phonetic ability. We had not completed our studies of fossils like Skhul V, Steinheim, and Rhodesian man, but it was apparent that they did *not* resemble "classic" Neanderthal man. We weren't providing ourselves with an "out," as Carlisle and Siegel insist. We were exercising reasonable caution. We have since reported (Crelin 1973; Lieberman 1973) that Skhul V had a supralaryngeal vocal tract which would *not* have placed any anatomical restrictions on producing the full range of human speech. Rhodesian man seems to represent an intermediate case. Our interpretation of the results is that there existed at least two classes of fossil hominids. Some like Skhul V appear to be very close to modern man. Others, like La Chapelle, appear to be members of side branches that are now extinct. These conclusions are, in part, consistent with Howells' analyses.

To conclude, we do not claim that we have "solved" the Neanderthal problem, but we think that we have formulated a productive scientific theory, one which relates facts that hitherto were thought to be unrelated.

The theory bears on:

(a) The affinities that exist between newborn humans and classic Neanderthal. These similarities explain why so many of the anatomical features that characterize Neanderthal man sometimes occur in adult modern man. The rate and nature of ontogenetic development is not uniform in the "normal" human population.

(b) The evolution of the human skull and mandible. The affinities that exist between Neanderthal and newborn have enabled us to reconstruct the supralaryngeal vocal tracts of extinct fossils. This has provided an answer to a long-standing and puzzling question: what are the functional characteristics of the specializations of the human skull? We think that they reflect, in part, adaptations for articulate human speech.

(c) In other words, we think that natural selection for human speech was as important a factor in the late stages of hominid evolution as bipedal posture was in its early stages. Neanderthal man seems to have been a side branch that specialized for other functions.

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Unique Beginners and Covert Categories in Folk Biological Taxonomies

CECIL H. BROWN
Northern Illinois University

Berlin, Breedlove, and Raven (1973:214-242) have presented several hypotheses concerning folk biological taxonomies that promise to "throw considerable light on prescientific man's understanding of his biological universe" (1973:214). In my opinion, their most important observation—clearly supported by

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my own work (Brown 1972) among Huastec speakers of Northern Veracruz, Mexico—is that "nomenclature is often a near perfect guide to folk taxonomic structure" (1973:216). Indeed the latter generalization and several others made by Berlin et al.—with the exception of the two to be discussed here—almost certainly will be verified many times as research in the area of folk biological classification continues.

Berlin et al. propose two general features of folk biological classification I believe to be gratuitous: (1) that taxa of the category "unique beginner" can be and most often are linguistically unlabeled (1973:215), and (2) that "covert categories" can sometimes be found as intermediate taxa in folk taxonomies (1973:216). "Unique beginners" are the most inclusive biological taxa (1973:215) in which all other biological taxa are included (1968:290). Berlin et al. offer the taxa labeled in English *plant* and *animal* as examples of unique beginners (1973:215). "Covert categories" are biological classes or taxa that are not linguistically labeled (1973:216).

The claim for unlabeled or "covert" unique beginners lacks empirical support. Berlin et al. admit that the number of societies whose principles of biological classification have been studied is so small as not to permit significant comparative inferences at present (1973:214). I agree, however, that the evidence is abundant enough at some levels of taxonomic description to make generalizations concerning some principles of classification and nomenclature, but this is not so at the highest level where the unique beginner—and only the unique beginner—is found.

Berlin et al. have undertaken an extensive study of folk biological classification in Tzeltal, a Maya language of Chiapas, Mexico. Tzeltal plant taxonomy described by them in several places (1966, 1968, 1973, n.d.) lacks a linguistically labeled unique beginner equivalent to the English taxon *plant*. Even so, they claim that "the plant domain for the Tzeltal, though not named as such, is unambiguously bounded and distinctly defined" (1973:219) and, as Berlin et al. imply, represents an explicit nonlabeled taxon belonging to the category unique beginner. One of the bases for this claim is that Tzeltal speakers can use numerous expressions "to contrast any one member of the plant world with a member of some other domain, for example, animals," one such expression being "plants 'don't move,' . . . while animals do" (1973:219). There is an important logical fallacy here. That *specific* plants can be contrasted with *specific* members of other domains is no logical ground for assuming that *all* plants as a class are or will be contrasted with *all* members of another domain as a class.

A more formal linguistic argument for the existence of a Tzeltal unlabeled taxon corresponding to the English taxon *plant* is that plant words occur with their characteristic numerical classifier *tehk* and animal words with their characteristic numerical classifier *koht* (1973:219). The same argument would seem to have equal force here: that words for specific plants take the same classifier is no reason to assume that *all* things named by words occurring with the classifier are thought of as some sort of *class* or *category* contrasting with some other class or category. The question begged by Berlin, Breedlove, and Raven's latter argument is, "Does this syntactic feature have anything to do with *taxonomy* or, in other words, with *named categories of things that are hierarchically juxtaposed with respect to class inclusion?*"

The implication of Berlin, Breedlove, and Raven's ubiquitous "covert" unique beginner is that people globally, if not universally, conceptualize living things in terms of the dichotomy "plant-animal." The point is that people everywhere can make the distinction and often do, but that does not mean that they make it taxonomically. (If the distinction is not made taxonomically, it may often be made *specifically*, e.g., in the manner of the Tzeltal who make the distinction by contrasting specific plants with specific animals.) In view of the lack of empirical support for their hypothesis, Berlin et al. are simply being ethnocentric, i.e., they make the taxonomic distinction, so hence everyone else must too.

A different approach is needed to argue the gratuitousness of the authors' "covert categories" found at intermediate levels of folk biological taxonomies. Covert categories in Tzeltal plant taxonomy were originally

hypothesized because the taxonomy possessed only a few midlevel taxa or categories (cf. Berlin et al. 1968:291) resulting in large undifferentiated groups of specific taxa. To test this hypothesis Berlin et al. had Tzeltal informants perform a number of sorting tests (1968:295-296). These tests revealed a certain amount of subcategorization or subgrouping of plant specific taxa strongly suggesting the existence of covert or unlabeled midlevel categories in a folk taxonomy.

One of the tests employed by them (1968:293) is the *triads test* that "requires informants to specify which item in a set of three [in their application items were Tzeltal plant names written on sheets of paper] is 'most different' from the others." The triads test, which is run for all possible triads in a set of terms, clearly reveals unlabeled subgroupings of Tzeltal botanical categories.

Berlin et al. (1968:296) claim that unlabeled groupings are not "generated in terms of culturally irrelevant oppositions of [their] own invention." I believe that while this may be true of some unlabeled groupings, it is probably not true of most of those revealed through sorting procedures like the triads test. Such tests often present informants with culturally irrelevant options coercing them to sort items together which they rarely, if ever, group together on an *ordinary day to day basis*. Such groupings can hardly be regarded as culturally relevant.

An important aspect of the Berlin et al. argument is that biological entities are assigned to taxonomic categories on the basis of morphological similarities. I do not deny that informants subjected to sorting procedures often group items together on the basis of shared perceptual properties. If asked to sort together on the basis of similarity two of the three symbols "x," "d," and "b," I would choose "d" and "b." I do not, however, ordinarily make this sorting, and the fact that I do so sort them has nothing whatsoever to do with my ordinary perception of things or, for that matter, with named categories of things hierarchically juxtaposed with respect to class inclusion.

Berlin et al. (1968:292) also note that informants' comments on plants in the field were important checks on the cognitive validity of unlabeled groupings of taxa revealed through sorting tests. These were descriptions of certain plants as "food, herbs, firewood, and so on" (1968:292), and as such refer to "cross-indexing" of plants under categories unrelated to ethnobiological taxonomy proper. Consequently they do not reinforce arguments to the effect that unlabeled groupings are found in botanical taxonomy at intermediate levels. On the other hand, such comments may indicate that many unlabeled groupings are in reality not covert after all, i.e., that their taxa are cross-indexed under some nonbiological labeled categories.

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Further Notes on Covert Categories and Folk Taxonomies: A Reply to Brown[1]

BRENT BERLIN
University of California, Berkeley

In Berlin, Breedlove, and Raven (1973:214-242), we present several hypotheses concerning the nature of folk biological classification and nomenclature. Cecil Brown (1972) questions two of these hypotheses, both of which pertain to the nomenclatural peculiarity for taxa of the

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