ETHNOBIOLOGY, COGNITION AND THE STRUCTURE OF PREHENSION: SOME GENERAL THEORETICAL NOTES

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ABSTRACT.—Over the last decade taxonomy has been shown increasingly to be an inadequate representation of the results and devices of classifying activity in folk biology. Its concepts of rank, level and contrast, and assumptions about thought processes, do not exhaustively account for observed behavior, while flexibility and contextual considerations have been under-stressed. Taxonomic approaches, moreover, have tended to produce patterns which reflect method as much as anything intrinsic to the data. This paper summarizes some of these difficulties, and suggests that a methodology focused on the notion of *prehension* may provide the basis for a broader approach encompassing both cognitive and social processes involved in the generation of classifications.

Man and life and nature are none of them domains that present themselves to the curiosity of knowledge spontaneously and passively. [Foucault 1970:72]

In its strict and technical sense, taxonomy is an hierarchical metaphor involving linked notions of rank, level and contrast. In European thought, it finds its first known historical expression in Aristotle, but has since passed via Linnaeus into modern biological usage. As a model of classification it had gained such wide currency by the middle of the present century that a great many ethnographers assumed that it must also necessarily order the folk schemes they were beginning to describe and analyze. This assumption has given rise to the formal theories associated most closely with Brent Berlin and the American school of ethnosemantics (see Ellen 1979b:12-13). In the last decade these theories have come under sustained attack.

The objections have been various and interconnected (eg. Ellen 1979a, 1979b; Fox 1975:118-119; Friedberg 1968, 1970; Healey 1978-79; Hunn 1976, 1977b, 1982—despite Hunn 1975). However, it is helpful to present them under a limited number of headings, even at the risk of appearing to conflate certain matters and separate others that are clearly linked. The headings are: definition, rank and level, contrast, flexibility, context, taxonomizing as a thought process, taxonomic artifacts and taxonomy as theory. In this paper I wish, first, to provide a resume of the various criticisms and then, in the light of my own analysis of Nuaulu ethnozoology (Ellen 1972, 1975; 1976a; Ellen, Stimson and Menzies 1976a, 1976b; Ellen 1978, 1979a, 1985 forthcoming, in press), to outline a broader alternative approach focusing on a concept of *prehension.*

AGAINST A GENERAL TAXONOMIC THEORY OF CATEGORIZATION

1. *Definition.* It has not helped matters greatly that there has been some confusion over what, precisely, is meant by "taxonomy." For some it is no more than a synonym for classification, and in this respect folk taxonomies are seen as equivalent to folk classifications. Thus, Berlin, Breedlove, and Raven (1973:214) use *taxa* to refer to all "linguistically recognized groupings of organisms of varying degrees of inclusiveness." The confusion is compounded by the different meanings attached to the term and its cognates in different European languages. In French, **taxonomie** is broadly equivalent to the narrow definition of "taxonomy" in English; while **taxinomie** (from the Greek

taxis) has a much broader reference. As Claudine Friedberg (1968:315) points out, while the distinguished nineteenth century botanist, A. P. de Candolle, used *taxonomie*, Lévi-Strauss uses *taxinomie.* It is, of course, possible that this distinction is responsible for the different usages of English language writers. Here I have employed "taxonomy" only in its strict Aristotelian sense. That is, in the sense understood by Kay (1971), as a model which owes its form to the Linnaean analogy expressed in set theoretic terms.

2. *Rank and level.* The notion of hierarchy is integral to the taxonomic model found in ethnobiology. Berlin, Breedlove, and Raven (1973:214) pursue the logic further and suggest that taxa of differing degrees of inclusiveness can be placed in a limited number of ranked categorical types (unique beginner, life-form, generic, specific, varietal) and that these characteristically occur at the same taxonomic level. Upon these distinctions and definitions they erect a complex theoretical edifice which is then used to describe particular ethnographic cases.

The notion of hierarchy, at least as it has been applied to entire classifications, and the insistence on assigning categories to different levels, has sometimes given rise to problems which are altogether spurious. The concept of "level," except in a limited and rather crude way, is very difficult to demonstrate beyond particular local regions of classificatory space in particular domains of particular peoples. Similarly, although, used in a loose metaphorical way, all folk classifications are "hierarchic," none are of great depth in any absolute sense (Hunn 1976:509). If we wish to retain some semblance of taxonomic organization we might preferably do so by opting for the tree-diagram model rather than that of a true taxonomy. That is, not by making *a priori* assumptions as to level. Thus in Fig. 1, although it may well be possible to demonstrate "local" contrasts
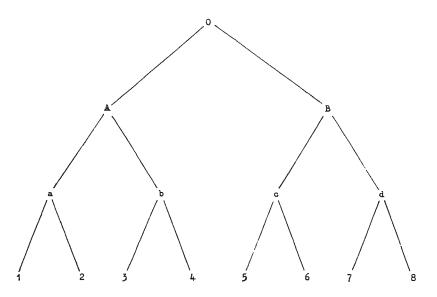

FIG. 1.—Standard abstract representation of a taxonomy. Upper case letter, lower-case letters and figures respectively indicate items at the same *level*. Items at the same level are regarded as being in a relationship of *contrast*. Items at each subordinate level are contrasting *segregates* of a *more-inclusive class* at the next highest level.

(1:2), we cannot always show with the same ease that 1 contrasts laterally with 8 even though 1 and 8 may share the same number of categorical links from 0, as in: 0Bd8, 0Aa1.

The recognition of taxonomic rank requires "inelegant complications of formal taxonomic models" (Hunn 1976:510), provides no basis for either distinguishing induction from deduction, for explaining how taxa or other categories are actually generated, for handling non-transitivity, or for the fact that paired and contrasted items do not necessarily imply more inclusive categories (Brown 1979:794-795; Lancy and Strathem 1981:78). I summarize some of the reasons why this should be so in the remainder of this section.

Hierarchy is not the only metaphor that can express inclusivity in the relations between categories, and there are many ethnographic demonstrations of this. Notions of "broadness" and "narrowness" may, for example, be more appropriate. Moreover, there appears to be a curious notion that classificatory space is consistent with the dimensions of a type of two-dimensional graphic representation which, historically and ethnographically, is of limited extent. Indigenous conceptual arrangements and their linguistic expression may be least violated by employing other means of graphic representation: networks of focal points around which categories cluster, Venn diagrams in two or three dimensions, "sphere of influence" models, or "type-token representations (Bright and Bright 1965; Ellen 1979a:354-357, 1979b:12-14; Friedberg 1970; Tyler 1978:278-279). Hunn (1976:515) has employed the notion of a system of differences in "classification space." Distances within this space are assessed in terms of overall similarity and difference between organisms with respect to perceptible attributes of morphology and behavior. Categories are defined by reference to patterns perceived within this system of differences.

3. *Contrast.* The model of taxonomy employed by Berlin, Breedlove and Raven (1973:214) requires that categories at the same level be mutually exclusive and contrasting. This ethnosystematicist notion developed in conjunction with componential analysis, which has proved inadequate for defining such concepts (Hunn 1976:509; cf. Turner 1974:16-17). Folk biological categories in general are not defined by reference to verbalizable feature contrasts, let alone single characters. They are semantic primitives, at their lower levels generated by induction (Hunn 1975:313, 1976:515). In some classifications not even the notion of contrast need be present, at least not in the sense that we normally understand it. Thus, rather than something being x or y it is common for it to be more x than y, or more y than x. The idea of gradation eliminates distinct boundaries and has been termed by Lakoff (1972) "fuzziness." Thus we are invited to speak of "shrubness" or "treeness," "birdness" or "snakeness" (Randall 1976:549-551). The vagueness can sometimes exceed even such fuzzy expressions as these, especially in the form of such hedges as "well, it might be," "it's a sort of bird" (where the stress is on "sort" rather than "bird"), and "may be." All of this fits well with the idea that the prevailing relations between categories are through polythesis.[1]

4. *Flexibility.* The problem of applying notions of hierarchy and contrast consistently lead us to suggest that taxonomies (and particularly those which are imputed to rest predominantly upon morphological distinctions) have often been assumed incorrectly to be the only (or if not the only then certainly the primary or dominant) means of classification. Berlin's approach has been strongly and widely criticized for attempting to impose a form of taxonomic rigidity on a cultural apparatus the general characteristics of which are quite antithetical: namely fluidity, flexibility and elasticity (Bulmer 1974:24; Dwyer 1976:442; Ellen 1979b; Healey 1978-1979). For Friedberg (1974:327), there are a multiplicity of systems of reference, and there is always the possibility of disjunction between the separate spheres of nomenclature, identification and classification; or we

might say between "models for" and "models of," or between keys and classifications. It would appear then that taxonomies have to be extracted and created from a much more complex classificatory web of which they are part, and in the process even constructed in the interests of neat presentation. The evidence for the inadequacy of taxonomy has come to light partly through the so-called "special problems," which many have seen as simply the artifacts of method. Examples of these include multiple and inter-locking hierarchies, the employment of radically different principles at different levels, and extra-hierarchic relations (eg. Conklin 1969:50; Perchonock and Werner 1969:232, 234). Thus, to say that a taxonomy has cross-cutting classes is to beg the primacy of the taxonomy itself, especially when such categories are highly significant in cultural terms, however we may agree to measure this. Synonymy, homonymy, polysemy and anomaly, all lead us to question the legitimacy of an approach in which there are distinct ranked levels and clearly-bounded contrasting categories. Covert categories, in so far as they can be demonstrated to exist at all, seem to contradict the very idea of taxonomy (Taylor 1980:273), as does the fact that taxonomic categories appear in many cases not to partition classificatory space exhaustively, leaving "empty" and unlabeled residual regions (Hunn 1976:57-58; cf. Kay 1971:878). And all of this is not simply a matter of empty theoretical or methodological assertion, but arises from the direct experience of ethnographers attempting to represent the actual means by which real people set about employing categories and labels, and the relations between them, in their everyday lives. I have tried to show this myself for the Nuaulu of Seram, eastern Indonesia. Rather than explaining away such complications as "special problems" (peripheralizing them, making them exceptions to the rule), I am inclined to regard them as central and inseparable devices of classificatory thought. In particular, there appear to be considerable options in folk classifying behavior in terms of the degree of elaboration at internal degrees of inclusiveness (Bulmer 1967:24; Healey 1978-1979:362; Majnep and Bulmer 1977:48). The evidence for conflicting arrangements, cross-cutting ties, and so on, is now so overwhelming that it cannot be ignored in any general theory (Bulmer 1972-1973, 1975; Dwyer 1976; Healey 1978-1979:364).

5. *Context.* Trenchant also has been the criticism of taxonomic and allied ethnobiological studies for becoming divorced from the situational considerations of ethnography, of the context in which folk classifying takes place (Ellen 1979b; Martin 1975). Thus, for Friedberg (1974:320), "une classification d'objets naturels n'est indépendante de ces derniers; ils existent en dehors de la perception que l'on peut avoir d'eux dans une culture particulière." The problem with decontextualization is not simply that it isolates classifications from the rest of culture and thus presents us with something which is abstract, but that by shedding extraneous cultural information it presents us with the illusion that knowledge consists merely in understanding resemblances (cf. Foucault 1970:111). In other words, it is a complete reversal of the ethnoscience position in which an adequate description should provide enough information to know how to perform in a culturally acceptable manner. If the decision is made to isolate classifications as formal systems, treating each as a separate universe of discourse after the fashion of a mathematics which can be used for any description, we reduce classification to the realm of technical procedures, and taxonomies can easily be shown to be adequate descriptions of that reality. The elimination of context enables the assumption that what is being examined is, in fact, a formal *system.* Identifying a taxonomic, or any other classificatory system, is just like identifying a religious "system" in a tribal society; it is always possible to isolate it if you want to, but to do so may lead to a complete misunderstanding of its structure, function and position in the social and cultural fabric. Social and cultural anthropologists, of all people, cannot make this assumption (Harrison 1970, 1977).

6. *Taxonomizing as a thought process.* There has been an assumption in much formalistic research that the reconstruction of taxonomic hierarchies provides us with a basis for understanding thought processes involved in classifying behavior, that categories are classified in a particular way as a reflection of how people think. It is now clear that taxonomic classifications play a lesser role in human thinking than has been hitherto assumed, especially outside the area of biological kinds (Wierzbicka 1984:325). Strictly taxonomic categories have often not been distinguished from other types of category, the conceptual relation "kind of" has not been clearly dissected from the referential relation of set inclusion (Wierzbicka 1984:313, 315). But even in ethnobiology there has been considerable scepticism. Randall (1976) has criticized the taxoomic model on the grounds that its assumptions about transitivity suggest incorrectly that elicited taxonomies represent structures involved in memory storage. Rather, he suggests, taxonomic trees are the *result* of classifying behavior, and not the means by which information is stored. It is also now clear that certain hierarchical relations are not transitive, and that complex hierarchies are often a product of the procedures used. Connections between different categories are regularly made using short-cuts, in ways which seem to violate taxonomic reconstructions. Thus, in Fig. 1, the relationship between 1 and, say 6, is commonly not arrived at via the route *aABd* (as would be necessary using taxonomic logic), but often directly or through another level category. We may therefore conclude with Hunn (1977a:12) that, "it is more valid psychologically to describe the classificatory structure as based on non-hierarchical relations of perceived similarity." This is why network and spatial models are somehow more attractive than the undirectional, duodimensional taxonomy. Moreover, as Hunn has pointed out, "taxonomic theory provides no basis for distinguishing induction from deduction in logical thought." He argues that more inclusive categories, such as "birdoid," are determined deductively, while less inclusive categories, such as "sparrow," are determined inductivly (Hunn 1976:510, 519). The evidence for this now seems overwhelming.

7. *Producing taxonomic artifacts.* If folk taxonomies do not reflect the actual psychological processes involved in many of those activities we describe as classifying, it is necessary to ask what it is they do reflect. They were at first thought to reflect a true emic model. This, after all, was the aim of the "new ethnography" of which folk biology has been a noteworthy part. It does, however, begin to look more like an etic model of the emic, and then of a very particular kind, where the analyst has already decided what the model should contain and selected results accordingly, even rejecting some statements on the grounds that they appear to represent the idiosyncratic interests of particular informants.

What they do reflect, of course, to a very considerable extent, is an observer's model of taxonomy, which in its most "tight" stereotypical Linnaean form has been outlined by Kay (1971). The attraction of such a model lies partly in its intrinsic aesthetic appeal, partly in a desire for parsimony, lucidity and rational order, partly in its demonstrable resemblance to some processes in folk classifying, but partly also in its implicit, and occasionally explicit, mimicry of scientific natural historical taxonomy and linguistics. This latter influence is evident both in terms of how the enterprise is phrased, the ways in which data are conceived, in the technical procedures for elicitation, in the formal precision of analysis and in the formulation of the problematic. In some cases, I would suggest that individuals with a grounding in natural history have a hidden bias towards finding "natural" categories, and towards an under-emphasis of variation, with a corresponding stress on the taxonomic approach. One gets the impression that ethno-biological taxonomists are by inclination collectors, and as Bulmer (1974:82) has pertinently remarked:

      . . . almost all collectors like nice, perfect specimens, and derive considerable
      aesthetic pleasure from the ordering *they* impose on them. A danger which the

ethnobiologist must guard against very consciously is that of letting his personal aesthetic judgements override his data . . .

But more generally, while the specific scientific model is certainly the immediate and most obvious source for representations of folk models, it is well to remember that the taxonomic model and its close classificatory congeners is grounded in a special cultural tradition, in which the graphic and written representation of the relations between categories markedly alters, rigidifies and directs their conceptualization (Ellen 1979b).

In addition to the specific conventions of literacy, graphic representation and the scientific tradition, there is the general fact that in all inter-cultural communication (of which ethnobiological ethnography is just one, very specialized, example) all local explanations have to be rendered in a form which is in some sense meaningful to the ethnographer. Although we pride ourselves on our grasp of alternative worldviews and organizing principles, we cannot be sure that we ever known everything that is relevant, since knowledge may be caste in an idiom with which we are quite unfamiliar, and therefore ill-placed to understand. The history of anthropological theory itself provides ample confirmation of this, as successive explanatory frameworks are able to indicate previously unexpected relationships and arrangements in old data. Moreover, within the confines of our own range of conceptualization we are always predisposed (by virtue of our cultural socialization and professional training) to favor one view rather than another.

The attraction of the taxonomic model is that once you have data devoid of contextual considerations (in their widest sense) it is virtually impossible not to put a taxonomic construction upon them. It is easy enough to represent a classification as taxonomically ordered and based entirely on morphological criteria if you *a priori* assert that this is what you are looking for. If you are attempting to reconstruct a native conceptual universe as it applies to plants and animals then you cannot begin by excluding categories and arrangements based on non-morphological criteria since informants do not, in the coursee of their ordinary lives, necessarily make such discriminations. In my experience people do not regularly make judgements which suggest that they operate with an all-purpose classification which is recognized as being in any way separate from, or different to, classifications organized in some other way.

As soon as you begin to exclude certain categories and arrangements from consideration then, of course, you begin to yield regularities which look much more like contrasting, hierarchically-ordered and ranked taxa. Thus, the unambiguous all-purpose, morphologically-based taxonomy is something which the ethnographer or linguist *extracts*, but even then seldom perfectly. What is extracted may serve as a basis for establishing the existence of universal processes, schemes of categories, but equally it may do no more than reflect an artifact created by common techniques of extraction and representation. What is more, the very character of taxonomizing as a process generates anomalies.

To summarize: many of the principal data-production techniques employed in cognitive anthropology and ethnoscience tend to provide us with taxonomies and their associated anomalies (see also Gardner 1976). Formalized question and answer frames to a large extent determine the outcome (Frake 1980 [1977]:49), and constrain otherwise articulate informants. Informants, unprompted, rarely in the course of their ordinary lives will use expressions such as "is x a kind of y," or "how many kinds of y are there?" Culturally inappropriate questions, assumptions as to the existence of unitary correct representations, are commonplace. We have all experienced severe frustration in attempting to answer correctly and honestly multiple-answer questions or public opinion poll style survey questions. But in addition to this, techniques such as card or slip-sorting, the drawing of tree diagrams and other techniques which mechanically encode an implicit or explicit assumption to dichotamize successively, will unfailingly

produce formal hierarchies, while eliciting definitions ruthlessly erases any fuzziness between categories as ordinarily used. It is only possible to approach the essential cultural reality of categories if techniques are *for the most part* basically non-directive, that is much looser and more varied (see Perchonock and Werner 1969:236-238). Hence, in my own work I have increasingly relied on simply listening to people talking about animals, using prompts which are less inclined to force the data into formal taxonomies.

I am particularly troubled that some scholars should believe so readily that taxonomies are incontravertibly "in the data," that they emerge "from the data as a consequence of their *natural properties*" (see Lakatos and Musgrave 1970:98). The taxonomic view of classification is in this sense like Lévi-Straussian structuralism, in that if you try hard enough it is possible to discern the kind of order you are seeking, wherever you wish. My field data (by which I understand what was written down in my notebooks, on cards, on specimen tags, in photographs and drawings, and recorded on magnetic tape) were obtained through a mixture of directive and non-directive methods. In this form they consist of fragmented, sometimes contradictory statements, and have to be "processed," transformed into clear generalizations, testable hypotheses and descriptions. There is nothing *in* the data in this form which would suggest that a distinction between morphological and non-morphological characteristics, or between taxonomic and non-taxonomic processes, is justified, or that there exists something called "the Nuaulu classification of animals," which is conceived of as some kind of structured totality. This is a construction which I have placed upon the data. Certainly, one of the properties of the Nuaulu data (as I have presented them) is that many permit a taxonomic construction; but they may permit others as well. The "natural" properties referred to are ambiguous and the term contentious. I would wish strongly to resist the kind of empiricism which uncritically sees taxonomies as simply facts out there waiting to be collected, like so many herbarium specimens. Such an approach begs crucial questions in the understanding of classification, and (ironically) its general application has only been possible through the systematic neglect of the full range of factors at work in "classifying." In the highly particular social world of professional biology the principle of "taxonomic rigidity" is an important working assumption; in the context of ethnobiology it has become simply dogma (Healey 1978-1979:379). But you cannot work from taxonomy as if it provided a set of axioms (Hunn 1976:510).

8. *Taxonomy as theoretical icon.* A final problem, and perhaps a factor explaining the tenacity with which its practitioners defend the taxonomic approach, is linked to the fact that it is not simply that taxonomy and formal elicitation has become indelibly linked to certain kinds of substantive investigations, but that "taxonomy" has become central to, even to emblazon, particular theories of culture (Conklin 1969), conceptions of ethnography (Spradley 1979), and models of thought processes (Bruner, Goodnow and Austin 1956). In other words, it has become reified; to some extent at least because it provides a universal model to counter cultural relativism (Brown, Kolar, Torrey, Truong-Quang and Volkman 1976). Curiously (and paradoxically), the approach is based on the rigorous definition of cultural boundaries in order to provide the basic units for the construction of pan-human hypotheses. However, this notion of the boundedness of cultures is linked to an organic model and crude functionalist assumptions, without being genuinely sociological, systematic or contextual. This becomes readily apparent if it is compared with the treatment of classification in, say, historical linguistics and philology. In the latter, cultural boundaries disintegrate, and diffusion and historical explanation rule, and the patterns themselves *are* contingent upon particular social and cultural processes.

BESIDES TAXONOMIES

While we may agree that attempts to define, rigorously, the principles of classi-
fication and nomenclature in folk biology are certainly useful for particular ethnographic
populations, the making of inductive generalizations about certain types cross-culturally
assumes that variability is according to a limited number of well-understood criteria along
parallel axes. The restricted check-list approach exemplified by the work of Berlin and
his associates cannot, then, cope with the wider dimensions of variation between systems.
It not only tends to reify a particular kind of classification (that which we call taxonomic),
but seems to claim that a large number of semantic fields are at all times similarly
organized. "Taxonomy has been elevated to an artificially high status as *the* mode that
humans employ to organize and act upon discrete elements in the environment" (Rosch,
Mervis, Gray, Johnson and Boyes-Braem 1976). It is compelling because it is a stylish
(p. 790) representation of relationships among natural elements and because taxonomizing
appears to us as an efficient strategy for organizing, storing, and retrieving elements
(especially words) in memory (see eg. Ericsson, Chase and Faloon 1980). Even if we agree
that the taxonomic mode typifies Western culture (and I am not convincd of this either),
we cannot assume that all cultures have exactly the same formulations of resemblance,
relationship, class or contrast (see Hobart 1982:56). Nevertheless, many field researchers,
sceptical of the claims for an all-embracing taxonomy in human categorization of nature,
have reasonably argued in favor of a "limited natural taxonomy," or have found
taxonomies a convenient descriptive framework (Ellen, Stimson and Menzies 1976b;
Taylor 1980:285).

We may agree that, as one available common process, taxonomy is universally
available in the classifying repertoires of all people. It is, however, more important
in some societies than in others (see also Super, Harkness and Baldwin 1977), and although
the taxonomic mode is useful in describing the structure of some systems it is not
entirely adequate (Hunn 1977a:13), and in some cases limited to particular domains (Lancy
and Strathern 1981:780). Thus, taxonomy works quite well for Nuaulu vertebrates (and
among vertebrates for birds and reptiles), but it works less well with mammals,
invertebrates and fungi. Elsewhere, although the Tobelorese use non-taxonomic features
they appear to prefer taxonomic structure, especially "below" basic terms (Taylor
1980:276-277).

Using procedures for testing taxonomic thought (see Bruner, Olver and Greenfield
1966), and using tests to reflect presence or absence of taxonomic thought, it has been
suggested that there is a close correlation between the tendency of children to employ
taxonomic-like strategies and the degree of depth and complexity of folk taxonomy in
a language (Lancy and Strathern 1981:774). Work undertaken by Lancy and Strathern
in two New Guinea populations suggested that Ponam children improved their taxonomic
thought with age whereas Melpa only improved with education (p. 777). "Melpa lessons
are in English and advances in learning names of things (as opposed to categorizing) in
mother tongue should be associated with general improvement in fluency" (p. 778). Melpa
appear to mute a taxonomizing tendency as socialization advances (p. 778) and other
modes of representation are employed; in tests pairing was interpreted as blocking
taxonomizing. In the case of the European tradition, the taxonomic approach is firmly
linked to the development of literacy and scientific culture. So, where literacy has even
just a toehold (as among the Tzeltal) it cannot but help encourage taxonomic expression.

Thus, two things become abundantly clear: firstly, that the cognitive and linguistic
constructs employed in classifying are varied and combined in different ways in different
cultures, and, secondly, that the ways in which they are employed are exceedingly
flexible. In addition to hierarchic class inclusion, folk biological classifying activity
involves indices, keys, paradigms, typologies (Conklin 1964:39-40), non-hierarchic binary

opposition and pairing (Brown 1979:794-795; Lancy and Strathern 1981:782), simple dichotamous division, and possibly other forms as well (Tyler 1978:290). We must also be prepared for categories to be expressed in different ways on different occasions in different places. Thus, pairing may involve a wide variety of principles and these will be realized differently in different cultures: difference, duality, complementarity, alliance, hostility, equality (Lancy and Strathern 1981:788). Similarly, pairs may be elaborated to form more complex constructs, analogies, series of paired opposites, series of vertical similarity, alternation, and more complex symmetries. These basic relations in ordering social classifications are well-known (Needham 1979).

Moreover, items may be assigned to different categories, arranged in different ways, according to different principles, depending on context. In other words, culture *enables* various forms of alternative orientations, organizations and actions; culture in this sense is a tool-kit (Salzman 1981). Friedberg (1971) has shown this for Bunaq plant categories. For the Nuaulu, it occurs not only in terms of the allocation of certain salient terminal categories to more inclusive ones (Ellen 1975), but also in the identification of the content of terminal categories themselves (Ellen, forthcoming). How these different modes are employed will vary. Taxonomy will, in certain cultures, be dominant. In some cultures styles of classification may vary between domains, which may have their own special organizational structure; elsewhere they may vary according to situation.

UNDERSTANDING THE GROUNDS FOR PREHENSION

Rather than documenting taxonomies or other kinds of classifications and categories as so many butterflies (Leach 1961:2), it is necessary to focus upon the processes which generate them; not detached cognitive processes, but those rooted in particular situations. To distinguish it from the arid abstraction of the notion of classification, we might call this *prehension*. Prehension refers to those processes which through various cultural and other constraints give rise to particular classifications, designations and representations. What results depends on the input at all stages in the process (elicitory techniques, etc.) and the interaction of various factors. Prehension stresses the situational bias of classification, whereas *cognition* and *perception* suggest purely cerebral processes. Indeed, classification itself may be deemed too narrow a definition of what is involved (Reason 1979), and too easily ends in psychological reductionism, in a discussion of states of mind. Prehension recognizes, without the necessity of qualification, the difficulty of distinguishing mind from matter, thinking from doing or speaking, individual from group, cerebral from social, natural from cultural. Thus, prehension entails individual acts of perception, but is not (and cannot be) confined to them. We can only begin to approach a realistic understanding of classifying behavior if we begin by observing people assigning items to categories and using names in natural ethnographic settings, as well as experimental ones. Moreover, if we use experimental techniques which we might reasonably expect to produce particular results, we should try also to devise other techniques which might produce other, different, results.

The structure of prehension is as follows. People bring to situations in which classifying activity takes place, and from which verbal statements about classifying behavior result, information of diverse kinds acquired through both informal and formal socialization experiences of the world in general and of earlier classifying situations. How they then classify depends upon the interplay of this past knowledge (including prescriptions and preferences with regard to particular cognitive and linguistic idioms) with the material constraints of the classifying situation, between conscious and subconscious, the purposes of the classifying act, and the inputs of others. Thus, thinking, saying and doing are not separate activities but inter-penetrating ones, while cognitive bricolage provides us with both models "of" and models "for" (in terms of

Geertz's distinction (Geertz 1966).[2] Practical problems do not exist on their own, and in a very real sense all classifications are, therefore, "practical" (see also Hunn 1982).

There is a further important aspect of prehension. This arises from the fact that the processing and storage of information in the mind is imperfect, and communication of that information less perfect still. Paradoxically, there is a connection between this short-coming and the considerable capacity of the human mind to re-order information in different ways, replacing irrelevant information with that of greater and more immediate utility. That classifications are messy, cross-cutting and changing is a reflection of this. Consider also the paradox that while the human mind always strives for order, the reality it deals with is so complex that it can never fully attain it. Concepts are often used, operationalized, without defining them. On the other hand, however, in order for communication to take place, classification must have at least some intersubjective structure, some agreed cultural rules, some "doxa" (Bourdieu 1977).

Prehension is an inherently social process. Classifying activity may be solitary or inter-personal; that is we may wish to communicate not simply with others but with ourselves as well. But even solitary behavior is modeled on that hypothetically occuring between individuals. In a solitary classifying act there is no communication with others, but, nevertheless, thought usually takes the form of linguistic expression, though not necessarily verbalized. The problem is that at the point of transformation into the lexical output of language there is a fundamental simplification of cognitive and semantic relations. Linguistic expression necessarily entails both *decomposition* and *facilitation*; decomposition because language faculty is unable to encode at one instance the totality of informational relations in the mind, and facilitation because that same complexity must be translated into a form which makes communication and expression possible. It is, if you like, the same as translating from the machine code of a computer, designed for the internal organization and manipulation of information, to a program-ming language. Thus, it is impossible for a sender to communicate everything to a receiver. We must, therefore, distinguish the intended message from the outward signs of the message and it is these latter which will vary depending on who the recipient is. Receivers will encode the message, not in terms of the intentions of the sender, but in the light of the recipient's own expectations and knowledge. Sender A may say "X is a kind of Y," based on the unverbalized information that X and Z are types of Y, although recipient B (with no knowledge of the Y-Z relation) may interpret this statement, through his or her own experience as a fourth item W, to imply that X (like W) is a kind of Y. There is, then, a degre of ambiguity, and people must interpret and operate with respect to the codes and outward signs of others without knowing what inner processes are taking place and the information which generates them (Wallace 1970). We may, therefore, agree (with Reason, n.d.) that "linguistic utterance is not, generically precise at all; it is generically *sufficiently* precise." Interpretation depends upon whether the person of the same culture interacting in a particular activity will be different from that of a member of the same culture but one who is not prepared by previous mutual experience. The interpretation of a non-member of the culture will be different yet again, and that of the specialist ethnographer especially so. All this will affect the degree of possible ambiguity. Interpretation will also depend upon the questions or commands of the interlocutor (recipient). For example, an individual may get from A to B according to a variety of ad hoc conscious and unconscious procedures, and certainly without the use of any mental map. On the other hand, if asked to draw one *post facto* there may be no problem, even though the map had no bearing on the original decision. As Reason (Reason, n.d.:7-8)(see also Crick 1976:159) has pointed out: "ambiguity, ambivalence, metaphorization, are not peripheral and arcane aspects of language use, but central and essential . . . It is hypostatized, reified classificatory usages which require special social conditions to obtain." What this implies, and what I have tried to focus on here, is

classifying (as an historically situated activity) rather than an emphasis on classifications. "Classifications," says Reason in continuing the passage just quoted, "as such are, if at all, only derivatively meaningful." Indeed, formal representations may be "positively misleading, for they purport to, but cannot incorporate the grounds of such interpretations" (cf. Tyler 1978:290).

THE COGNITIVE ACQUISITION OF THE INSTRUMENTS OF PREHENSION

The dynamics of prehension cannot simply be understood in terms of the interplay of factors at the instance of classification or verbal expression. The outcome depends on the life experiences of the classifier: learned cultural behavior, personal experience, adaptation and individual socialization. In this respect it is important to acknowledge the significance of materiality and the child's acquisition of that materiality. Thus, in the early development of every child, bodily discovery, experience and perceptual salience will determine a cultural dominance of *front* over *rear*, *above* over *below*, *hands* over *feet*, and so on (Clark 1973). Linguistically, the second item in each of these pairs will be accordingly marked. But not only does materiality affect the handling of knowledge, but the experience of time also. Thus, the *past* appears to markedly dominate the *future*. This must be is so in two senses. First, past experience has cognitive priority and is only displaced through repeated contrary cases. Secondly, temporal ordering itself serves as a basis for serial signification (as in the contrast set *older:younger*, or in birth order names).[3] Thus, overall, the dominant cognitive relations brought to prehension are material, historical and biographical, rather than non-material and contemporary.

In addition to such processes ingrained in early socialization, we must add the linguistic and classificatory idioms resulting from cultural convention, but what we must not then do is simply to accord to the mind a mechanistic model. The mind itself organizes information extensively in terms of paradigms, and is an active rather than a passive system in which images are connected and constantly transformed. What is certain is that unless there is clear and explicit cultural evidence for a total unitary classification of animals, it is as unlikely to be generated subconsciously in the mind as it is to be a logical consequence of the structure of language. For Ardener (1980) "it will be no wonder if we cannot sometimes tease out in real life whether we are dealing with a "social" or a "linguistic" phenomenon. Language is to the social as a measuring rod is to the measured, where, however, the inches or centimetres stretch or contract at the same time as the object itself deforms in related or independent directions."

SEMANTIC UNIVERSALS AND CLASSIFICATIONS IN SOCIETY

Once we have understood the process of prehension, and the degree of predictability as to its outcome among particular populations, we can return to the level of societal generalization and cross-cultural comparison, and at once the debate between universalists and relativists is seen as the caricature it inevitably must be; an entirely false opposition sustained through ideological mystification and polemic (Ardener 1982:3; see also Ellen 1979b; Hollis and Lukes 1982 (particularly Gellner)). Classifying behavior *does* reflect social organization, but the degree to which we can discover close correlations will depend upon the constancy in application of a particular mode. One possibility is the employment of grid-group forms of analysis pioneered by Douglas, 1982. I have elsewhere (Ellen 1979b) attempted to list the mix of variables for any given society that is likely to affect the structure, content and function of classification. It is possible to demonstrate correlations between pairs of characteristics: between division of labor and classificatory complexity, literacy and arbitrariness, semantic field integration and social

integration, and so on; linking the formal properties of particular classifications with the substantive ones of the societies in which they are found.[4] If we could eliminate all other variable we might reasonably expect such *horizontal* pairs to show a regular correlation. The problem for the anthropologist, and one reason why I am suspicious of attempts to seek constant macro-relations between classifications and types of society defined in terms of vague and general criteria, is that it is not always possible to find predictable regularities in the *vertical* relations between variables. For example, literacy does not always accompany hierarchy, while rigorous expression of inclusiveness may eliminate anomaly as well as generate it. The pattern observed and the extent to which particular pairs of correlation are evident depends upon the entire nexus of variables.

The attempt to generate a neat concordance demands care, since for one thing it seeks cultural generalizations on the basis of very limited information about the behavior of individuals. Since, as I have indicated, the process of prehension operates through individuals in the context of collective social experiences, it can hardly be expected to coincide with statistical generalizations or necessarily reflect what is culturally dominant. I detect a confusion of the individual with the collective level in work in the taxonomic tradition, in which culture is assumed to be some mythical omnipresent speaker-hearer, both the sum of its component individuals and a constant from which we might infer the classifying behavior of individuals.

On the other hand, we cannot deny that universals can be extracted, though their character must be subject to considerable qualification. Brown's universalist-evolutionary arguments (eg. Brown 1984) have, for example, been severely criticized, along with his presumption of a definitive, monothetic set of "life-form" categories specified in terms of content and structure which exclude non-morphological groups. Such categories are polythetic, structurally diverse, and much more likely to involve special purpose significata; as categories become more general so they become more cultural, less biological (Ellen 1977; Hunn 1982:12-16; Randall and Hunn 1984). Similarly, the detailed wiring for some aspects of color classification sheds little light on how basic naming principles shape language, and it seems unlikely that detailed neural specifications will find much of a place in explanations of language universals. However, it is possible to detect apparently universal ordering principles underlying the character of lexica: conjunctivity (including binary opposition), criteria clustering, marking, and dimension salience (Witkowski and Brown 1978:443-444). It appears that a "rich cognition" model, one permitting both the intrusion of general underlying principles and possibly domain-specific ones, is warranted by the little evidence available. But while such a model is attractive, our attempts to tease out convincing domain-specific semantic universals, other than for color, have not yet met with much success. So, rather than stressing the patent substantive invariance of semantic universals through formalism, it is equally important to stress isomorphic patterns, that is the latent relational aspect (Bateson 1973:615; Lévi-Strauss 1966). It is this, rather than the former, which accounts for the feasibility of cross-cultural communication, and the substantive semantic continuities which give rise to the very real problem of misunderstanding at the level of close interactions between individuals. This is so much the case, suggests Ardener (1982:4), that it is itself a human universal. Thus, before we can, with confidence, make claims for the existence of semantic and lexical continuities, it is first necessary to consider the *limits* of cultural discontinuity. In other words, the formalists have got it the wrong way round. Rather than making *a priori* essentialist assumptions (wrapped up in positivist methodology and ethology) which limit the appearances we decide to subject to analysis, we should instead follow the practice of Bayle (Flug 1971:5), who in his *Dictionnaire historique et critique* sought universals which encompassed all conceivable appearances, including the most obscure and atypical.

NOTES

[1]One of the most barren attempts to employ notions of hierarchy and contrast with respect to a particular domain must surely be Stark's (Stark 1969) analysis of body parts. In a domain (the body) where classifying procedures are necessarily analytic rather than synthetic owing to material continuity of the parts (see Ellen 1977b), the notion of level becomes absurd and entirely arbitrary. Thus, in what sense does "face" contrast with "knee" at all, and why should it not contrast with "head"?

[2]As representational models are not neutral in their relation to action, it might even be argued that the notion of representation should be avoided altogether. Moreover, Geertz's distinction is a product of a literate tradition in which representational models and plans for action are more obviously separate.

[3]I am grateful to Kevin Durkin for pointing out to me that the dominance of past over future is controversial among developmental psychologists and philosophers of time. Thus, we must contrast a "moving time" view, in which ego is static and time passing by, with a "moving ego" view. There is also some disagreement in the literature as to which of, say, "before" and "after" should be said to be the marked term. Nevertheless, in mundane classificatory events, as in much individual interaction and subsistence decision-making, practical experience (of the past) on (future) action is crucial.

[4]For a specific ethnobiological example see Dwyer, 1979:19, 25.

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