's treatment of Subanun kinship (1960:58–63) emphasizes the nonarbitrary delimitation of domains, the determination of complete contrast sets, the environmental features determining selection between alternative sets of terms, and the investigation of behavioral correlates of the kinship categories, as well as presenting a well-diagrammed componential analysis. Wallace and Atkins (1960) illustrate their theoretical discussion with a componential analysis of some American-English consanguineal terms of reference, admitting that for purposes of simplified illustration they use an arbitrarily delimited paradigmatic set.

Conant (1961) presented an interesting "componential comparison" of

"Jarawa kin systems of reference and address," emphasizing eliciting procedures for maintaining the Jarawa distinctions between the two systems, pointing out that the system of address here involves "non-kin terms" ("which have meanings and usages not restricted to kinship") in the same contrast set with kin terms, and discussing problems of establishing the psychological reality of the componential analysis (including evaluation of disagreements between informants). One of his conclusions from the comparison is that the Jarawa address system has more behavioral correlates than the reference system. In contrast, Grimes and Grimes (1962) restrict their componential analysis of Huichol terminology to those terms of reference "that are amenable to simple structural statement" (1962:104). Yet within this arbitrary boundary, they are concerned with criteria of cultural relevance—for example, in setting up an unusual dimension of "distance from ego" because it accords well with some other characteristics of the social structure.

Epling (1961) published a detailed analysis of Njamal terminology, in which he assumed that his componential description is the psychologically real one. Burling (1962) challenged this assumption by presenting an alternative componential analysis of the same set of terms. He greatly simplified the componential formulae required, mainly by defining components specifically relevant to this system rather than following Epling (1961:155) in using only dimensions as defined by Kroeber (1909) plus one from Lounsbury (1956). Burling also discussed some of the problems in applying the criterion of economy in choosing between different componential analyses, and emphasized that this criterion, difficult as it is, is not the same as the criterion which demands cognitive saliency for a componential analysis. In a later paper using his own Garo data and influenced by recent developments in linguistics, Burling (1963a) modified the usual procedures of componential analysis by selecting a set of "core terms" which are readily analyzable componentially, then using these terms "as building blocks to provide definitions for the remaining kinship terms used by the Garo," these latter being labelled "derived terms" (1963a: 80). He disavowed any implication of psychological reality for this scheme. A third paper combines the two points, in an analysis of Burmese kinship terminology (Burling 1963b). Here he presents two full analyses of a set of referential terms, one of them componential in the usual sense, and the other using a method similar to the one he had previously applied to the Garo system. While Burling gives some evidence that certain features of the second analysis better reflect the Burmese cognitive system, he again disavows an interest in using this kind of criteria to choose between alternative analyses.

Lounsbury's recent work (1962, 1964) has concentrated on increasing the parsimony of componential analyses. The two papers cited deal with the Seneca consanguineal terminology (1962) and with some general techniques for simplifying analyses by the application of "generative rules" resembling those used by Burling but more elegantly stated and of more general application. As before, Lounsbury is concerned with improving the logic of the analytical method. He does point out possible sociological correlates, but he views "formal accounts" such as his, which operate with criteria of parsimony and suffi-

ciency, as logically prior to "functional accounts" (Lounsbury 1964). The criterion of sufficiency requires that the analysis correctly account for all the empirical data in hand; but since the data he uses derive from fieldwork not oriented towards his problems (e.g., his Seneca data are largely from Lewis Henry Morgan) he cannot be certain that his paradigms represent complete contrast sets—thus the "root meaning" or common semantic feature defining the domain of a paradigm is that feature shared by all the forms of the set, but the additional phrase "and no others" which would define a complete contrast set is missing (Lounsbury 1962)—and the adequacy of his analytical models is tested by their ability to account for data which is at least secondary as compared to the primary field data against which ethnographic theories should be tested if they are to be adequate cultural descriptions. There can be little doubt, however, that Lounsbury's improvements in analytical method will be very valuable to ethnographers interested in gauging the cognitive saliency of structural analyses: procedures for developing additional alternative models should help in discovering the one (or more than one) which is the most "real" for a given culture, and it is not difficult to conceive of cognitive saliency for generative rules such as Lounsbury's. Furthermore, his models are already enabling him to devise ethnological typologies much more powerful than previous ones for cross-cultural comparisons of kinship terminologies and their correlates.

Romney and D'Andrade (1964) have again demonstrated the possibility of alternative componential analyses of the same paradigm—in this case, the restricted set of American-English terms previously analyzed by Wallace and Atkins (1960). The results of their testing of informants to determine the cognitive saliency of the variant analyses imply that it might be useful to use some such tests as an aid in the construction of models, rather than using tests to compare formal models previously devised.

Conklin (1964) emphasizes the desirability of combining "in actual field situations, recording activities, analytic operations, and evaluative procedures." The evaluative procedures on which he concentrates involve "the discovery of locally recognized contrasts, within recurrent ethnogenealogical settings." This paper is important for its many suggestions for methodological improvement, particularly in the development of more rigorous field techniques. It is illustrated by a detailed presentation of the Hanunóo kinship system as analyzed by these methods.

An advance in a new direction has been made by Friedrich (1963), who uses componential analyses to reconstruct the evolution of the Russian kinship system from the Proto-Indo-European period to the present, giving particularly detailed analyses of the Old Russian and Modern Russian terminologies. He demonstrates the advantages of such an approach over previous methods for reconstructing the history of kinship terminologies.

### 3. *Color Terminologies*

Useful contrasts with the problems of ethnoscientific analysis of kinship are provided by recent work on color terminologies. Some such domain is probably universal, but it is very clear here that domain boundaries vary from culture to

culture. Conklin's analysis of Hanunóo color terminology (1955) provides a good starting point. Several features of this overly-brief account are too often overlooked: (1) The culturally relevant domain, for which Hanunóo lacks a covering lexeme, is *not* equivalent to that labelled "color" in English, since it involves semantic dimensions additional to those of hue, saturation, and brightness which delimit the domain in English; (2) This being the case, the basic structure of the terminological system would not have been discovered had the ethnographer restricted his investigation to the use of artificial stimuli such as color chips; (3) Discovery of the taxonomic nature of the system required observation of Hanunóo behavior in contrastive situations normal to them; (4) The "two levels" of contrast described for this system are relevant to it, not proposed as a cultural universal, and even the Hanunóo second, more specific, level (not here analyzed) is said to include several sublevels. The first two points particularly have not been attended to in other studies—including those which cite this paper as a model. Eliciting procedures such as those recommended by Ray (1952, 1953) and those used by Lenneberg and Roberts (1956), Landar, Ervin, and Horowitz (1960), and Goodman (1963) will not reveal such criterial attributes as moisture, surface texture, etc., as may exist (cf. Newman's remarks quoted by Lenneberg and Roberts 1956:23), nor will they make evident the nature of different coexisting systems which may occur (e.g., special color terminologies for horses in Navaho [Landar, Ervin, and Horowitz 1960:371 n. 12] and Papago [O'Neale and Dolores 1943], or for cattle in Nuer [Evans-Pritchard 1940:41–45]). Nevertheless, work to date provides nice illustrations of the cultural relativity of semantic distinctions. Taking only the spectral dimension of hue, the most central feature for cross-cultural equating of 'color' domains, one could now add Hanunóo (Conklin 1955), Navaho (Landar, Ervin, and Horowitz 1960), Malayalam (Goodman 1963), and perhaps Zuni (Lenneberg and Roberts 1956) to the chart Gleason (1955:4 and 1961:4) gives comparing the very different placement and number of basic categorizations made by English, Shona, and Bassa.

Probably color terminologies are everywhere taxonomies of at least three levels. The relation of inclusion which defines a taxonomy is well illustrated by the specific studies of Hanunóo, Navaho, and Malayalam. All have a domain 'color' (sometimes lacking a lexeme) at the most inclusive level, with a small number of basic or primary terms at "Level I," and with a great number of more specific terms, all included under one or another of the basic terms, at "Level II" (probably usually with further levels below this). However, the Zuni research did not investigate this point but evidently assumed that the artificial testing situation itself would elicit terms on the same taxonomic level; deductions as to probable primary terms in Zuni can be made from the data provided as to which terms were most frequently used, but neglect of levels of contrast certainly accounts for some of the variability between subjects in the Zuni experiments.

Conklin (1955) gives a componential analysis of the four Level I Hanunóo

terms; the other authors are prevented from making such analyses by their concentration on contrasts along the dimension of hue. The Navaho study gives more detail than the others on more specific Level II color terminology and mentions several dimensions on this level, but does not analyze them componentially. One step which should now be taken is to investigate levels below the primary level in a folk taxonomy of color; it is evident that this will be a considerably more difficult task than the analysis of Level I terms.

The research of Lenneberg and others (Lenneberg 1961; Landar, Ervin, and Horowitz 1960; and references in both) on "color codability" has shown interesting variation between speakers of the same and of different languages in the extent of agreement on the application of terms to specific colors, on the width of overlap of terms measured against scaled stimuli, and on the relationship between the folk terminology and abilities to recognize and discriminate between colors. The domain would seem to be particularly useful for such tests of the effects of folk classification—culture—on behavior, because different areas of the same taxonomy vary in the extent to which individuals agree on categorization, and vary in the discreteness and degree of criteriality of semantic features, and because at least some of the distinguishing features are relatively easy to codify and display in testing materials.

4. *Other Domains*

It is instructive to compare what has been accomplished and what can be envisaged in the analysis of color terminologies with the possibilities in the domain of smell and taste. In these areas English has a relatively small and weakly terminologized vocabulary, and, particularly in comparison to color, the etics involved are very poorly known. Thus Aschmann (1946) lists a number of Totonac stems in a domain which may reasonably be glossed 'smell,' in the form of a taxonomy with eight primary categories each labelled with a basic root, with a quite vague characterization of the meaning of each of these eight roots. Each class is in turn subdivided by terms for more specific 'smells,' but the lack of relevant etics forces Aschmann to define each more specific term merely by listing objects characterized by that 'smell.' Each term on this lower level consists of the root labelling the higher class, plus an affix; these affixes recur with the different roots in the set, but the lack of etics prohibits the recognition of any features of meaning which may be associated with the affixes. It would be possible to determine, with informants, whether Aschmann's analysis represents a true folk taxonomy and whether this has more than three levels, but the lack of appropriate etics would make it exceedingly difficult if not impossible to identify criterial attributes.

A domain in which smells and tastes in turn might be expected to serve frequently as criterial attributes is that of cuisine. Lévi-Strauss' brief suggestion for the analysis of "gustèmes" (1958:99–100) influenced L.-V. Thomas (1960) in a description of the Diola cuisine. But this first attempt of any length must be said to have failed. Thomas listed and described the principal Diola

recipes, but grouped them according to an arbitrary imposed scheme. He then took up the binary oppositions suggested by Lévi-Strauss, plus a few of his own, and applied them as *a priori* descriptive devices to the whole cuisine—without any effort to account exhaustively for the corpus he had just presented, and without any attempt to discover any Diola classifications of foods or recipes other than that implicitly recognized by his (incomplete) mention of Diola names for the recipes described. The oppositions were not even related to the distinctions between individual recipes. As Thomas remarks, it is surprising that more attention has not been devoted to cuisines by ethnographers. The domain should yield readily to an ethnoscientific approach.

Ethnobiology is frequently cited in illustration of ethnoscientific methods, particularly the study of folk taxonomies. A great deal of work in this field is partially relevant, in that it has frequently been realized (although also too often ignored) that the species and genus categorizations of other cultures normally do not coincide with those of Western science. A good example is Bulmer's (1957) discussion of bird naming practices among the Kyaka of the New Guinea highlands. He recognizes differences between the Kyaka and scientific classifications, but tends to assume, for example, that apparent synonymy represents ignorance or confusion without testing for levels of contrast or the effects of setting (he does note that names for hawks are more accurately—i.e., more consistently—applied to specimens seen in flight than to the rare specimens seen dead). Malkin's papers on the ethnozoology of the Seri, Sumu, and Cora (1956a, 1956b, 1958) are unusual in the attention devoted to the higher level taxa in the folk taxonomies, and to native knowledge of such subjects as the sex differentiation, development, and food habits of local animals. But Malkin's approach is to evaluate ethnozoological knowledge in terms of scientific zoology—to see whether the distinctions and characteristics known to scientific zoologists are locally recognized—rather than to investigate the nature and principles of the local systems of zoological knowledge. An early work which exhibits some of the same merits and faults, and is still worth attention for its detailed description of a system of ethnozoological knowledge, is that by Henderson and Harrington (1914).

Studies of any sort in ethnozoology are rare. There are, however, hundreds of publications relating to ethnobotany, which are emic or ethnoscientific in varying degrees (again, an unusually sophisticated early example must be credited to Harrington: Robbins, Harrington, and Freire-Marreco 1916). Despite the importance of the domain, the previous interest in the topic, the usual explicit taxonomic structure of the terminology, and the relative ease with which names may be tied to specimens and 'translated' into scientific terminology, there is only one full-scale ethnoscientific investigation of ethnobotany: that in Conklin's dissertation on the Hanunóo (1954). Some of this material as well as ethnoscientific analyses of many related domains appears in his monograph on Hanunóo agriculture (1957). But the analysis of the Hanunóo classification of plants together with the corpus of terms on which it is based, and a great deal of material on the significance of plants in other areas of Hanunóo culture, remain unpublished. Some illustrations taken from this

research are presented in a methodological context in Conklin's recent paper on folk taxonomies (1962a).

Frake's ethnoscientific treatment of Subanun disease diagnosis (1961) is important as a demonstration of the utility of the new methods in a quite different domain, but even more so for its emphasis on ethnographic analytic techniques. The system is a multilevel taxonomy. Among the points taken up by Frake are problems involved in the extensive occurrence of the same term at different levels of contrast, the relation between hierarchic levels and sets consisting of stage names, and methods for the discovery of the significant attributes of categories via verbal descriptions (possible because other methods for learning distinctive features here are difficult for both Subanun and ethnographer). To explain why some areas of the Subanun folk taxonomy of diseases are more elaborated than others, Frake advances the general hypothesis that "the greater the number of distinct social contexts in which information about a particular phenomenon must be communicated, the greater the number of different levels of contrast into which that phenomenon is categorized" (1961: 121).

In another ethnoscientific study of medicine, Metzger and Williams (1963a) have investigated several aspects of the roles of Tzeltal curers. Again, emphasis is placed on discovery procedures—in this case particularly question frames. One interesting feature of the Tzeltal situation is that while there are clear criteria for placing curers into two classes, one more highly valued than the other, these are not in general groups with fixed and widely recognized membership; yet the choice of a curer is clearly very important to the patient, and Metzger and Williams succeed in indicating how such choices are made.

The research of Barker and his coworkers in the "psychological ecology" of the "behavior systems" of American and English children (Barker and Wright 1955; Barker and Barker 1961; Barker 1963) converges in several respects with ethnoscience (awareness of the convergence is apparently one-sided: Barker *et al.* do not cite ethnographers or linguists, nor relevant work in systematic biology). These authors emphasize their concern with the inherent segments of the normally-occurring "stream of behavior," as opposed to the artificial "tesserae" into which behavior is segmented in more usual psychological investigations. They are interested in the discovery and analysis of natural "behavior units" and in the classification and interrelationships of such units, the identification and segmentation of their significant behavioral and nonbehavioral environments or "settings," and the relations between these environments and the behavior units. Their requirements for a "natural unit" are less strigent than those of ethnoscience: it is sufficient for them that the investigator does not himself influence the behavior he is observing, and that his segmentation is not entirely arbitrary. It is not required that the units be cognitively salient to the subjects. Thus, speaking of behavior settings, Barker and Barker (1961: 467) write:

> Because the list of settings which we have identified reads, for the most part, like a commonsense directory of a town's businesses, organization meetings, school classes, and so forth, it is sometimes overlooked that their identification involves highly technical operations and pre-

> cise ratings of interdependence . . . . the precise quantitative criterion which we have used to establish the limits of behavior settings. . . . was selected so that the settings would fall within the usual range of laymen's discrimination. Nevertheless, the criteria for their identification are not lay criteria.

Reliability is measured by agreement between observers trained in the special analytical method, and while subjects are observed in detail, their own terminology for units of behavior or environment is not thoroughly investigated and their own perception of units is otherwise deemphasized. It is assumed that there is a "normal behavior perspective," i.e., a normal size for perceived units, which varies between subjects, rather than that the size of perceived units is determined by the environment (with variations in judgments on artificial questionnaires being more the result of varying interpretations of the directions given than of differences between individuals' behavior perspectives). The behavior units are not viewed as a separate system, a folk classification, which is actualized in the stream of behavior. Quite plainly, familiarity with the notions of contrast sets, and of levels of contrast, would be advantageous.

The "behavioral segments" investigated by Richard N. Adams in rural Guatemala (1962) superficially resemble Barker's "behavior units." But Adams' approach is explicitly ethnoscientific, he concentrates on local terminology as an indicator of "reported acts," and says his segments must be distinguished from the preceding and following phases of the continuum by formal attribute differences recognized by the actors. He is concerned also that the classification of these acts reflects the participants' cognition. Adams' early results indicate considerable success in identifying natural segments and their sequential arrangements; he recognizes the existence of unsolved problems in componential analysis (including that of the cognitive saliency of components).

O. E. Klapp (1962) has published an intriguing study of American "social types"—more than 800 "informal" roles which have explicit colloquial names. The domain makes good sense intuitively to an American. Klapp fully recognizes that his social types represent a terminological folk classification specific to American culture, and he approaches a nonrigorous analysis of the semantic components involved. But he provides no explicit description of the boundaries of the domain, his higher level taxa are artificial rather than folk taxa, and he is little concerned with the structural relations between social types. While this folk classification is certainly more "weakly terminologized" than such domains as kinship or "institutionalized offices" (both excluded by Klapp), it seems probable that a more ethnoscientific approach, with attention to complementary distribution and levels of contrast, would show it to be more highly structured and more hierarchical than does Klapp's description.

I stop at this point, in the hope that sufficient examples have been given to illustrate the new approach; however, I do not mean to imply that there are not a number of other studies which are clearly ethnoscientific, and many more which are partially so.

## CONCLUSIONS

It is claimed that ethnoscience is a general ethnographic method. It may be useful to indicate a few of the classical interests of ethnology to which the relevance of the new methods is already quite obvious. The measurement and significance of individual variation among bearers of a culture is touched on in ethnoscientific contexts by Frake (1964), Romney and D'Andrade (1964), and Metzger (1963), among others. Lévi-Strauss (1962a, 1962b) has devoted much attention recently to symbolism seen as the equating and movement between folk classifications in different domains. It seems likely that there are great differences between cultures in the pervasiveness of symbolic or metaphoric equation between folk classifications; the Dogon (Hopkins 1963; Griaule and Dieterlen 1954; Palau Marti 1957:53ff.) and the Ancient Chinese (Bodde 1939) seem to exhibit such symbolism to a higher degree than is indicated by the usual ethnographic literature for most other cultures. Perhaps this is best viewed as one aspect of the interlinking of domains noted by Frake (1964:140); the manner in which these networks may be revealed by Frake's interlinking queries promises to clarify some of the meanings of the concept of function in cultural analysis. Barnett's view of the process of innovation makes particularly obvious the relevance of ethnoscience to the study of culture change. He sees innovation as essentially a process of cognitive reorganization, where innovators substitute an element from one folk classification into another, and this often by a sort of idiosyncratic metaphorical equating of different domains (Barnett 1961; see Wallace 1961:ch. 4 for a critical expansion of this idea). Adams (1962), in a somewhat similar approach to culture change, is examining changes in the formal definitions and the frequency of occurrence of behavioral segments.

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 single 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. The emphases in ethnography will therefore continue to be guided by ethnological, comparative, interests. Some domains will receive more attention than others.

In the present state of interest in cross-cultural comparisons, continued ethnoscientific emphasis on domains such as kinship is assured. Existing generalizations require testing, and new theories require development, by the comparison of ethnographic statements which reveal the relevant structural principles. It is the classificatory principles discovered in ethnography which should be compared, not the occurrence of categories defined by arbitrary criteria whose relevance in the cultures described is unknown (cf. Goodenough 1956b:36–37).

But fuller development of ethnographic method and theory, and also intracultural comparisons to determine the "nature of culture" or the nature of

cognition, the generality and interrelations of classificatory and other cognitive principles and processes within any one culture, both require that the New Ethnography be applied to a variety of domains, not just to areas of much current interest in ethnological theory.

Cross-cultural comparison of the logic of classification requires a great deal more knowledge of the varying logics of different domains in the same culture, as well as better ethnographies of different cultures.

> It is probable that the number, kind, and "quality" of these logical axes [of relations between classificatory categories] are not the same in different cultures, and that the latter could be classed as richer or poorer according to the formal properties of the reference systems they appeal to in erecting their classificatory structures. However even cultures less endowed in this respect operate with logics of several dimensions, of which the inventorying, analysis, and interpretation require a richness of ethnographic and general data which is too often lacking. (Lévi-Strauss 1962b:85–86; my translation.)

Ethnoscientific work so far has concentrated on the sorts of cognitive structure involved in selection classes: the interrelations of categories considered as sets of possible alternatives under varying environmental conditions. Little attention has yet been paid to the methods required for the investigation of the sort of structures involved in rules of combination, the temporal or spatial ordering of co-occurring categories from different selection classes. To understand "how natives think"[6] we need to know about both kinds of structure.

NOTES

[1] In revising the original version of this paper I have profited greatly from the papers, discussion, and criticism presented by the other participants in the Conference on Trans-Cultural Studies of Cognitive Systems. I acknowledge also my debt to many discussions over several years with Harold C. Conklin, Charles O. Frake, Dell H. Hymes, and Floyd G. Lounsbury. Helpful written criticisms of the earlier version of this paper were provided by the editors, and by Conklin (at first from the field), Hymes, Richard N. Adams, Robbins Burling, Wallace L. Chafe, Paul Friedrich, Ward H. Goodenough, and Duane Metzger. I thank them all, and do not intend to commit any of them to agreement with everything said here.

[2] I use the term cultural anthropology to include ethnology (of which social anthropology is one variety) and archeology. Obviously ethnology/social anthropology generalizes and typologizes on the basis of ethnographies, and it is a commonplace that archeology depends ultimately on ethnography for its cultural interpretations.

[3] It is significant that Olmsted in a general survey of the relations between linguistics and ethnology made in 1950 envisaged nothing like the present adaptation of linguistic methods to ethnography.

[4] I well remember once asking my father, a specialist on the taxonomy of the Diptera, how he could so readily identify Drosophila to the species in a glance at his collecting bottle. He replied, "How do you tell a horse from a cow?" The answer may at first seem surprising, coming from one intimately familiar with precisely those characters taxonomically significant for differentiating the species, but the situation is surely quite an ordinary one for biological systematists no less than for others.

[5] Definitions of categories in response to an explicit question about classification may differ from the definitions implicit in the actual conversational use of the same categories. Thus I recently heard my sister's husband refer to my wife in speaking to a friend of his who does not know her; he said, "My sister-in-law is a good cook." I then asked him, "Do you call your wife's brother's wife your sister-in-law?" "No," he immediately replied, and remarked that he had done so "because it was easier than explaining."

[6] The phrase is the translator's title of one of Lévy-Bruhl's books. But of course, as Lévi-Strauss stresses (1962b), "la pensée sauvage" is typical of us all.

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