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**comment on Cecil H. Brown, The growth of ethnobiological nomenclature**  
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**Citation**

Schadeberg, T. C. (1985). comment on Cecil H. Brown, The growth of ethnobiological nomenclature. *Current Anthropology*, 14-15. Retrieved from <https://hdl.handle.net/1887/8812>

Version: Not Applicable (or Unknown)  
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**Note:** To cite this publication please use the final published version (if applicable).

more profoundly affected by culture contact than the agriculturalists; they typically live in biotically impoverished environments, such as deserts and polar regions, as opposed to the rich tropical and subtropical environments of many of the agriculturalists studied; they live in smaller social groups than the agriculturalists, often numbering tens rather than thousands; and their ethnobiological knowledge has been less intensively studied than that of agriculturalists. Many of the commentators express suspicion of the criteria used to select cases, while several suggest that if a group of hunter-gatherers were as well studied as were many of the agriculturalists, the results would be quite comparable. These are serious criticisms, but they are largely dismissed by Brown in his reply and are not mentioned at all in this article. Since this article depends on the earlier survey, his failure to address these problems of data quality seriously undermines its credibility.

These problems are compounded by the fact that the important variables in his model are extremely difficult to measure accurately. The demonstration that a folk category stands in a one-to-one correspondence to a scientific taxon requires comprehensive collections of the complete biological range of each of the categories in question. Since one discovers the range of a folk category only through careful study, casual observations (particularly of cultivated plants; see Berlin, Breedlove, and Raven 1966) will be biased toward records of one-to-one correspondence to scientific species. Similarly, as the earlier commentators Hays and Headland point out, the distinction between obligatory and optional binomialism requires careful ethnolinguistic recording. Saliency is the most difficult to measure, as is demonstrated by the admitted crudeness of the approximations that have been employed to date (e.g., Berlin et al. 1981; cf. Hays, CA 26:56-57). Yet in order to test Brown's model, one would need not only synchronic measurements of saliency but also accurate measures of diachronic change in saliency for entire botanical nomenclatures of a number of hunter-gatherer and agricultural societies. Given the strenuous requirements for an adequate cross-cultural sample, I believe that Brown's hypothesis is safely beyond test; studies that meet the standards of data quality required for accurate measures of even some of these variables are extremely rare and are biased toward agricultural societies. However, we do not need these data to evaluate Brown's hypothesis; it founders on internal substantive and logical grounds.

Brown's discussion of generalization versus restriction of reference in the evolution of folk generic categories is, for the most part, a muddled restatement of Berlin (1972:58-65) with one important confusion. An ample cognitive psychological literature demonstrates that members of many natural categories are not alike in their representativeness of the category (Mervis, Catlin and Rosch 1976; Rips, Shoben, and Smith 1973; Rosch 1973, 1975; Rosch and Mervis 1975; Rosch, Simpson, and Miller 1976). For example, though both robins and chickens are regarded as kinds of birds, robins are generally regarded as more typical than chickens. In psychological terms, the category is recognized by extension of membership or generalization from its best example, or prototype. In nomenclatural terms, however, the category label refers to the entire range of potential members. As vocabularies grow and differentiate, the internal structure of the category is explicitly recognized: extensions from the prototype are labeled with the head term plus a modifier while the prototype is often labeled simply by the head term or the head term plus the modifier "true" or "genuine" (Berlin 1972:58-65). Brown treats undifferentiated generic categories as though reference of the category label were restricted to the best example, thereby confounding psychological and nomenclatural phenomena. This confusion underlies his call to relabel terminal monolexemic categories "specific" rather than "generic." In turn, this relabeling of concepts is the most important difference between Brown and Berlin.

Other aspects of Brown's reformulation do not accord with common sense. For example, he claims that hunter-gatherers have at once smaller vocabularies and smaller referential ranges for each of their terms than agriculturalists; this means that a much larger proportion of the biological diversity that they encounter will go unnamed. Unfortunately, he does not recognize or explain this consequence of his position. Again, he claims that as vocabularies expand and differentiate, the categories decrease in saliency; but from this it would follow that the most highly differentiated portion of folk botanical classification, cultivated plants, would be the least salient.

If errors, confusions, and relabeling of concepts are removed from Brown's reformulation, we are left with Berlin's original framework. The fact that, at base, Brown's "reformulation" is virtually indistinguishable from Berlin's framework testifies to the essential soundness of Berlin's insights.

There are problems with Berlin's framework. Some of his expectations have not been met, in particular his notion that generic categories will not include taxa labeled by primary lexemes except for salient type-specifics. Furthermore, it is sometimes difficult to draw a clear line between taxa of different folk biological ranks, and categories of a given rank can vary enormously in their biological range (e.g., generics range in reference from subspecies to entire botanical families). On the whole, however, both Berlin's concept of folk biological rank and his descriptions of the evolution of nomenclatural systems have proved stimulating and useful. The exceptions to his generalizations mainly suggest the complexity of the phenomena ethnobiologists attempt to characterize: much of the variation in folk biological classification systems appears to be the consequence of the confrontation of panhuman classificatory strategies with variable biological structures.

As shipwrecks mark dangerous shoals, the problems in Brown's attempted reformulation should instruct us. Some are inherent in his methods of cross-cultural comparison; others appear to stem from a failure to think through the implications of his position. Until the procedures of ethnobiological research become more standardized and the data base greatly expanded, I believe we can learn more from carefully controlled comparisons of systems than from large uncritical surveys. Progress will depend on more careful measurement and analysis of the great variety of potential explanatory variables mentioned above.

by THILO C. SCHADEBERG

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The kind of research reported in Brown's article is of great potential importance for the method of reconstructing semantic change. Historical linguistics has reached a high degree of sophistication and formalization where the reconstruction of forms such as sounds, words, and sentences is concerned. This contrasts with the more intuitive appeal to plausibility that is commonly invoked when a particular (change of) meaning is reconstructed (see Bynon 1977:62). It appears to be common opinion that replacement, extension, and restriction of a word's possible set of referents are all possible changes (see Jeffers and Lehiste 1979:127-29). The introduction of general principles that would narrow the range of possible semantic changes—at least for ethnobiological nomenclature—would greatly increase the accuracy of semantic reconstruction. We might then hope to overcome the major stumbling block to the application of the "Wörter und Sachen" method for hypothesizing the homeland of speakers of a reconstructed protolanguage.

It is extremely common for a word to have both a more generic and a more specific set of referents. Brown (contrary to Berlin) believes that in such cases the more specific use is al-

ways primary and that this kind of "polysemy" invariably results from extending the set of referents. A documented counterexample (see the *Oxford English Dictionary*) is the history of the English word *deer*: "†1. A beast; usually a quadruped—1481. 2. The general name of a family (Cervidae)." The intermediate stage of "polysemy" is attested by Shakespeare. "Mice, Rats, and such small Deare "

There is also interesting evidence from first-language acquisition where the generic use of a word predates its restriction to more specific uses. A German baby, for instance, may pass through a stage in which its whole universe is classified into *Mama*, *Wawwau*, and *Auto*. I know of a small boy who for a time called a lamp suspended from the ceiling *Engel* 'angel'. This happened shortly after Christmas, when the family had had a Christmas tree decorated with a tinsel angel, and for a time his vocabulary contained an item *Engel* 'suspended, dangling object'.

I believe that this situation is typical for human language. The common type of "polysemy" in which a given word refers typically to its most typical referents but also—if the need arises—to similar referents for which no conventional more generic label exists may well be the primary situation par excellence. Unless we push speakers of other languages to use equivalents of our own popular or scientific classes of referents (e.g., "banana," "kangaroo/wallaby"), we shall probably find it very difficult to distinguish the first two stages that Brown postulates for the emergence of generic/type-specific polysemy (in both patterns).

More specifically, I would like to challenge Brown's statement "Wood/tree polysemy is invariably the result of extending a term for a high-salience referent, wood, to a much lower-salience referent, tree." In Bantu, where the same "polysemy" is attested, the meaning "tree" is almost certainly the more basic one. The item "tree" is formally marked as belonging to a grammatical gender which has sometimes been called "tree gender" because it contains not only the word for "tree" but also many specific tree names. Some Bantu languages have dissolved the "original polysemy" by using the same root as for "tree" but assigning it to a different gender when referring to "wood," e.g., Kinga *umu-biki* 'tree', *iki-biki* 'wood', i.e., "something of the nature of trees."

With regard to Brown's statement "*Overt marking usually, if not always, develops from polysemy*," I would like to point out that there are many "binomial labels" that must have been created without any intermediate "polysemy" stage (see, for example, the 20-odd terms *sea X*, where *X* = *anemone*, *butterfly*, *cow*, etc.).

Brown's observation that small-scale agriculturalists have larger vocabularies for wild plants and animals than hunter-gatherers surprises me. Peoples depending on cattle have rich related vocabularies; peoples that keep bees have large apicultural terminologies. I had always assumed that hunter-gatherers possessed unexcelled knowledge about their ecological habitats and that their languages encoded this knowledge. As far as Africa is concerned, I doubt whether this point can profitably be investigated by counting terms in existing dictionaries. Linguistic knowledge about agriculturalists is generally much more extensive than about hunter-gatherers.

by LEONTINE E. VISSER

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Brown's article follows the lines of thought set out by Berlin but also refines them. Apparently the mode of subsistence, whether it is hunting-gathering or small-scale agriculture, is crucial for ethno-biological classification. Binomialization occurs far more frequently among agriculturalists and, according to Brown, results from the diminishing salience of the binomially labeled classes.

But is this hypothesis valid for all biotic forms or only for those indigenous to the environment? For instance, there does not seem to be a one-to-one relationship between low salience and the use of binomial labels to encode newly introduced food crops like sweet potato and cassava.

When the sweet potato was introduced to the Sahu swidden cultivators of northwestern Halmahera (Indonesia), they labeled it a "tuber" after the yam (*lame*). Maize is labeled monomially as *kaitela*, probably because of its Iberian association. The cassava is classified in Sahu as "maize with a stem" (*kaitela pohong*), not as a tuber. However, among the Bugis of southern Sulawesi the sweet potato is labeled binomially as a "kind of tuber" (*lame kandora*) and so is the cassava (*lame ayu*). It is difficult to say that food crops in general are of low salience, yet the monomially labeled sweet potato is of little cultural value in Sahu, whereas the binomially labeled cassava is the staple crop of the Bugis. Here classification principles other than those which are expressed linguistically seem at issue.

## Reply

by CECIL H. BROWN

*DeKalb, Ill., U.S.A. 27 VIII 85*

Much of Berlin's criticism is based on a profound misunderstanding of my position. Berlin writes that I "treat all terminal taxa as folk species." What in fact I propose is that terminal generic taxa occurring in biological taxonomies of hunter-gatherers be relabeled "specific" taxa. My reason for this is that such a change will avoid the awkward characterization of expansion of biological reference as a process involving extending a term for a generic class to a more comprehensive generic class of which the former is a member. In my terminology, a term for a specific class will expand to a more comprehensive generic class. Another reason for this change, not mentioned in the paper, is that generic taxa (*sensu* Berlin), in taxonomies of foragers, tend to bear a one-to-one relationship to scientific species. Indeed, Nancy Turner (personal communication), who has undertaken extensive ethnobotanical research among several foraging groups of the Pacific Northwest, has found my relabeling suggestion sensible and appropriate in view of her discomfort with using the term "generic" in reference to labeled terminal classes which encompass only one species.

I do not propose in my paper that all terminal taxa in classificatory systems of agriculturalists be identified as specific classes. In fact, I accept Berlin's framework intact for identifying ethno-biological rank affiliation of classes in taxonomies of agrarian peoples. Thus, in my opinion, terminal generics can and do occur in folk taxonomies. I do not, then, "reject" Berlin's general principles. I only seek to revise them as they apply to biological taxonomies of hunter-gatherers.

Undoubtedly Berlin would object even to a relabeling proposal restricted to classes of hunter-gatherer taxonomies because of the importance he and many others before him attribute to the psychological (i.e., perceptual) status of generic taxa. In his view, one that, as he points out, in one form or another has prevailed among biosystematists since Linnaean times, generic classes are "those smallest biological groups readily perceptible to human beings without close study." Almost certainly most terminal generic taxa *sensu* Berlin of hunter-gatherer taxonomies are readily perceptible to foragers without close study. Consequently, in Berlin's view, such classes are generic, not specific.

The problem with Berlin's approach is that it tends to obfuscate patterns in the growth and development of folk biological taxonomy by insisting on use of inflexible jargon. The basic