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Mode of Subsistence and Folk Biological Taxonomy[1]

by Cecil H. Brown

SCHOLARS CONCERNED WITH FOLK CLASSIFICATION and naming of plants and animals, with the exception of Morris (1976), have by and large neglected the possibility that significant differences exist between folk biological taxonomies of hunting and gathering peoples and those of small-scale agriculturalists.[2] This is certainly true of those who have viewed folk taxonomy from a comparative perspective with the aim of discovering general principles of classification and nomenclature. For example, Berlin's pioneering work (1976; Berlin, Breedlove, and Raven 1973, 1974) proposes such principles on the basis of comparative evidence extracted solely from small-scale agrarian societies.[3] If hunting and gathering peoples and those who practice agriculture indeed differ significantly in ways in which they categorize and name biological organisms, then proposed general principles based on data from agriculturalists alone should be reconsidered.

In this paper cross-cultural evidence is compiled attesting to these fundamental differences, and an explanation of why they exist is proposed. In a subsequent paper, implications of such differences for the postulation of general principles of folk biological classification and nomenclature will be considered, with special attention to biotaxonomic growth and development (cf. Berlin 1972).

Folk taxonomies of hunters and gatherers differ from those of small-scale agriculturalists in two major respects: (1) the number of labeled biological classes in evidence and (2) the extent to which these classes are named through use of binomial labels. While hunting and gathering peoples apparently possess sizable inventories of labeled biological classes, the inventories of small-scale agrarian groups tend to be considerably larger. Data presented below indicate that small-scale cultivators on the average have roughly five times as many labeled plant classes as hunting and gathering groups and nearly twice as many labeled animal categories. A binomial label for a biological class is a composite lexeme consisting of a unitary term for a particular plant or animal category and some sort of modifier. *Blue oak, beefsteak begonia, cutthroat trout,* and *whitetailed deer* are American English examples of binomial labels. Binomial names are very common in folk taxonomies of agrarian peoples and very rare in those of hunters and gatherers. Data presented below indicate that on the average only 3.6% of plant classes and only 7.6% of animal classes in taxonomies of hunters and gatherers are labeled binomially. On the other hand, small-scale agriculturalists on the average have binomial labels for 35.9% of all plant classes and for 31.6% of all animal classes.

COMPARATIVE EVIDENCE

Table 1 lists 39 languages (societies), giving for each the number of labeled plant categories, the number of binomial labels, and the percentage of named classes that are binomially labeled. Table 2 presents similar information from 17 lan-

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[2] In this paper the expressions "hunters and gatherers" and "foragers" are used synonymously and refer to groups lacking both significant cultivation and pastoralism. Groups designated by these names are not uniform with respect to details of mode of subsistence. They range from peoples such as the Tasaday (Yen and Nance 1976), who until very recently acquired food only through collecting, to peoples of arctic and subarctic regions who are primarily hunters and very rarely gather plants (Lee and DeVore 1968: 7).

The expression "small-scale agriculturalists" refers to societies in which virtually all members are directly involved in some aspect of plant cultivation. It excludes peoples of nation-state societies who practice intensive agriculture entailing such features as monocropping of high-yielding varieties and use of pesticides.

[3] At the time of Berlin's work, very few studies were available treating in detail the biological classification and nomenclature of hunting and gathering peoples.

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TABLE 1

NUMBER OF LABELED BOTANICAL TAXA, NUMBER OF BINOMIAL LABELS, AND PERCENTAGE OF TAXA BINOMIALLY LABELED IN 39 LANGUAGES

LANGUAGE	NO. OF TAXA	NO. OF BINOMIALS	% BINOMIAL
Ifugao	2,131	?	?
Hanunóo	1,879	961	51.1
Eastern Subanun	1,400+[a]	?	?
Jörai	1,182	765	64.7
Tobelorese	1,162	317	27.3
Aguaruna	866	?	?
Huastec	861	282	32.8
Mende	844	147	17.4
Taubuid	825	237	28.7
Ndumba	766	343	44.8
Tzeltal	720	237	32.9
Wayãpi	714	189	26.5
West Futuna	654	461	70.5
Temne	492	66	13.4
Bellonese	450[b]	151	33.6
Chuj	382	89	23.3
Bontoc	354	32	9.0
Amuzgo	341	120	35.2
Gosiute*	337	43	12.8
Okanagan-Colville*	299	18	6.0
Mandarin Chinese	288	59	20.5
Sahaptin*	236	6	2.5
Gupapuyngu*	217	0	0.0
Tasaday*	215	25	11.6
Casiguran Dumagat*	208	5	2.4
!Kung*	193	4	2.1
Haida Masset*	191	7	3.7
North Saami#	190	9	4.7
Gugadja*	186	0	0.0
Anindilyakwa*	181	4	2.2
Haida Skidegate*	169	1	0.6
Bella Coola*	167	0	0.0
Lillooet*	154	1	0.7
Flathead*	147	1	0.7
Nitinaht*	140	3	2.1
Shuswap*	122	3	2.5
Kiowa-Apache*	108[b]	8	7.4
Kootenai*	72	0	0.0
Yupik Eskimo*	56	1	1.8

NOTE: A plus (+) indicates that the actual number of taxa is larger than the number given; a question mark (?) indicates that the number or percentage is undetermined.

[a] Includes only terminal taxa.

[b] Includes only "utilized" taxa.

* Hunting and gathering economy.

Reindeer-herding economy, no significant cultivation.

TABLE 2

NUMBER OF LABELED ZOOLOGICAL TAXA, NUMBER OF BINOMIAL LABELS, AND PERCENTAGE OF TAXA LABELED BINOMIALLY IN 17 LANGUAGES

LANGUAGE	NO. OF TAXA	NO. OF BINOMIALS	% BINOMIAL
Aguaruna	800+	?	?
Wayãpi	737	230	31.2
Ifugao	597	?	?
Nuaula	584	345	59.1
West Futuna	534	91	17.0
Tzeltal	492[a]	224	45.5
Kyaka Enga	466	?	?
Hanunóo	461	?	?
Tobelorese	443[b]	?	?
Anindilyakwa*	420	?	?
Gupapuyngu*	390	0	0.0
North Saami#	342	36	10.5
Ndumba	323	17	5.3
Sahaptin*	290	14	4.8
Montagnais*	255	34	13.3
Gosiute*	250	38	15.2
Flathead*	149	3	2.0

NOTE: A plus (+) indicates that the actual number of taxa is larger than the number given; a question mark (?) indicates that the number or percentage is undetermined.

[a] Excludes covert taxa.

[b] Terminal taxa.

* Hunting and gathering economy.

Reindeer-herding economy, no significant cultivation.

guages involving animal classes and labels. In both tables asterisks (*) following language names indicate that these languages are spoken by hunting and gathering groups. Languages lacking asterisks are spoken by small-scale agrarian societies with the exception of Mandarin Chinese, which, of course, is associated with a large-scale nation-state society. One language found in both tables, North Saami, is spoken by reindeer herders who lack significant cultivation. Information presented in these two tables has been extracted from both published and unpublished works which either specifically address the question of folk biological classification in the languages considered or in some other way deal with the ethnobiology of the groups in question. Sources are given in the appendix.[4]

A few sources systematically identify classes by ethnobiological rank (see Berlin, Breedlove, and Raven 1973, Berlin 1976) and give summary statistics from which numbers of plant and animal categories could be easily determined. In the vast majority of cases, however, sources simply supply lists of plant and animal names and identify the organism or organisms to which they refer. Frequently synonyms are found among these lists, but tables 1 and 2 report only the total number of classes in languages, not total number of labels. It is possible that occasionally I failed to identify synonyms as such and, thus, in a few instances counted two or more names for the same class as labeling two or more separate categories. Consequently, totals based on lists should be regarded not as exact representations of the number of classes pertaining to those lists but rather as close approximations. In a few cases it was possible to determine only the number of terminal classes pertaining to a language. (A terminal class is one that includes no labeled categories.) Totals involving only terminal classes are indicated in the tables. In two instances I was able only to determine that the languages involved had at least a certain number of labeled classes. Numbers for these languages are followed by a plus sign (+) indicating that more categories are clearly pertinent. In two cases, indicated in table 1, only the total number of "utilized" classes could be determined.

A few sources provide analyses permitting easy determination of number of pertinent binomial labels. In the vast majority of cases, however, especially where only lists of plant and animal names and their referents are supplied, determination of binomiality was somewhat more difficult. In judging binomiality of terms, five guidelines were followed:

1. A composite term is judged to be binomial if one constituent of the label stands on its own as the name of the class in which the category labeled by the composite term is immediately included. Thus, for example, *blue oak* is judged to

[4] No systematic sampling procedure was used in selecting languages to be included in this study. Rather, the procedure followed was exhaustive, entailing inclusion of all languages known to me whose folk biological taxonomy and nomenclature are described in sufficient detail in the literature to be useful to this study. Obviously, this literature is not extensive.

be binomial because the constituent *oak* labels the class in which the referent of *blue oak* is immediately included (blue oak is a kind of oak).

2. A composite term is not counted as a binomial even when condition (1) holds if the superordinate category labeled by the constituent is a major "life-form" class such as those denoted by English *tree, grass, bird, fish,* etc. Thus, for example, English *oak tree* is not considered a binomial label.[5]

3. A composite term is not judged to be binomial even if one constituent stands on its own as the name of another class that could be superordinate to the category labeled by the composite term if the (possibly) superordinate class includes organisms which are morphologically very dissimilar to those denoted by the composite term. By this guideline English *sea horse,* for example, is not considered binomial even though its constituent *horse* also labels a zoological entity.[6]

4. A composite term is judged to be binomial if there is at least one other composite term with which it shares a constituent and the unshared constituents of these labels are clearly modifying elements. Thus, for example, *white oak* and *blue oak* would be judged to be binomial even if it were not known that both designate classes included in a superordinate category labeled by *oak* because *white* and *blue* are clearly modifying constituents of these labels.[7] On the other hand, by this guideline *poison oak* and *poison ivy,* for example, would not be considered binomials, since the shared constituent, rather than the unshared ones, is obviously a modifying element.

5. Composite terms having constituents which translate "mate of," "brother of," "like," "similar," and so on, are not counted as binomials.

Binomial counts are lacking for several languages in tables 1 and 2 (indicated by a question mark [?]). This is because some sources give the number of plant and/or animal classes pertaining to languages without presenting lists of names and their referents from which counts of binomials could have been made.

Data provided for languages in tables 1 and 2 clearly are not uniform with respect to the realities they reflect. This inconsistency arises because different fieldworkers have had different goals and, consequently, have approached the problem of gathering and presenting data in different ways. For example, class counts for Ndumba in tables 1 and 2 are the numbers of plant and animal classes of which knowledge is shared by at least nine of ten Ndumba informants (Hays 1979, 1983). Total numbers of biological classes reported in other sources are not limited to those that are widely shared; in most cases sources do not spell out the extent to which the information they provide is shared within a group. Despite such difficulties, I am confident that statistics presented in tables 1 and 2 give a relatively accurate idea of the size of inventories of labeled plant and animal classes in the languages considered. In addition, while very few of the sources consulted claim that their studies are exhaustive, almost certainly most have collected for their respective languages close to all of the labeled plant and/or animal classes known to the majority of their speakers.[8]

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[5] Berlin (1976; Berlin, Breedlove, and Raven 1973, 1974) proposes that taxa of "specific" and "varietal" ranks of folk biological taxonomies are characteristically labeled by "secondary lexemes." Secondary lexemes are always binomial labels. However, a binomial label is a secondary lexeme only if all items immediately included in the same biological class are binomially labeled. Thus, for example, all members of the English class *oak* are labeled by secondary lexemes, since all of their labels are composed of the word *oak* plus a modifier, e.g., *white oak, shingle oak, post oak,* etc. *Tulip tree* is not a secondary lexeme, since all members of the "life-form" class *tree* are not similarly obligatorily labeled by equivalent binomial terms (cf. Berlin 1976: 397). This study is principally interested in binomial labels that are also secondary lexemes in Berlin's scheme. Thus, apparent binomials that label taxa of ranks other than "specific" and "varietal" are not counted as binomials in this survey, since secondary lexemes do not typically label taxa other than "specifics" and "varietals." In Berlin's framework labeled classes immediately included in "life-form" taxa are "generic" rather than "specific" or "varietal" categories. Consequently, apparent binomial labels for "generic" classes such as *oak tree* are not counted. In this work classes are determined to be affiliated with the "generic" rank if they appear to be immediately included in taxa roughly equivalent in referential scope to English *tree, grass, vine, bird, fish, bug,* etc., i.e., "life-form" taxa that have been identified as "universals" of folk biological taxonomy (Brown 1977, 1979, 1984*b*). The current cross-cultural survey of folk biological classification reveals that obligatory binomials rarely label taxa associated with ranks other than "specific" and "varietal." This is especially true of folk taxonomies of hunters and gatherers.

[6] When morphologically distinct biological entities, such as horse and sea horse, are connected nomenclaturally through a composite construction, such as *sea horse,* it is highly unlikely that the composite label is a secondary lexeme. Thus, for example, since a sea horse is clearly not a kind of horse, obviously *sea horse* does not label a taxon which is part of a contrast set of the class *horse* all of whose members are labeled by composite lexemes consisting of the term *horse* and a modifier. (In the scheme of Berlin, Breedlove, and Raven [1973: 217] *sea horse* is an "unproductive primary lexeme.")

[7] This guideline is motivated by the possibility that some sources consulted may occasionally fail to list names of taxa, such as *oak,* in which classes labeled by composite terms, such as *white oak* and *blue oak,* are immediately included.

[8] Headland (1981) observes for Casiguran Dumagat, spoken by a hunting and gathering Negrito group on the Philippine island of Luzon, that there may be vastly more labeled plant classes in the language than the 208 he reports, perhaps "over a thousand." This observation is curious given that he is fluent in Casiguran Dumagat, has been involved in extensive linguistic fieldwork with the Negrito group for 17 years (p. ix), has produced a dictionary of the language (Headland and Headland 1974), and has specifically addressed his research to Casiguran Dumagat botanical classification during four field sessions from 1974 to 1979 (p. ix).

There are several reasons for believing that the number of labeled plant classes he reports is more representative of the actual magnitude of the language's botanical class inventory than is a figure of "over a thousand." In collecting folk botanical taxonomy from speakers of Huastec, a Mayan language of northern Mexico spoken by swidden agriculturalists (cf. Brown 1971, 1972), I was able within a period of only four days or so, working four or five hours a day with a single informant, to elicit 358 plant names. This is so despite my lack of fluency in Huastec. (I collected Huastec data through use of the elicitation-frame technique [see Black 1969].) I estimate that the number of classes I elicited for Huastec is around a hundred or so fewer than the number of plant categories known to most speakers of the language. According to Alcorn and Hernández V. (1983), the total number of Huastec plant taxa is 861. My experience suggests that if the large number of plant categories Headland believes may be known to Casiguran Dumagat–speakers existed, then he would have collected a vastly larger number of named plant categories in a much shorter period of time. Clearly, the number of classes pertaining to the language is greater than 208, but I would be surprised to find that it is much larger than around 300.

Headland (1981) refers to other evidence which appears to support my contention concerning the actual size of the Casiguran Dumagat plant class inventory. In a footnote (p. 98) he mentions Melinda S. Allen's study of the botanical taxonomy of another Philippine Negrito hunting and gathering group located 200 km north of the Casiguran Dumagat–speakers. In a personal communication to Headland, Allen has written, "To date 293 folknames have been elicited for an unspecified number of Latin equivalents." This figure for a group having a mode of subsistence and living in a habitat comparable to those of Casiguran Dumagat–speakers strongly suggests that the number of plant classes pertaining to the latter language is more in the range of 300 than "over a thousand." In addition, it is informative that only five binomial terms were elicited by Allen from speakers of her Negrito language. Interestingly, Headland (1981) reports that only five binomial terms label plant classes in Casiguran Dumagat.

Headland refers to earlier studies of Philippine groups whose adult members "can identify at least several hundred different plants" (1981: 15), among them the Eastern Subanun (Frake 1969), the Hanunóo (Conklin 1954), and the Ifugao (Conklin 1967), with over 1,000 labeled plant categories, and a generous number of other groups with names for at least 600. All of these peoples are agriculturalists. Not recogniz-

TABLE 3

NUMBER OF LABELED BIOLOGICAL TAXA IN 14 LANGUAGES

LANGUAGE	NO. OF TAXA	NO. OF PLANT TAXA	NO. OF ANIMAL TAXA
Ifugao	2,728	2,131	597
Hanunóo	2,340	1,879	461
Aguaruna	1,666+	866	800+
Tobelorese	1,605+	1,162	443+
Wayãpi	1,451	714	737
West Futuna	1,188	654	534
Tzeltal	1,212	720	492
Ndumba	1,089	766	323
Gupapuyngu*	607	217	390
Anindilyakwa*	601	181	420
Gosiute*	587	337	250
North Saami#	532	190	342
Sahaptin*	526	236	290
Flathead*	296	147	149

NOTE: A plus (+) indicates that the actual number of taxa is larger than the number given.
* Hunting and gathering economy.
# Reindeer-herding economy, no significant cultivation.

An additional table combines statistics of tables 1 and 2. Table 3 lists the 14 languages for which both plant and animal taxa counts are available and aggregates these.

Most, if not all, of the hunting and gathering groups identified by their languages in tables 1–3 are today involved to some extent in plant cultivation and/or animal husbandry. However, most of these groups have acquired such practices only very recently and in almost all instances still commit considerable time and energy to collecting and/or hunting.

ANALYSIS

The order of listing of languages in tables 1–3 is according to number of labeled biological classes in evidence, from most to least. In all three tables the distinction with regard to magnitude of folk biological taxonomies of hunters and gatherers versus those of small-scale agriculturalists is immediately apparent. For example, all 18 languages of table 1 having more than 337 labeled plant taxa pertain to agrarian peoples, while of the 21 languages having 337 or fewer named classes, 19, or 90.5%, are spoken by foragers. The two apparent exceptions to the dichotomy of ranking seen in table 1 are not really exceptions. Neither North Saami nor Mandarin Chinese, both of which have fewer than 337 labeled plant taxa, is spoken by a hunting/gathering people. However, as I have said, neither language is associated with small-scale agriculturalists. It may be the case that the number of labeled biological classes in languages such as North Saami, spoken by herders who lack cultivation, typically falls within the range characterizing hunters and gatherers. In the case of North Saami, number of labeled animal classes (see table 2) as well as labeled plant categories is congruent in magnitude with those of hunters and gatherers. However, since only one such language is surveyed in this study, obviously no firm conclusion can be reached concerning the usual size of folk biological taxonomies of herders.[9] Similarly, since only one language spoken in a nation-state society, Mandarin Chinese, is included in this study, no definitive conclusion can be reached regarding the typical size of folk biological taxonomies of such languages.

The source for Mandarin Chinese is Chao (1953), who drew on his own speaking-knowledge of the language for botanical terminology.[10] In doing so, he restricted himself to words for plants that he had "actually seen" (p. 380). Using myself as an informant for American English, I have been able to recall 273 folk labels for the same number of plant taxa.[11] However, I can associate accurately only 123 terms, or 45.1% of the total, with actual plants, the other 150 terms constituting labels for "empty" categories, for example, *rosewood tree,* whose referents I cannot identify. Presumably if Chao had also included terms for empty plant categories in his list for Mandarin Chinese, it would have been somewhat larger, if not considerably so.

Chao's (1953) study and my own investigation of American English using myself as an informant suggest that the number of terms for "non-empty" botanical classes in nation-state languages usually accords with the range for hunters and gatherers rather than with that characterizing small-scale farmers. Data from other studies touching on plant naming and classification in nation-state languages seem to suggest the same (Gatewood 1983; Dougherty 1978; Mulcahy 1967, 1973).

Table 2 treating folk animal taxonomies is considerably less extensive than table 1 with regard to numbers of languages listed.[12] However, similarly to table 1, table 2 shows a dichotomy of ranking entailing small-scale agrarian peoples and foragers. The nine languages having more than 420 labeled animal taxa are all spoken by agriculturalists while all but one of the eight languages having 420 or fewer labeled taxa are spoken by peoples lacking significant cultivation (including speakers of North Saami). The one exception is Ndumba, a Highland New Guinea language, with 323 animal names. It is interesting that a source for Ndumba (Hays 1979) writes that "there is little question that they [the Ndumba] are relative newcomers to plant domestication" (p. 269).[13] This perhaps indicates that there is a time lag between acquiring agriculture and increase in size of some areas of ethnobiological taxonomy.

Table 3 aggregates numbers of plant and animal classes of the 14 languages found in both tables 1 and 2 and, thus, presents the total number of labeled biological taxa in each of

ing the possibility of fundamental differences between folk biological taxonomies of hunters and gatherers and those of small-scale agrarian groups, and given knowledge of these comparative data from Philippine groups, it is a reasonable, though incorrect, conclusion that 208 labeled plant classes must be only a fraction of the total number that actually pertains to any Philippine language.

In a report describing the ethnobotany of another hunting and gathering group, Peile (1976) concludes that his inventory of 186 labeled plant classes for Gugadja, an Australian Aborigine language, is "very much incomplete." Like Headland, Peile has spent considerable time with Gugadja-speakers in undertaking a "depth-study" of this northern Western Desert language, and it is surprising that he has isolated only 186 categories if indeed vastly more labeled plant classes are known to these people. In addition, sources for the two other Aborigine languages listed in table 1, Gupapuyngu and Anindilyakwa, give labeled plant class counts close to his figure, 217 and 181. It should be added that the investigation of Anindilyakwa plant names and classes is in fact an exhaustive study (Levitt 1981).

[9] In addition, of course, no conclusion can be reached concerning the typical size of biological taxonomies of peoples whose economies are based primarily on fishing, such as Indian groups of the U.S. Northwest Coast, since languages pertaining to such societies are not found in the current survey.

[10] Stephen Murray (personal communication) informs me that Chao was not a native speaker of Mandarin Chinese.

[11] Because of my long-standing interest in folk botany, the list of plant terms I elicited from myself is probably somewhat longer than that typical of most speakers of American English. However, I have never undertaken a detailed study of the folk botanical classification of American English–speakers. I should also add that I have had no formal training in scientific biosystematics.

[12] Historically, folk classification of plants has proven of greater interest to researchers than folk classification of animals. This may relate at least in part to the fact that plants are usually more easily collected than animals.

[13] Terence Hays (personal communication) writes that he believes that Ndumba "horticulture *is* relatively recent, but we could be dealing with hundreds of years."

these languages. Again, a dichotomy of ranking is observed. The eight languages pertaining to agriculturalists all have more than 607 labeled biological classes while the six nonagricultural languages (including North Saami) have 607 or fewer biological categories.

Setting aside North Saami from calculations, the data of table 3 indicate that the average number of labeled biological classes found in languages of small-scale cultivators is 1,659.9. This compares with an average of 523.4 biological taxa in languages of hunters and gatherers. On the average, then, biological taxonomies of small-scale agriculturalists appear to be approximately three times the size of those of hunters and gatherers.

Looking at plant and animal vocabularies separately, languages of agriculturalists and those of hunting/gathering people differ most with respect to size of botanical taxonomies. The data presented in table 1 indicate that the average number of plant classes of agricultural languages is 890.2, compared with an average of 178.8 for languages of foragers. Thus the former languages on the average tend to have roughly five times as many plant classes as the latter. On the other hand, the data of table 2 indicate that cultivators on the average possess almost twice the number of animal classes pertaining to hunters and gatherers, with averages of 543.7 and 292.3 respectively. (These averages exclude numbers for North Saami and Mandarin Chinese.)

Aggregating averages derived from the data of tables 1 and 2 for both agriculturalists and foragers yields 1,433.9 total average biological taxa for the former group (890.2 + 543.7) compared with 471.1 for the latter (178.8 + 292.3). These figures accord with those cited above based on the data of table 3 indicating that on the average biological taxonomies of cultivators are approximately three times the size of those of hunting/gathering people.

In addition to indicating number of labeled taxa in languages, tables 1 and 2 present number of plant and animal classes labeled by binomials and the percentage of total taxa labeled binomially. For comparative purposes, reference only to percentage of taxa binomially labeled is appropriate here, since number of binomial labels in languages is partly influenced by sheer size of the biological taxonomy, which, of course, differs radically across languages.

Table 1 shows a dichotomy of ranking of small-scale cultivators versus hunters and gatherers with respect to percentage of plant classes labeled binomially. With one exception, Bontoc, the 14 languages of small-scale agriculturalists for which botanical binomial counts have been determined all have binomial percentages above 12.8%. Excluding Bontoc with 9.0%, botanical binomial percentages for most cultivators surveyed range from 13.4% to 70.5%, with a mean of 35.9% for all 14 languages. In contrast, the 20 languages spoken by peoples lacking cultivation (including North Saami) without exception have binomial percentages at or below 12.8%. Excluding Gosiute and Tasaday, with 12.8% and 11.6% respectively, botanical binomial percentages for most hunters and gatherers surveyed range from zero to only 7.4%, with a mean of only 3.6%, for 19 of the 20 languages (North Saami is excluded). In fact, 4 of these languages apparently totally lack binomial labels for plant classes.

Three languages of table 1, Bontoc, Gosiute, and Tasaday, seem to deviate significantly from the average botanical binomial percentages usually associated with the respective modes of subsistence. In the case of Tasaday, there appears to be a plausible explanation for this discrepancy. The Tasaday, who before 1971 were an undiscovered group on the island of Mindanao in the Philippines, until very recently supported themselves through gathering alone. However, there are several reasons for believing that these people were once cultivators who for some reason gave up an agricultural way of life (Yen 1976:157). If so, the relatively high percentage of plant taxa labeled by binomials in the language (11.6%) may relate to a time in the past when Tasaday folk botanical taxonomy and nomenclature fit the typical profile of an agricultural group. Loss of agriculture could have influenced both reduction of number of labeled plant taxa and decrease in the percentage of binomials. In the former case number of taxa apparently was reduced to the range typical of hunters and gatherers. In the latter, decrease in number of binomial labels apparently was not so extensive as to result in a binomial percentage close to the low average percentage of peoples lacking cultivation (i.e., 3.6%).

The data of table 1 indicate that as the size of a folk botanical taxonomy increases, the percentage of included taxa labeled binomially increases as well. In fact, there is a very strong positive association between taxonomy size and binomial percentage. This correlation is presented in table 4 (gamma = .99, $p < .001$, $N = 36$). The correlation of table 4 would be perfect except for two "miss" cases, Bontoc and Mandarin Chinese. The latter two languages, then, can be interpreted as not meeting expectations regarding the typical positive association of taxonomy size with percentage of binomials.

The fact that Mandarin Chinese is a nation-state language, spoken by neither small-scale agriculturalists nor hunters and gatherers, may partly explain why it has a larger percentage of binomial labels for plant classes (20.5%) than expected given the size of its folk botanical taxonomy. It does not seem implausible that at some time in the past most speakers of Mandarin Chinese had a botanical taxonomy and nomenclature typical of small-scale cultivators. For reasons to be outlined below, when Mandarin Chinese developed as a nation-state language, the size of its botanical taxonomy apparently decreased significantly, approximating the typical size of taxonomies pertaining to groups lacking cultivation. However, similarly to the Tasaday situation discussed above, percentage of classes labeled binomially did not decrease to the range characteristic of hunting/gathering peoples. Why this should be the case is not apparent at present.

The findings of my study of American English botanical classification and nomenclature, in which I served as my own informant, accord closely with figures for Mandarin Chinese, this perhaps indicating a pattern for nation-state languages. Of the 273 labeled plant taxa in my American English, 60, or 22.0%, are labeled binomially. Table 1 shows that of the 288 plant classes in Chao's (1953) Mandarin Chinese, 59, or 20.5%, have binomial labels. These similar figures for nation-state languages suggest that development from small-scale cultivation to the intensive agriculture of nation-states may result in a decrease in the number of labeled plant classes in botanical taxonomies to roughly one-third of the average number associated with small-scale agriculturalists (890.2) and a reduction of the percentage of binomially labeled plant classes to roughly two-thirds of the average percentage found in languages of small-scale cultivators (35.9%).

The data of table 2 relating to binomial labels for animal classes are not extensive, with binomial percentages determined for only 11 of the 17 languages listed. Of these 11 lan-

TABLE 4

ASSOCIATION BETWEEN SIZE OF BOTANICAL TAXONOMY AND PERCENTAGE OF TAXA LABELED BINOMIALLY

	% BINOMIAL	
NO. OF TAXA	12.8 and below	Above 12.8
Above 337	1	14
337 and below	20	1

gamma = .99, $p < .001$, $N = 36$

guages, 5 are spoken by small-scale cultivators. The average binomial percentage for the latter languages is 31.6%. This compares with an average binomial percentage of only 7.6% for the 6 languages of peoples lacking cultivation. It should be noted that the binomial percentage for Ndumba (5.3%), a language of horticulturalists, is lower than the average binomial percentage for noncultivators (7.6%). I have suggested above that the relatively small inventory of animal classes labeled in Ndumba may be linked to the recency of Ndumba agriculture. Perhaps this same factor also explains the language's unexpectedly low binomial percentage for animal taxa.

EXPLANATORY FRAMEWORK

A working assumption in attempting to account for the above findings is that vocabulary is to a large extent reflective of the long-term interests and endeavors of the people who use it. Thus, in the case at hand, it is assumed that people who have large biological taxonomies have a more extensive traditional knowledge of and interest in the world of plants and animals than people having smaller ones. If this assumption is correct, then it is probably also the case that cultural importance of plants and animals affects size of biological taxonomies rather than vice versa. In other words, greater importance determines larger biological vocabularies and lesser importance determines smaller vocabularies.

A possible explanation of the finding that biological taxonomies of small-scale agriculturalists are larger than those of hunters and gatherers is that in the shift from noncultivation to cultivation people develop an interest in a new set of biological organisms, i.e., domesticated ones, which is added to an existing set of organisms that are collected and/or hunted. Perforce, then, the resulting total inventory of organisms of cultural interest is larger than the preagricultural one. Larger biological taxonomies, of course, would reflect this development.

An argument against this explanation is that development of small-scale agriculture may bring about a diminishing dependence on hunting and gathering and, hence, a lessening interest in wild plants and animals. In such a case, one might expect folk biological taxonomies often to decrease in size. The data compiled here indicate that such a reduction rarely, if ever, occurs, at least in the transition from hunting and gathering to small-scale agriculture. Consequently, this argument must be set aside as having little empirical support.

Since biological taxonomies are not reduced but apparently are increased in size during the shift from noncultivation to agriculture, it follows that the cultural importance of wild plants and animals is at least maintained. Indeed, there is evidence indicating that such a transition actually leads to an augmented interest in wild organisms. For example, Lee (1979: 180–81) notes that the Tonga, small-scale cultivators of the Zambezi Valley (Africa), use at least 131 species of edible wild plants compared with 105 species used by the !Kung Bushmen, hunters and gatherers located 500 km west of the Tonga. While Lee states that the Tonga habitat is better watered than that of the !Kung, he argues that this alone does not explain why their stock of used wild plants is larger. According to Lee, the Tonga actually eat portions of 21 species that are also found in the !Kung area but are not considered by the !Kung to be food. For example, the Tonga consume an abundant seedpod, *Acacia albida,* which the !Kung ignore even though they could harvest thousands of kilograms of them each year. As it happens, *Acacia albida* is toxic to humans. Nevertheless, the Tonga have developed an elaborate method of processing the seedpods involving several steps of soaking, boiling, and leaching that renders them edible.[14] With regard to these findings Lee (p. 181) writes: "The fact that the Tonga eat *A. albida* and many other species ignored by the !Kung is an index of the seeming paradox that an agricultural people may utilize wild plant and animal resources more intensively than a hunting and gathering people."

Another index of this apparent paradox is the invariably larger size of folk biological taxonomies of small-scale cultivators compared to those of nonagriculturalists. This difference is not attributable just to the fact that domesticated organisms are added to a preexisting inventory of utilized wild ones when the transition to farming is made, because inventories of words designating wild plants and animals in languages of small-scale agriculturalists are usually larger than complete biological vocabularies of foragers. For example, of the 825 labeled plant classes in Taubuid, 727 are noncultivated, and of 714 labeled plant taxa in Wayãpi, 551 are noncultivated. In addition, of 563 "generic" plant taxa in Aguaruna, 502 are noncultivated; of 381 "generic" plant taxa in Tzeltal, 354 are noncultivated; and of 1,625 "terminal" plant classes in Hanunóo, 1,180 are noncultivated.[15] In each of these agricultural languages inventories of terms for wild plants are considerably larger than any inventory of labeled plant taxa of hunting/gathering languages listed in table 1. This indicates that the transition to agriculture typically results in a vastly increased number of labeled wild plant and animal taxa. This, of course, implies an augmented interest in wild organisms for small-scale agrarian peoples.

Another aspect of this issue is that with the spread of agriculture, hunting and gathering peoples have regularly found themselves occupying the less floristically complex world regions, such as arctic tundra and desert areas. Typically, then, agriculturalists inhabit world regions that are biologically richer than those in which noncultivators live, and this perhaps promotes a considerably enhanced knowledge of and interest in wild plants and animals—and in turn larger taxonomies—among farmers as compared with foragers. Thus, size of taxonomy might be only indirectly influenced by mode of subsistence, the really important influence being the nature of world areas typically inhabited by foragers and agriculturalists respectively.

This argument is based on the assumption that degree of species diversity in a region affects extent of cultural interest in wild plants and animals, with (1) more diversity encouraging interest and (2) less diversity, if not actually discouraging interest, at least imposing a rather severe constraint on the number of wild plants and animals that could be of importance to a people. However, evidence exists which calls into question the first of these assumptions. Several hunting and gathering groups live in biológically rich areas but nonetheless demonstrate the lower levels of interest in wild plants and animals typical of foragers of less rich regions. For example, there are 181 labeled plant classes in Anindilyakwa, which is spoken by foraging Aborigines of Groote Eylandt in Australia. The latter figure is close to the average for noncultivators, which is 178.8. Nonetheless, the environment of Groote Eylandt, an island in the Gulf of Carpentaria, supports considerable botanical species diversity. Most of the island is covered by open forest, but there are also rain forests and considerable areas of swamp (Levitt 1981: 6). Another Australian language, Gupapuyngu, has a folk botanical taxonomy whose size (217) accords with those of other hunting and gathering groups but is nonetheless

[14] One explanation of the !Kung failure to exploit *A. albida*, suggested to me by Terence Hays (personal communication), is that these people have precious little water for soaking, boiling, and leaching this seedpod.

[15] Sources for these figures are Pennoyer (1975) (Taubuid), Grenand (1980) (Wayãpi), Berlin (1976) (Aguaruna), Berlin, Breedlove, and Raven (1974) (Tzeltal), Conklin (1954) (Hanunóo). These are the only sources of those consulted (see appendix) that distinguish cultivated from wild plants in botanical taxonomies of small-scale agriculturalists.

spoken in a botanically rich environment. Rudder (1979) describes this environment, in northeastern Arnhem Land, as consisting of mangrove flats, areas of rain forest, and vast freshwater swamps. Clearly, in these cases biological richness has not resulted in a level of interest in wild organisms—as revealed by size of folk botanical taxonomy—of the magnitude typical of small-scale cultivators.

Two hunting and gathering groups of the Philippines, the Tasaday and the Casiguran Dumagat, similarly live in environments characterized by floristic complexity but have folk botanical taxonomies of sizes typical of noncultivators. The Tasaday, who inhabit the great rain forest of South Cotabato on the island of Mindanao, label 215 plant classes. The Casiguran Dumagat of the northeastern coast of Luzon, whose environment is a tropical rain forest described as being "extremely rich in flora" (Headland 1981: 14), have a botanical taxonomy consisting of 208 labeled plant classes. In both of these examples botanical taxonomies do not resemble in size those typical of small-scale cultivators but rather are close to the average size (178.8) for peoples lacking agriculture.

These examples indicate that world region is not the only or even the primary influence on size of folk biological taxonomies. Clearly, a habitat poor in species diversity would severely restrict the number of floral and faunal categories labeled in a language. For example, lack of botanical species diversity is almost certainly part of the reason only 56 labeled plant taxa pertain to Yupik Eskimo. On the other hand, while biological richness may be a prerequisite for large plant and animal taxonomies, it clearly does not necessitate large size. If it did, we would expect botanical taxonomies of languages such as Anindilyakwa, Gupapuyngu, Tasaday, and Casiguran Dumagat to rival in size those of languages of small-scale cultivators. Since they do not, and since the former four languages are all spoken by foragers, a logical conclusion is that mode of subsistence is the primary factor determining size of folk biological taxonomies. In addition, since taxonomies of hunters and gatherers typically are significantly smaller than those of small-scale agriculturalists, it follows that a shift from the former to the latter mode of subsistence usually involves a significant increase in the number of labeled biological categories. Furthermore, as argued earlier, this increase entails not only addition of terms for cultivars to a preexisting set of terms for noncultivated organisms, but also incorporation of a significant number of terms for previously unlabeled wild plants and animals.

One factor that probably relates to the enhanced cultural importance of wild plants and animals after the shift to food production is that subsistence farming itself actually changes the local environment in such a way that biological diversity is considerably increased (Kunstadter 1978, Rambo 1982, Scudder 1971). Subsistence cultivation leads to diversification of habitats and a significant widening of the range of immediately available wild plants. Land farmed by subsistence agriculturalists is usually divided into a variety of ecotypes. Some are perennially cultivated, others involved in a bush-fallow system of cultivation in which there is a variety of plant succession. In effect, the system of land use of subsistence farmers tends to create a local environment significantly more complex than if such a system were not in place. These diverse-vegetation complexes provide small-scale farmers with a wide range of useful wild plants that are immediately accessible as food and medicine, for construction purposes, and so on (see Scudder 1971: 24).

Clearly, then, the local habitats of small-scale agriculturalists are usually botanically, if not zoologically, more diverse than those of hunter/gatherers. Indeed, this is probably usually the case even when the foragers in question live in biologically rich areas such as tropical rain forests. However, the biological diversity created by subsistence agriculture alone cannot explain why farmers' knowledge of plants and animals is typically vastly greater than that of noncultivators. To understand this, one need only refer to the fact observed above that foragers of biologically rich areas do not necessarily develop plant and animal taxonomies larger than those of hunting and gathering peoples of biologically poorer regions.

In my view, the human-created biological richness of local environments of small-scale farmers should be interpreted as providing an opportunity to expand traditional knowledge of and interest in wild plants and animals, not as the cause of that enhancement. In his comparison of subsistence activities of the Tonga and !Kung, Lee (1979: 180–81) gives some insight into why wild plants and animals develop considerable cultural importance for small-scale farmers: "The Tonga live at a population density approximately 100 times that of the Dobe !Kung. When Tonga crops fail, enormous pressure is brought to bear on the . . . wild plant foods—far greater than that exerted by the !Kung on their plants even in time of drought." A major benefit of agriculture is that it supports population densities many times greater than those that can be maintained by a foraging way of life (see Cohen 1977). Of course, this benefit becomes a liability if broad crop failure occurs. In such an event, as Lee suggests, small-scale farmers must exploit wild plant and animal resources more intensively than hunters and gatherers, since there are vastly more mouths to feed. Consequently, expanded knowledge of and interest in wild plants and animals in fact may be essential to the existence of small-scale agricultural groups.[16]

Foraging peoples, of course, are also subjected to fluctuations in availability of food resources. There are, however, reasons for believing that hunters and gatherers are seldom pressured to exploit wild plants and animals as intensively as small-scale agriculturalists under stress of broad crop failure. First, wild plants and animals are considerably less vulnerable than domesticated ones to such hazards as drought and disease. As Cohen (1977:29–30) notes, wild organisms are preselected for their capacity to endure and survive in their natural habitats. Domesticated plants and animals, on the other hand, are often imported to environments to which they are not adapted and, hence, lack such natural defenses. Given the natural resistance of wild plants and animals to drought, disease, etc., fluctuation in the food supply of foragers is rarely as severe as that pertaining to small-scale agriculturalists. Added to this is the fact that there are vastly fewer people to feed in foraging societies than in agricultural societies, so that even in times of scarcity no exceptional efforts need be undertaken by hunters and gatherers to acquire food. Scudder's (1971:6; cf. Woodburn 1968:52) discussion of several African groups is instructive on this point:

> [T]he Hadza and the Bushmen . . . are less susceptible to seasonal hunger than are their immediate agricultural neighbours. During Lee's early field work local crops failed during three successive years. Yet throughout this period the Dobe Bushmen had sufficient food in spite of the fact that some of the Herero were also utilizing the same wild plant resources as substitutes for their usual [agricultural] diet. As for the Eastern Hadza . . . there is no period of shortage . . . rather food . . . is always abundant even at the height of the dry season in a year of drought. . . . Furthermore . . . for a Hadza to die of hunger, or even to fail to satisfy his hunger for more than a day or two, is almost inconceivable. . . . In contrast . . . [i]t is clear that agriculturalists are liable to suffer from recurrent famine in this area while hunters and gatherers are not. . . .

[16] Hunn and French (1984) independently recognized the correlation between mode of subsistence and size of biological taxonomy outlined in this paper. In addition, they independently arrived at much the same explanation of the phenomenon as given in this paragraph.

Peter Brosius (personal communication) points out that agriculturalists should not all be lumped together with reference to susceptibility to crop failure and famine. For example, groups growing root crops are *much* less susceptible than those growing grain crops.

If the phenomenon observed for African groups by Scudder is typical, then it would not be unexpected to find that hunters and gatherers more than occasionally are less intensive foragers than small-scale agriculturalists occupying the same world region. For example, Terry Rambo (personal communication) writes, "A point frequently commented on by ethnologists working in Peninsular Malaysia is that the Semang or Negritoes, who are foragers, hunt and trap less intensively than do the neighboring Senoi shifting cultivators."

There is an additional factor which almost certainly contributes to the enhanced cultural importance of wild plants and animals for subsistence agriculturalists. Several recent studies (e.g., Larsen 1981 and especially Cohen and Armelagos 1984*a*) have convincingly shown that the shift from foraging to agriculture is typically correlated with a considerable increase in new health risks. For the most part these studies are based on investigations of paleopathologies in archaeological skeletal populations. Where pre- and postagricultural populations are found in the same location, postagricultural skeletons inevitably show much higher incidences of certain paleopathologies (Cohen and Armelagos 1984*b*). At least one factor contributing to this phenomenon is clearly understood. The higher population densities supported by agriculture result in higher frequencies of infectious crowd diseases (Larsen 1981). Major changes in diet which accompany the development of agriculture also probably contribute to the greater health risks of small-scale farmers.

In response to the increased incidence of disease with the development of agriculture, it seems plausible that people would be highly motivated to exploit broadly those biological organisms in their environments, especially plants, that may have medicinal value. I am, of course, proposing another reason subsistence farmers should vigorously tap the biological complexity of their local habitats, a complexity which ironically is produced by the very factor underlying increased health risk, the development of farming. Foragers also collect medicinal plants and animals. However, given the lower incidences of disease to which they are subjected, one might expect them to be less interested in the full range of medicinal biological organisms available in their environments than small-scale agriculturalists. The smaller size of their biological taxonomies would certainly seem to bear out this observation.

In addition to supporting higher population densities, agriculture, of course, also promotes a settled way of life. It seems likely that sedentariness itself would encourage a greater use of things in the natural environment, especially wild plants and animals. A sedentary way of life may permit more orientation to material objects than is possible or practical in the typical nomadic society of hunters and gatherers. Agricultural groups are usually much more involved in the production of material culture than are foragers. Of course, raw materials are required for such production activities, and a common source for such materials is wild organisms. Thus the sedentary way of life of small-scale agriculturalists would also tend to promote an increased knowledge of and interest in wild plants and animals.

Clearly, for several reasons outlined above, initial development of agriculture results in an augmented rather than diminished dependence on hunting and gathering. In addition, data compiled here indicate that such a dependence remains intact as long as farming continues on a small-scale basis. It is only when societies shift from small-scale cultivation to intensive agriculture that foraging diminishes greatly in importance. Such a transition, of course, results in numerous other changes involving both cultural practices and the environments in which they occur. For example, intensive monocropping of high-yielding varieties leads to the biological simplification of an environment (Scudder 1971: 47). As a result, there are vastly fewer wild plants and animals to be collected and hunted.

Intensive agriculture also leads to the increasing removal of people from direct contact with the world of plants and animals as urban settings become more important than rural ones. Loss of intimacy with nature results in a greatly diminished traditional knowledge of and interest in organisms (Berlin 1972: 83). Consequently, languages of nation-state societies, such as Mandarin Chinese and American English, have folk biological taxonomies considerably smaller than those of small-scale agriculturalists. Ironically, folk biological taxonomies of peoples of nation-states, who are severely removed from nature, apparently tend to be of roughly the same magnitude as those of hunters and gatherers, who, of course, are directly dependent on the world of plants and animals for their livelihood.

BINOMIAL LABELS AND SALIENCE

The strong positive correlation demonstrated in table 4 indicates that as the size of a folk biological taxonomy increases with a shift to agriculture, the percentage of taxa labeled binomially is augmented as well. Recent investigations of the role of "overt marking" in language (Witkowski, Brown, and Chase 1981; Witkowski and Brown 1983, 1984; Brown and Witkowski 1981, 1983, 1984; Brown 1983, 1984*a*) provide a framework for explaining this finding.

A common phenomenon in language is the nomenclatural linkage of semantically related referents. Polysemy is one form of such a linkage in which two or more related referents, for example, wood and tree, are labeled by a single term (wood and tree are semantically connected through a "part of" relationship).[17] Overt marking is another way in which related referents can be nomenclaturally associated. This involves a base term, such as a word for wood, united with a modifier (overt mark) such as "upright," creating a complex expression such as "upright" + "wood" as a label for tree.

The investigations cited above reveal an association between referent salience and nomenclature. This is that referents labeled by overt marking constructions strongly tend to be less salient than those labeled by nonpolysemous, unitary lexemes. For example, the referent tree is almost always less salient when labeled by an overt marking construction such as "upright wood" than when labeled by a nonpolysemous, unitary lexeme such as English *tree* (Witkowski, Brown, and Chase 1981).

Referent salience relates to two factors, natural salience and cultural importance. Some things are naturally salient for humans because certain physical properties, e.g., a bright color, make them "stand out" perceptually (Berlin, Boster, and O'Neill 1981; Hunn 1976, 1977). Other things may be salient because they are culturally important in some way, e.g., wood in societies in which it is the only fuel. Of course, both natural salience and cultural importance can combine to contribute to overall salience. The salience of a referent, be it natural or cultural or both, can be measured by how often its label is used in a language. Words labeling highly salient referents are frequent in use; those labeling less salient referents are used somewhat less frequently (Witkowski and Brown 1983: 570).

Binomial labels of folk biological taxonomy are overt marking constructions. For example, the binomial label *blue oak* consists of the base term *oak* and a modifier or overt mark, *blue*. This construction nomenclaturally relates two referents, oak and blue oak, which are semantically connected through the relationship "kind of"; a blue oak is a kind of oak. Since binomial labels are overt marking constructions, their referents (biological taxa) typically are of lower salience than biological classes labeled by unitary lexemes.

[17] Wood/tree polysemy occurs in approximately two-thirds of the world's languages (Witkowski, Brown, and Chase 1981).

The percentage of biological taxa labeled binomially in languages of small-scale agriculturalists is invariably larger than that pertaining to languages of foragers. This, in effect, means that there are significantly more biological classes of lower salience in folk taxonomies of cultivators than in those of hunters and gatherers. Another way of putting this is that nearly all biological referents named by hunters and gatherers tend to be of relatively high salience, while for small-scale agriculturalists this is not the case.

A plausible explanation of this finding is suggested by the strong positive correlation of table 4 indicating that as a folk biological taxonomy increases in size, so does the percentage of taxa labeled binomially. As the number of labeled biological classes expands there is, of course, an increase in the number of terms and associated referents to be remembered. Obviously, not all labeled classes in a biological taxonomy, whatever its size, are going to be remembered equally well. In other words, not all biological referents will have the same degree of salience for a people. Given obvious limitations on human memory, it follows that labeled classes of a small taxonomy will be more easily recalled than labeled taxa of a larger taxonomy. Thus, while it may be possible for all labeled classes of a small taxonomy to be of relatively high salience for a people, it is extremely unlikely that such a condition could hold for a much larger taxonomy. Indeed, it seems plausible that more and more taxa will develop lower rather than higher salience values as a taxonomy grows. Consequently, such an expansion will bring about an increase in the percentage of classes labeled binomially.

Since for many small-scale agriculturalists many wild plants and animals are primarily "famine foods"—that is, are intensively collected and hunted primarily during periods of crop failure—it seems likely that most of these organisms would be of relatively low salience for most members of such a group during normal times. This is so because "famine foods" are only intensively utilized occasionally and, hence, are not of pressing importance to an agrarian people on a continuing basis. On the other hand, one would presume that domesticated plants and animals for the most part are relatively high in salience for small-scale farmers, since these constitute their usual source of nutrition. However, evidence assembled by Sillitoe (1980) indicates that it may not be unusual to find that many plants cultivated by small-scale agriculturalists are actually of relatively low salience for them.

One index of the salience of organisms is the degree to which people agree on their names, high agreement indicating high salience and low agreement low salience (Berlin, Boster, and O'Neill 1981). Sillitoe (1980) reports a detailed investigation of the degree to which the Wola of the Papua New Guinea highlands agree on the naming of cultivars. For example, asked to identify nine different taro plants, the replies of 52 men and women taken together show only a 53% agreement with the majority opinion (p. 140). Logically, if all nine taro cultivars were of high salience, disagreement would not occur or, at least, would be minimal. The actual extensive lack of agreement, of course, indicates that some of the taro cultivars are not of great salience for the Wola. Sillitoe (1980: 141) writes that essentially all Wola hold in common a set of cultivar names, but when forced to apply these names to actual plants "they only agree around fifty percent of the time about which name goes with which plant," this indicating that a considerable number of Wola cultivars lack high salience. In explaining these findings Sillitoe calls attention to the marked correlation between the occurrence of different cultivars in gardens and agreement over their identification (p. 142): "The more often certain plants occur under cultivation the greater the consensus over their naming."

Sillitoe's study constitutes documentation independent of binomial evidence of the considerable extent to which named biological organisms are of only lower salience for small-scale farmers. It also suggests a reason this should be the case. The Wola cultivate at any one time only a few of all the cultivars for which they have names. Consequently, cultivars that are seldom planted will be of relatively low salience. Thus the very richness of the Wola's inventory of cultivars contributes to the fact that many are not of high salience. If known cultivars were so few that all could be planted on a regular basis, then few, if any, cultivated plants would have a level of salience other than high. Sillitoe's study, then, supports the conjecture that development of large biological taxonomies significantly expands inventories of taxa that are of lower salience. This, in turn, results in augmentation of biological classes that are labeled binomially.

Hunters and gatherers, of course, have relatively small biological taxonomies, and this, plausibly, facilitates high levels of agreement among such people with respect to association of terms and biological referents. In other words, the vast majority of organisms named by foragers are probably of relatively high salience. However, to my knowledge there are no studies that have directly investigated salience levels of labeled biological taxa in languages of peoples lacking cultivation. Of course, the dearth of taxa labeled binomially in such languages constitutes some evidence, albeit indirect, that very few biological referents are of low salience.

Clearly, some wild plants and animals named by hunters and gatherers are more salient than others. Scudder (1971: 4) notes that most of the vegetable diet of both the Eastern Hadza and the Dobe Bushmen comes from a relatively small proportion of available food plants. For example, while the Dobe Bushmen identify at least 113 wild plants as food, only 9 of these are staples, and a single species, the mongongo nut, is calculated to provide from one-half to two-thirds by weight of the edible vegetable materials. The mongongo nut, then, is obviously of exceptionally high salience for the Dobe Bushmen compared with other wild foods (Lee 1979: 182–204), and the few vegetable staples of both the Dobe and the Hadza are more salient than other wild edible plants that they gather.

On the other hand, there is no reason to believe that nonstaple wild plants eaten by hunters and gatherers are ever especially low in salience for them. Unlike the wild "famine plants" collected by many small-scale farmers primarily when crops fail, most edible wild plants known to foragers—other than vegetable staples—are usually opportunistically collected on a regular basis. There is no evidence that hunters and gatherers ever identify a set of wild plants that are intensively collected principally in times of food scarcity (cf. Scudder 1971: 30) and consequently less salient than plants opportunistically collected on a regular basis, at least in normal times. Indeed, this is understandable, since, as mentioned earlier, wild food supplies of foragers are highly resistant to failure. In short, given the typical small size of biological taxonomies of foragers and the fact that "famine foods" are not recognized by them, there is little reason for believing that considerable numbers of their labeled taxa are ever especially low in salience. This, of course, helps to explain why the percentage of biological classes labeled binomially in languages of hunters and gatherers is typically very low.

One reason that binomial nomenclature is associated with lower-salience referents may be that such composite terms somehow help to increase the human ability to remember names for those referents. Intuitively this makes sense. For example, it would clearly be more difficult for me to recall the names of such species as scarlet oak, shingle oak, post oak, dyer's oak, and so on, if each of these were labeled by its own unitary lexeme. On the other hand, I have no difficulty in remembering names such as *oak, walnut, pine, ash, fir, maple, gum, hickory, birch,* and so on, despite the fact that each of these has its own unitary lexeme. The critical difference here is that in the former case the species involved are of low salience for me while referents of the latter terms are for the most part of high salience. In other words, I can easily and correctly identify an oak, a maple, or a pine, while I cannot distinguish a scarlet oak from a shingle oak or a shingle oak from a post oak, and so on. This suggests a functional reason lower salience organisms are usually labeled binomially: binomial names for lower-salience referents are more easily remembered than unitary lexemes for those referents.

The "rememberability" of binomial labels possibly relates to the fact that overt marking constructions almost always are built up out of terms current in a language and frequently used (i.e., highly salient), for example, *blue* and *oak* in the construction *blue oak* (Brown and Witkowski 1981: 606). The high salience of the constituents of a binomial label, of course, would contribute to its overall salience and, hence, to its rememberability. In addition, the very structure of a binomial label probably enhances the human ability to remember the lower-salience organism it designates. It does so by (1) calling attention to the general (and highly salient) category of organisms (e.g., oak) of which its referent is a member and (2) signaling some special feature of its referent (e.g., a subtle bluish tint) which sets it apart from other members of the general category.

In summary, evidence considered here indicates that binomial nomenclature arises when folk biological taxonomies become so large that it is both culturally and psychologically impossible for all labeled taxa to be of high salience for a people. "Cultural impossibility" relates to the fact that cultures of groups having large taxonomies do not, or cannot, arrange for all biological referents to enter into the everyday experience of people. Thus, for example, "famine foods," intensively experienced only occasionally by many agriculturalists, are less salient than foods eaten on a day-to-day basis. "Psychological impossibility" relates to the proposal that increases in number of labeled taxa result in more and more strain on human memory for them. Consequently, some taxa will be less well remembered than others and, hence, less salient. Thus, for both cultural and psychological reasons, as taxonomies grow there is a significant increase in the number of taxa of lower salience, this resulting in a significant increase in the number of classes labeled binomially.[18] The latter development may be facilitated by the possibility that binomial labels for lower-salience classes enhance the human ability to remember such taxa.

CONCLUSION

Cross-cultural data assembled in this paper reveal significant differences between folk biological taxonomy and nomenclature of hunters and gatherers and that of small-scale agriculturalists. While foragers possess sizable inventories of labeled biological classes, the inventories of small-scale agrarian groups tend to be much larger. In addition, binomial names are very common in folk taxonomies of cultivators but very rare in those of hunters and gatherers.

The explanatory framework developed to account for these findings proposes that subsistence agriculture creates a diversity of ecotypes which supports a range of wild organisms considerably greater than that found in the habitats of foragers. While this diversity provides small-scale farmers with the opportunity to expand their traditional knowledge of and interest in wild plants and animals, it is not the cause of the enhanced importance of wild organisms for agriculturalists. Small-scale agriculture supports population densities many times greater than those permitted by a hunting and gathering way of life. However, a liability of subsistence farming is that crops are susceptible to periodic failure. On the other hand, the food supply of foragers consists of wild plants and animals that are naturally resistant to drought and disease, so that these organisms rarely, if ever, "fail." In addition, given the low population densities of hunting and gathering societies, even in times of scarcity food acquisition need not entail exceptional effort. In contrast, when crops fail severely, the dietary needs of the vastly denser populations of agrarian societies can be met only through highly intensive exploitation of wild plants and animals. This is facilitated by the local biological diversity created through subsistence farming. Small-scale agriculturalists, then, are virtually required to have an extraordinarily broad traditional knowledge of wild plants and animals in their habitats that can be utilized as food.

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[18] In a response to an earlier draft of this paper, Terence Hays has called into question the proposition that binomial labels are primarily associated with biological classes of lower salience. He (personal communication) writes, "it seems that binomialization might be most common in sets of taxa that are *highly* salient, i.e., domesticated plants or animals of which varieties or species (binomially labeled) would have *resulted from* domestication." He builds his case as follows: "I don't think you have adequately taken into account the fact that binomialization is likely to occur in distinguishing among closely related taxa, and that these are especially common with domesticated plants, where botanical diversity (along few or a single dimension) has been *fostered* and then recognized with binomial expressions. . . . [I]t seems that one thing cultivators are doing that foragers are not is that they are 'creating' new forms, and especially new forms at certain taxonomic ranks, viz., those which most commonly are claimed to be labeled binomially. Thus, it is important that one separate wild from cultivated plants. . . . When that has been done, we can say more certainly where and how much foragers differ from cultivators in their classification as well as nomenclature."

In response to Hays I have attempted to distinguish wild plants from cultivated plants labeled in agricultural languages and to ascertain what percentage of each set of plants is labeled binomially. Given the nature of my data, only two languages could be so treated, Hanunóo and Wayãpi. In both cases a greater percentage of cultivated plants is labeled binomially. Wayãpi shows the greater difference, with 54% of 163 cultivated plants having binomial labels versus 17% of the 551 noncultivated ones. In Hanunóo 81% of 483 cultivated plants are labeled binomially, while 58% of 1,142 wild plants have binomial labels. These results clearly support Hays's contention that binomialization most commonly involves terms for domesticated organisms. They do not, however, relate to his other contention that the most highly salient sets of taxa in languages of agrarian peoples are domesticated plants and animals. There is no question that domesticated biological classes at the "generic" rank (cf. Berlin, Breedlove, and Raven 1973), such as American English *corn*, *wheat*, *horse*, and *dog*, are among the most salient biological classes for agriculturalists (Brown 1984*b*: 51–55). However, the precious little evidence that exists (see above description of Sillitoe's [1980] study) indicates that domesticated taxa that partition generic classes and are regularly labeled by binomials are not particularly salient. Empirical investigations may eventually shed light on this matter. In the meantime, I hold to the view that the biological lexicon will prove no different from the rest of vocabulary with regard to the association of overt marking constructions with lower-salience referents, be they plants, animals, or whatever.

If my position is correct, then how does one account for the apparent fact that binomialization most commonly entails terms for domesticated rather than wild botanical organisms? Hays's discussion would seem to provide a clue, that is, that domesticated plant taxa tend to be more "closely related" than wild botanical classes. Consequently, binomially labeled domesticated taxa dominated by the same generic class tend to be morphologically *very similar*, differing from one another only with respect to variables of one, two, or three dimensions at most. It seems plausible to propose that this similarity, especially when larger numbers of closely related cultivars are involved, would tend to lower the salience of taxa. Lower salience, for example, would be manifested by lack of consensus or confusion in distinguishing cultivars from one another. Another manifestation of lower salience, of course, would be binomialization of their labels.

The biological knowledge of small-scale farmers also extends to organisms that have medicinal uses. Studies of archaeological skeletal populations show that agrarian peoples face considerably more health risks than foragers. Consequently, they are more motivated than hunters and gatherers to expand significantly their knowledge of medicinal wild plants and animals. For this reason and for others outlined above, agriculturalists' traditional knowledge of biological organisms is necessarily significantly greater than that of foragers. An index of the enhanced importance of wild plants and animals for small-scale agrarian groups is that their folk biological taxonomies are significantly larger than those of hunting and gathering peoples.

The greater binomialization of folk biological taxa of small-scale agriculturalists is explained as a function of referent salience. Overt marking constructions such as binomial labels tend strongly to designate lower-salience referents. Because of limitations on human memory, the larger biological taxonomies of farmers tend to contain more taxa of lower salience than the much smaller taxonomies of hunters and gatherers. In addition, because of the magnitude of taxonomies of small-scale farmers, many labeled taxa cannot enter into their day-to-day experience, and this contributes to the lower salience of many biological classes. For the reverse reason, the smaller size of foragers' taxonomies may render most included taxa highly salient. For these and other reasons, folk biological taxonomies of small-scale farmers are considerably more binomialized than those of hunters and gatherers.

While I do not presume to view this explanatory framework as final and definitive, I do believe it to be highly plausible and predict that much of it will survive the rigors of further scholarly consideration. In any case, such explanations should be judged independently of the phenomena explained. In other words, whether the explanatory framework is found acceptable as a whole or in part, the cross-cultural data assembled and the patterns to which they attest can stand on their own merits and challenge all to interpret them. Finally, the fundamental importance of these patterns is that they show an exceptionally detailed and intimate manner in which language and culture are related.

APPENDIX

Sources for tables 1–3 are as follows: Aguaruna (Berlin 1976, Berlin and Berlin 1977), Amuzgo (Tapia 1978, 1980), Anindilyakwa (Levitt 1981, Waddy 1982), Bella Coola (Turner 1974), Bellonese (Christiansen 1975), Bontoc (Reid and Madulid 1972), Casiguran Dumagat (Headland 1981), Chuj (Breedlove and Hopkins 1970, 1971*a,b*), Eastern Subanun (Frake 1969), Flathead (Hart 1974, Weisel 1952), Gosiute (Chamberlin 1908, 1964), Gugadja (Peile 1976), Gupapuyngu (Davis 1981), Haida Masset (Turner 1974), Haida Skidegate (Turner 1974), Hanunóo (Conklin 1954), Huastec (Alcorn and Hernández V. 1983), Ifugao (Conklin 1980), Jörai (Dournes 1968), Kiowa-Apache (Jordan 1965), Kootenai (Hart 1974), !Kung (Lee 1979), Kyaka Enga (Ralph Bulmer, personal communication), Lillooet (Turner 1974), Mandarin Chinese (Chao 1953), Mende (Deighton 1957), Montagnais (Bouchard 1973), Ndumba (Hays 1974, 1979, 1983, and personal communication), Nitinaht (Turner et al. 1981), North Saami (Anderson 1978), Nuaula (Roy Ellen, personal communication), Okanagan-Colville (Turner, Bouchard, and Kennedy 1980), Sahaptin (Hunn 1980), Shuswap (Palmer 1975), Tasaday (Yen 1976), Taubuid (Pennoyer 1975), Temne (Deighton 1957), Tobelorese (Taylor 1980), Tzeltal (Berlin, Breedlove, and Raven 1974, Hunn 1977), Wayãpi (Grenand 1980), West Futuna (Dougherty 1983), Yupik Eskimo (Oswalt 1957).

Comments

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Brown has performed a valuable service in drawing together the world's reported plant taxonomies and correlating them with mode of livelihood. This paper, along with the cited work of Eugene Hunn and David French, serves as a pioneering study that will stimulate more precise efforts with more safely controlled and statistically manipulable data.

Surely the most serious problem to be addressed in such follow-up work will be the matter of comparability of taxonomic lists from various cultures. The lists utilized in the present study are hardly comparable. We have some exhaustive recordings of the total plant nomenclature of entire cultures (e.g., for Hanunóo and Aguaruna); lists that are exhaustive for modern speakers but reflect a situation following from one or two centuries of catastrophic acculturation pressure with attendant loss of culture and language (Haida, Okanagan-Colville); partial lists by early observers and/or poorly trained observers (Gosiute); and casual records derived from one atypical speaker (Mandarin, English). Brown rightly does little with his own (English) plant lexicon, but he uses the Mandarin data extensively. Yet the Mandarin case is a single list consisting of the terms that one urban intellectual could recall off the top of his head—and, as Brown's n. 10 states, the man in question was not even a native speaker of the language. No wonder Mandarin Chinese is anomalous in comparison with his other data. I am happy to be able to report that using a more substantial data base on Mandarin would have brought the language more in line with the others examined.

But what, in the end, would be appropriate comparative material? How would we properly treat Mandarin or English? The total number of plant names in Mandarin (more correctly Putonghua, but I will stay with Brown's usage) is well into the tens of thousands, since Chinese botanists and herbalists have provided names for most species of any regional or world importance to them. A practical linguist may object to including recently coined "book names" in the sample, but any good Chinese herbalist of the old school—the sort one can find in any Chinese city and many overseas communities—knows hundreds or even thousands of traditional plant names. Even if we disqualified such specialists, we could find peasants who knew hundreds and peasant communities in which the pooled knowledge would be even greater. In a case like this, what is the appropriate sample? The average peasant? The total names known in one community? The sum total of established, traditional names? Or the whole body of names, including recent coinages for strange and alien items?

The situation is, of course, at least as messy for English, even if we disregard such problems as what to do with Singaporean English or Indian English. Brown reports that he knows 273 plant names in English. I suppose I am an equally typical representative of the same sub-sub-subpopulation of English-speakers (fortyish-age American professors of ethnoscientifically inspired anthropology), and I can recall several thousand; I once ran a small farm on which I noted over 500 plant taxa (wild or cultivated). Clearly the problems of whom we study and how exhaustive our lexicons are to be will have to be addressed at great length in future.

Besides the Mandarin case, the major problem of this sort that must somewhat bias the current study is the fact that many of the hunting and gathering groups have been so shattered by modern impact. Surely we do not have full taxonomies for such groups, some of which were quite large and sedentary. One must await with eagerness the publication of

Janice Timbrook's study of Chumash ethnobotany. The Chumash lived in the plant-rich Santa Barbara area of California and attained high population densities for a hunter-gatherer people. Their plant lore was already fragmentary but still impressive when recorded by John Peabody Harrington shortly after 1900. I am confident that in plant taxonomy, as in many aspects of social structure, the Chumash would have been more like settled cultivators than like small hunting and gathering bands. Studies of such groups can resolve questions of exactly what are the relevant variables—population pressure on the resource base, number of plants in the area, level of sociocultural integration, amount of specialization (herbalists, for instance), and the like. I do not think Brown's general conclusions will be overturned. There is much refining to do, however, especially in regard to which variables explain the most.

Finally, I wonder at the criteria used to select sources. Brown notes that "the procedure was exhaustive" (n. 4) and has told me that this was the reason for including even the absurd item by Chao (valuable in its way but not for this study). If Chao's paper qualifies, why not such a highly sophisticated, exhaustive, brilliantly executed study as Bean and Saubel's *Temalpakh* (1974)? Why not the Tewa ethnobotany of Robbins, Harrington, and Freire-Marreco (1916)—admittedly old, but far ahead of its time and an inspiration for the modern ethnoscientist? Why not any of several dozen other works like these?

So much for suggestions for future increments. The importance of the present study lies not only in its interesting conclusions but in its demonstration of what can be done with folk taxonomies, now that we have a significant number on record. It appears that folk taxonomies and nomenclatural systems correlate with the traditional Morganian categories of human society. I remember many denunciations of ethnoscience by latter-day "cultural evolutionists" a few years back: I hope the irony is not lost on them. There is irony the other way too: such a quintessentially Boasian enterprise as nomenclature-collecting bids fair to give aid and comfort to the evolutionists—up to a point. I hope this serves as a lesson to all anthropologists who would substitute controversy and confrontation for thinking. Complementary agendas should never again become excuses for uncomplimentary remarks.

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Brown's attempt to quantify and explain the puzzling differences in scale and structure between folk biological classifications reported from hunter-gatherer societies and those from small-scale agriculturalists is welcome. But, with regard to scale, his tabulations almost certainly exaggerate the contrast that might reasonably be expected, because the two main groups of studies included are not closely comparable, for reasons Brown either fails to consider or underestimates. The small-scale agriculturalists appear (1) all to have had at the time of ethnographic study viable and diversified subsistence economies, despite recent involvement of some in cash-cropping and migrant labour; (2) to occupy, in most cases, biologically rich and diversified environments in humid tropical or subtropical regions; (3) to be members of language or dialect groups numbering, in most cases, thousands rather than hundreds of persons; and (4) to have been, in many cases, subjects of intensive and prolonged research by professional ethnographers with a special interest in folk biology. In contrast, the hunters and gatherers in Brown's tables (1) have in most cases been subjected to several decades or even generations of highly disruptive change in their traditional subsistence economies and are not only "involved to some extent in plant cultivation and/or animal husbandry" but, more significantly, are in receipt of wages, welfare checks, or other cash payments and obtain much of their food supply from stores; (2) occupy a great variety of environments, most of which are significantly less rich and diversified biologically than those of all the well-reported agriculturalists; (3) represent speech communities numbered in hundreds, or even tens, rather than in thousands; (4) in some crucial cases have not been subjects of intensive and prolonged research by specialists in folk biology. Leaving, for the moment, this last point aside, the case of the Anindilyakwa-speakers of Groote Eylandt is instructive. Good modern reports suggest that they, more than most Australian Aboriginal groups, have retained not only their enthusiasm for traditional subsistence activities but a great deal of knowledge associated with these. Indeed, the ratio of their 220 reported plant taxa (Waddy 1982:70) to the approximately 600 botanical species so far identified in their 2,400 km^2 traditional territory (Levitt 1981:[v]) is higher than that of 543 folk taxa for wild plants to approximately 1,600 botanical species in the 135 km^2 territory of the Tenejapa Tzeltal (population ca. 10,000) reported in Berlin, Breedlove, and Raven's (1973) magisterial study. Yet the Groote Eylandters' subsistence economy and traditional socialisation practices have been progressively affected by European agencies since the 1930s (when the population was approximately 300), and by the 1950s it was reported that "people only 'go bush' for very limited periods, spending most of their time working at either the mission station . . . or at the secular settlement" (Worsley 1961:156; see also Levitt 1981:8–9; Rose 1960:13–14 and passim). I find it hard to believe that by the 1970s, when Levitt's and Waddy's valuable studies were undertaken, some attenuation of environmental knowledge and vocabulary had not occurred; and I would argue that, to an even greater extent, the same attrition has affected the folk classifications of the North American hunter-gatherers that constitute the majority of cases in Brown's tables. From outside Australia and North America the only hunting and gathering groups in these tabulations are the !Kung San and two Philippine societies. The !Kung, described as "superb botanists and naturalists" (Lee 1979:158), do not inhabit a region that is rich in flora, but it must also be noted that while Lee's ethnobotanical enquiries focussed intensively on plants used for food and technological purposes, he appears not to have attempted a comprehensive folk botanical study. The two Philippine cases are regarded as particularly important by Brown because they are groups occupying botanically rich rain-forest areas. For Casiguran Dumagat, Brown (n. 8) finds it necessary to contradict the author's own modest statement about the limitations of his study. The report on the Tasaday (Yen 1976) is by a highly competent ethnobotanist but is based on no more than six weeks' fieldwork, using an interpreter. It further appears that Yen was (very rationally) primarily concerned to identify and collect *plants*, not, as Brown was in his four-day Huastec study, to elicit *vocabulary*. But even if a later and comprehensive enquiry should demonstrate that Yen did manage to record a large proportion of the total number of Tasaday plant taxa, the fact that there were at the time only 13 adult Tasaday and that the total size of their speech community, including as yet uncontacted groups, seems unlikely to be larger than a few score persons might surely be expected to have some consequence for the scale of their lexicon in folk biological as well as other domains.

I therefore believe that while Brown's figures give a fair impression of the scale of folk biological classifications within a particular category of small-scale agriculturalists (it would be interesting to have tabulations of equally well-based figures from small-scale agriculturalists in arid or other less favoured environments), they tell us little about the scale, or varying scales, of folk classifications in societies in which life still essentially depends on hunting and gathering and the extent to which these may relate to habitat differences and to size of speech community as much as they do to mode of subsistence. Thus speculations about historical processes of transition from one scale of classification to another as populations move from hunting and gathering to agriculture are idle.

The proportions of binomically labelled taxa in different folk classifications may well, as Brown suggests, be rather directly related to the scale of the classifications (? or related lexicons) concerned. In this regard, the extent to which a hunter-gatherer classification may have suffered attrition prior to its investigation need not, perhaps, affect the argument. However, there are difficulties in the definition of "binomial labels" and notably in the identification and categorization of *optimal* binomials. The sole case that Brown considers is *oak tree,* and he excludes this not because it is an optional variant to *oak* but because he regards "tree" as a "major 'life-form.' " Apart from raising the contentious issue of whether or not the concept "life-form" (let alone "major" and "minor" life-forms) can be precisely enough defined for use in cross-cultural comparisons, there are numerous optional binomials in English which incorporate terms that not even the most ardent pro-lifeformer would accept. For example, sets within the class *horse* include (*contra* Brown n. 6) not only a minority of taxa labelled by such mandatory binomials as *race-horse* and *cart-horse,* but others labelled by optional binomials such as *thoroughbred (horse), bay (horse), Arab (horse),* and yet others such as *pony* and *hunter* labelled by strictly monolexemic terms. Other examples are such forms in English as *briar (rose), humpback (whale), shorthorn (cow),* and *rainbow (trout).* In Kyaka Enga and Kalam (both New Guinea Highlands) folk taxonomies for plants and animals, optional binomials occur at every level except that of most inclusive labelled taxa. Thus for Kalam I have recorded approximately 492 labelled taxa for animals, for about 120 of which the most usual labels are unambiguously monolexemic, 217 have optional binomials, and 155 have mandatory binomials (cf. Majnep and Bulmer 1977:46–47). A much rougher estimate for 466 Kyaka Enga animal taxa (my Enga folk biological enquiries were not as extensive or careful as my later Kalam studies) is that these are labelled by 66 monolexemic terms, 302 optional binomials, and 98 mandatory binomials. In both languages a high proportion of mandatory binomials are also optional trinomials, though the use of these is less frequent and idiomatic in Kalam than in Kyaka Enga. While Brown would doubtless wish to dismiss many of the optional binomials on grounds that they incorporate life-form terms, I would argue that the line between life-forms and other primary taxa in these, as also in English, folk classifications can only be quite arbitrarily drawn.

The extent of both optional and mandatory binomial usage in hunter-gatherer languages might also in some cases be masked by the fact that incorporated class terms are regarded by the investigator not as "biological" but as marking categories of food or technological significance. Thomson's (1946) brief account of Wik Monkan (Australian) folk biology raises this problem and, like the extensive use of optional binomials in Kalam and Kyaka Enga, also appears to provide evidence running counter to Brown's plausible suggestion that binomials are an aid to memory in distinguishing organisms with low natural salience and/or cultural significance.

It would not be surprising if there were significant differences between the ordering of folk biological classifications by small-scale agriculturalists and those by hunter-gatherers, and also between those of different subgroups in both categories. Allowing for the qualification Morris (1976) suggests, there is a tendency for religious systems of the former to focus on what Radcliffe-Brown (1952:126) long ago referred to as "the ritual relation of man to nature," whereas those of many of the latter give main emphasis to relations with ghosts and ancestors. Thus we might expect differences in folk classifications to relate to different symbolic roles of plants and animals as well as to differences in means of subsistence. Brown's paper encourages us to look more closely at the evidence, and in wider contexts than that of his initial survey.

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Brown's contribution is interesting, especially where it throws light on aspects of the evolution of folk taxonomies, but something important is missing. His analysis may be correct, but like so many other taxonomic studies it is lacking in theoretical orientation, or, worse, inspired by a scholastic ontology based on *substance* rather than *production.* The paper deals with *modes of subsistence,* but in my view it would have been more appropriate to speak of *modes of production.* The concept of production would allow us to recognize something which is obscured by the concept of subsistence, because production is essentially *dynamic,* while subsistence is not. The study of lexemes and their taxonomic order can indeed be useful, but only if it aims at unearthing the process which generates the taxa in the first place. Since Durkheim and Mauss (1963), the study of classification has focused unduly on the priority of society over the individual and his behavior. Classifications viewed in the light of an ontology based on substance have separated the product from the producer. Representations(= taxa) have been separated from the act of representing(= naming). But the taxonomic orders were initially created in social practice, and they may be dismantled and transformed when the need arises.

Therefore, I propose to think of *action* and *production* when one speaks of *modes of subsistence.* The biological lexemes of which Brown speaks are parts of processes of production. Production and action are linguistically expressed through *verbs.* Why does Brown neglect the verb? What are we supposed to do with taxonomies of nouns if a taxonomy of verbs is missing? One wonders what the results of Brown's analysis might explain. One understands from it that small-scale agriculturists have a more embracing and more systematic view of their environment than hunters and gatherers, but what next?

It is possible that Brown in a subsequent paper will arrive at an explanation of the taxonomies he has described. He indicates that he plans to do so, and I am curious to see how he will proceed. I hope he will show that what distinguishes hunters and gatherers from small-scale agriculturists is the way they produce their livelihood. The different biological taxonomies would then be explained in terms of the differences between the modes of production on which they are based. The activities entailed in production are expressed by verbs. As Fillmore (1968) has shown, the verb regulates the nouns. Verbs also may be ordered taxonomically (see Ballmer and Brennenstuhl 1981). Further ideas in this direction can also be found in Tyler (1978) and Casson (1983).

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Brown argues that plant domestication leads to an expanded inventory of biological terms and an increased degree of binomialisation as a device to handle the larger amount of information. He suggests that the latter is likely to occur when low levels of salience become preponderant, as is the case for nondomesticated plants among small-scale agriculturalists. However, he accepts (following Hays) that binomialisation may also occur among closely related high-salience plants once domesticated and that domesticated plants may be as low in salience as many nondomesticated ones. Apart from the fact that we seem to have a Panglossian solution in which a balanced consideration of all possible factors affecting the general global course of linguistic change is in danger of obscuring what actually goes on in particular places, it seems to me unwise categorically to contrast domesticated and nondomesticated plants when we know that in many cases the distinction at the level of individual species is difficult to make, when the specific historical processes of domestication are known to be empirically complex, and when botanical families and genera cut across the dividing-line. Binomialisation is a matter of degree, and as plants are domesticated, or as domesticated plants are adopted, the terms for them and their classificatory relations will be constructed with reference to preexisting terms and schemes used for wholly nondomesticated species. One consequence of this is likely to be that, at least initially, domesticated forms of species found in the wild or closely related to species found wild will tend to be marked. Thus, speakers of Nuaulu, a language of Seram in eastern Indonesia (Ellen 1978), use the world *naha* to refer generically to a number of species of wild rat that are not further differentiated terminologically. However, *Mus musculus* (the common commensal house mouse) is termed *naha numa* (lit. "house rat/ mouse"). It would appear that as cultivation gets established and (quite literally) grows in importance, so marked terms become increasingly those associated with wild rather than domesticated forms, as in the Ambonese Malay *cengkeh* and *cengkeh hutan* ("clove":"forest clove") or *keladi* and *keladi hutan* ("taro":"forest taro"). I also suspect that the rise in binomials in plant terminologies for cultivators has at least something to do with the division between "wild" and "cultivated" itself. There is bound to be a tendency to link similar species and varieties found wild with those planted. Terms such as "wild," "garden," "forest," "house," "mountain," and "village" all act as common adjectival qualifiers for binomials found in Nuaulu, as in many other languages of archipelagic Southeast Asia. If you divide the natural world in a way that places "related" species in contrasting categories, binomials are very likely to emerge.

The famine-food explanation for the large inventories of wild plants among extensive cultivators must be placed in proportion. If we look at the Nuaulu evidence, we find that "famine" foods are relatively rare, although the people are permanently dependent on a high level of extraction of nondomesticated species (Ellen 1978:61–80). Virtually all Nuaulu animal protein comes from wild sources, there being no sustainable and dependable relations of domestication. Consequently, the famine-food hypothesis cannot apply to animals except insofar as these species might be seen as a means of supplementing vegetable foods. The most obvious Nuaulu "secondary" foods are wild tubers (5–7 terminal categories) and palm sago (14 terminal categories). Two species of yam (*Dioscorea pentaphylla L.* and *D. hispida* Dennst.) and the palm *Metroxylon sagu* Rottb. (11 terminal categories) occur under both domesticated and nondomesticated conditions. Together these constitute about 3% of the total number of plant categories. By comparison, there are 28 terminal categories applied to cultivated species of yam and taro alone (Ellen 1973:450–64). But it would be misleading to regard these as "famine" foods in any strict sense. *Metroxylon,* itself largely undomesticated, is by far the most important Nuaulu source of carbohydrate. Other palms (e.g., *Nypa fruticans* Wurmb. or *Oncosperma tigillarium* [Jack]) are cut for their starch only if suitable *Metroxylon* palms are unavailable. The vast majority of nondomesticated labelled plant species are useful trees, rattans, and bamboos. Of the 700+ Nuaulu terminal categories for plants I have obtained, about 238 are for trees (excluding palms and pandans). Medicinal plants that are not also domesticated, foods, or sources of manufacturing materials comprise only a small group, and these are generally herbs and shrubs on the edges of cultivated areas, sometimes weeds of cultivation, and not species common to the rain forest. I suspect that the famine-food hypothesis will work well only where cultivation is chronically unreliable or where such foods comprise a significant proportion of nondomesticated plant species.

Brown argues that plant cultivation leads to an expansion in the inventories of animals as well as plants and a higher degree of faunal binomialisation. He does not adequately explain why this should be so, but, as I have indicated, the role of animals as famine foods appears rare under tropical forest conditions. Most ethnozoological inventories among nonmaritime peoples are dominated by insects and birds, and these are precisely the groups that are likely to become pests of cultivated crops. Thus, a more likely explanation for the increase in number of animal terms is that a sedentary and cultivating mode of existence (in which reliable harvests and storage are essential) is accompanied by the experience of "vermin" and "pests." By the same token, the rise in the number of plant categories must be at least partially explained by the requirement to distinguish and extinguish "weeds." Forty-three Nuaulu terminal categories (and about the same number of phylogenetic species), about 6% of all labelled plants, are regarded as weeds. The number is probably far higher amongst peoples whose cultivation techniques are more intensive, technically complicated, and exclusive as a means of producing food.

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Brown addresses issues of great importance in ethnobiology and linguistic anthropology and demonstrates that distinguishing among societies in terms of their mode of subsistence is a fruitful step toward understanding *patterned diversity* as well as "universals" in folk biological systems. Some other important distinctions are not made, however, in the rather crude analytic scheme employed.

As Brown acknowledges, the cases used constitute an opportunistic rather than a randomly selected sample of world societies; thus, representativeness cannot be assured. Also, a gross dichotomy of "hunters and gatherers" vs. "small-scale agriculturalists" cannot adequately reflect the diversity contained in the sample. Rank-ordering cases in terms of relative dietary contributions of hunting, collecting, and cultivation might yield a continuum against which true tests of covariation could be conducted.

Brown has wisely chosen to rely primarily on sources resulting from systematic, ethnobiologically focused investigations. However, this does not in itself guarantee strict comparability of data with regard to his key variables. Some of the lexicons and folk taxonomies, as reported, may not be exhaustive while others may include too much, as when the inclusion of synonyms significantly increases the size of lists of names and taxa. Also, sources vary as to whether they report "shared" or "composite" taxonomies and lexicons, yet these can differ substantially, especially in size (see Hays 1976, 1983). These considerations assume special importance when one is trying, as Brown is, to relate folk biological knowledge to utilitarian concerns of its users (Hays 1982).

Several problems plague any consideration of folk biological nomenclature, particularly regarding binomialization. First, despite Brown's folk English examples, distinguishing between proper composite names and descriptive phrases is sometimes exceedingly difficult even when one knows a given system well. We can hope but not assume that all of Brown's source authors have been conscientious in this matter. Perhaps more important is the question of whether one includes optional as well as obligatory binomial expressions. The Ndumba figures for table 2 would be either 39 or 17, a difference in magnitude comparable to that Brown is claiming between small-scale agriculturalists and hunters and gatherers. He has obviously chosen to include only obligatory binomials in the Ndumba case, but has sufficient information been available to make the decision *in the same way* for all of the other cases as well? Finally, since binomialization appears to occur almost exclusively with folk taxa of subgeneric ranks, Brown's analysis might more appropriately focus on the variable sizes of folk taxonomies at these ranks only rather than dealing with the total numbers of named classes.

In fairness to Brown, it must be said that he is constrained by the nature of the ethnobiological literature, in which most of the problems mentioned above are seldom addressed explicitly. If we grant his premise that the cases represent broadly comparable data, it does appear that some striking patterns emerge. Accounting for these patterns is another matter.

To argue that lexicon and taxonomy sizes are direct reflections of people's "interests" is intuitively reasonable, but it entails some difficult operations, e.g., controlling for variable biotic diversity in the environments of the peoples being compared and developing ways to assess and measure "importance" or "salience" of the organisms being classified and named. Since much of the botanical diversity encountered by agriculturalists is of their own creation, Brown's decision to compare folk taxonomies of *wild* plants makes his case stronger than would otherwise be the case, although systematic comparison of larger numbers of cases is still needed. He has not solved the problem of measuring "importance" or "salience," however, referring sometimes to frequency of word usage and at other times to vague economic notions.

Despite these methodological difficulties, Brown has produced a number of testable hypotheses and an imaginative attempt to determine *why* the "folk" order their biological worlds as they do.

by THOMAS N. HEADLAND
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Brown's argumentation is based partly on my data from Casiguran Dumagat (or Casiguran Agta), a Negrito hunter-gatherer group in the Philippines (Headland 1981, 1983). The main contribution I can make, therefore, is to clarify the number of plant labels in the Dumagat language and the percentage of those labels which are binomials. I will also comment on the problems of drawing from other scholars' data in order to prove hypotheses for which those data were not intended.

Readers of Brown's paper may be led to two assumptions concerning the Dumagat data: that it is my thesis that the Dumagat language has "over a thousand" plant terms and that in reality there are only 208 such terms. Both assumptions would be incorrect. In his effort to fit my data into his model, Brown has seriously misinterpreted my study of Dumagat folk botany. He says (n. 8) that "Headland (1981) observes . . . that there may be vastly more labeled plant classes in the [Dumagat] language than the 208 he reports, perhaps 'over a thousand.' " I never "observed" or argued that there are "over a thousand" but only said that there could be that many (Headland 1981:35, 1983:115), and the term "vastly" is his, not mine. Further, it is not the case that "the Casiguran Dumagat . . . have a botanical taxonomy consisting of 208 labeled plant classes." I never "reported" a total of 208 elicited Dumagat plant classes. On the contrary, I have stated repeatedly that the 208 taxa used in my study were only a sample of the known Dumagat plant lexemes (Headland 1981:20, 21, 35, 97; 1983:109, 111, 113, 115, 116, 120). The goal of my study was to discover how the Dumagat arrange their plant taxonomy, not to determine the number of plant classes in their language. If Brown had gone to the admittedly tedious effort of counting the plant lexemes in our dictionary (Headland and Headland 1974), he would have found there a total of 261. Furthermore, 60 of the 208 plant terms listed in my Master's thesis (Headland

1981) do not appear in the dictionary. The total number of Dumagat plant lexemes published in the dictionary and my thesis is 321.

Brown sent me a copy of his paper in March 1984, and because my wife, Janet Headland, and I were in the field at the time, we decided to spend a few days eliciting additional Dumagat plant terms. If Brown could elicit 358 Huastec labelled plant classes in just four days, without being fluent in that language (see n. 8), I felt we ought to be able to elicit many more Dumagat plant terms than the 321 we had, if such existed, in an equivalent amount of time. We worked with nine Dumagat assistants on eight days between March 29 and April 15. During that time we went on several walks through the various biomes in the area, accompanied by one or more of these assistants. The assistants listed orally for us all the plants they could identify and name. We rechecked all the terms we were given on these trips with other Dumagat in the camp in which we were then residing and accepted as valid Dumagat lexemes only those terms recognized by at least two other Dumagat.

Many of the terms given to us were already among the 321 plant lexemes in our dictionary or my thesis. However, we collected 282 new terms, thus bringing to 603 the total number of Dumagat plant terms we have recorded. All 603 have been checked and verified with multiple Dumagat adults. (We identified eight paired synonyms among these terms, so the number of actual plant classes would be slightly less.)

We now know that one hunter-gatherer language has at least 603 plant terms. This fact presents a serious challenge to Brown's hypothesis. The question remains how many more Dumagat plant labels could be found with further elicitation. A conservative estimate for the total plant-label inventory of the Casiguran Dumagat would be 700–800. An investigator possibly could, if he worked with a single older informant who was a "specialist" (e.g., a folk healer), elicit over 1,000 plant terms, but many of those terms would likely be unknown to others in the population.[1]

Of the 603 Dumagat plant terms, 394 label uncultivated plants (65%), 179 cultivated plants (30%), and 30 plants that are ambiguous or semicultivated (5%).

Do the languages of hunter-gatherer societies lack binomial biological taxa? The leading theoretician in the area of folk biological taxonomies, Brent Berlin, has proposed that the use of binomial specific taxa is a linguistic universal (Berlin, Breedlove, and Raven 1973:218, 221, 222, 224, 240; 1974:27; Berlin 1972; 1976:390; 1978:20). To my knowledge, only two researchers besides Brown have criticized this aspect of Berlin's model (Hays 1983; Headland 1981, 1983), although Berlin himself has had some later reservations (1976:392; 1978:20–21). I myself raised the question whether languages lacking binomial specific taxa might be limited to those spoken by hunter-gatherer societies (Headland 1983:54–55), but I had no access to comparative data for pursuing this hypothesis. I only knew that Dumagat lacked binomial plant taxa. I therefore find Brown's discovery of the apparent lack of binomials in the biological taxa of several hunter-gatherer societies both exciting and possibly significant. As Brown shows in his table 1, only 2.4% of the 208 plant taxa listed in my thesis are binomials. Of the 282 new plant terms we collected in April 1984, 21 (19 of which were wild) were definitely specific taxa, and none were binomials. If the languages of other hunter-

[1] I have long been critical of ethnoscience investigators who elicit semantic data, or lists of plant taxa, from a single informant both for this reason and because informants have been known to coin botanical terms on the spot to please the investigator (e.g., Reid and Madulid 1972:1, n. 1).

gatherers also lack binomial taxa (which is still an open question), the question is why. Brown has presented a plausible hypothesis for answering this question, and I hope those of us in the field will test it. There are, however, other hypotheses. I have outlined four elsewhere (Headland 1981:30–36; 1983).

The main weakness of Brown's argument is that, to build his case, he has drawn from the data of other researchers in order to prove hypotheses for which those data were not intended. For example, Brown cites data from several hunter-gatherer languages whose lists show very low percentages of binomial biological terms. It is possible and in fact quite common, however, for investigators to collect long lists of plant terms without eliciting any of the specific-level taxa, because informants tend to give only generic terms when they are asked the names of plants or animals. Generic taxa are, in any language, composed almost exclusively of monomials, while specific taxa are, in most languages for which data are available, mostly binomials. Only if an investigator works through a people's folk taxonomy, using a method such as Black's (1969:174), will he find the specific taxa.

The question to ask, then, is not what percentage of a people's biological taxa are binomials, but what percentage of the *specific* biological taxa are binomials. For Brown to substantiate his hypothesis that hunter-gatherers lack binomials in their biological terminology, he must, in my opinion, limit himself to data which discriminate specific from generic taxa and then calculate only the percentage of the specific labels which are binomials. Generic taxa must be excluded from his statistical calculations in every language compared.

Brown's paper has shortcomings, some of which I have pointed out, and if he has misinterpreted the data of others as badly as he has mine his arguments stand on weak foundations indeed. He is, however, the first to admit that his findings are far from final and definitive. He clearly wants his arguments to be considered as hypotheses to be tested, not as conclusions. I believe he has focused our attention on some important questions concerning hunter-gatherer world view and linguistic universals. If he can work out the discrepancies, the ultimate result should be a very significant theoretical contribution.

by Leo Howe

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I shall restrict myself to comments on the presentation of data in Brown's article. There are at least four criticisms which can be made.

1. The base data given in the paper consist entirely of aggregate totals, and there is nowhere any discussion of the possibility of extensive variation in the knowledge of different individuals. If such variation exists, each person's stock of knowledge may well be much smaller than the aggregate, and hence Brown's argument that increased binomialisation is in part due to limitations on memory capability could be entirely spurious. It is not good practice, without further substantial evidence, to employ psychological capacities in the explanation of such gross social statistics.

2. The very high aggregate totals of some of the societies mentioned may in part be due to extensive knowledge of ritual experts and those filling other specialist roles. Should that be the case it would seriously misrepresent the knowledge of the average member.

3. While the difference in average total inventories between hunters and gatherers and agriculturalists is impressive, the range of variation within each of the two categories is equally impressive. After all, the number of labeled botanical taxa of the Ifugao is six times that of the Chuj, the Bontoc, and the Amuzgo. I would like to have seen some discussion addressed to this point.

4. As far as labeled botanical taxa are concerned, the aggregate totals of the bottom five agricultural societies are not significantly different to those of the foraging societies at the top of their list. Certainly the cut-off point betwen societies practising small-scale agriculture and those based on foraging is much more obvious in the case of the extent of binomialisation than it is in the case of number of botanical and zoological taxa.

In short, the emphasis on aggregate totals and average inventories serves to obscure not only the possibility of wide individual variation within particular societies but also the obvious variation between societies in each category. Until some of these questions are cleared up satisfactorily, I cannot agree with the author that these "cross-cultural data . . . . can stand on their own merits and challenge all to interpret them."

by David C. Hyndman

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Brown's title leads one to assume that he treats mode of subsistence as a critical variable in determining the way peoples categorize and name biological organisms. He does in fact claim that biological taxonomies are fundamentally different for "folk" who practice a hunter-gatherer mode of subsistence. However, he does not take mode of subsistence seriously. He admits that the societies he refers to variously as hunters and gatherers and as foragers are not uniform with respect to details of mode of subsistence (n. 2), and Mandarin Chinese and Americans are included in the "small-scale agriculturalist" category even though it was specifically to exclude "peoples of nation-state societies who practice intensive agriculture entailing such features as monocropping of high-yielding varieties and use of pesticides" (n. 2). Moreover, the data presented in tables 1, 2, and 3 and in the appendix are inadequate to permit the reader independently to assign societies to their appropriate modes of subsistence.

Setting up a controlled cross-cultural comparison between societies that contrast on the dimension of domesticated vs. nondomesticated resource use is difficult because the continuum between these extremes is quite large. Nontheless, a minimum requirement is that all societies selected should be indigenous peoples who live in small-scale societies with a common territory and whose subsistence is based on kinship and customary rights and obligations. That indigenous peoples share ancestral land and claim exclusive rights to it and its resources and earn their living from internal production and circulation of local resources rather than from market exchange of labour and resources is ignored in Brown's paper.

Uncritical acceptance of data also invalidates Brown's claim that certain types of taxonomies are typically associated with particular modes of subsistence. For example, Brown accepts the recency of Ndumba agriculture. Agricultural evolution in the New Guinea highlands is the subject of debate (see Watson 1965, 1977 and Brookfield and White 1968), and the work of Golson (e.g., 1977, 1982) certainly establishes the antiquity of agriculture in the highlands at over 10,000 years. Again, Brown accepts the San as being pristine hunter-gatherers, even though Schrire (1980) has produced an ethnohistoric account of modern San moving in and out of foraging, herding, and farming. The assembled data and the patterns to which they attest do not stand on their own merits, as asserted by Brown. Rather, they collapse because of poor survey methodology.

by Knud-Erik Jensen

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I agree with Brown that the possibility of significant differences between folk biological taxonomies of hunter-gatherers and agriculturalists should be explored and find the idea of a possible correlation important and exciting. I disagree, however, with much of his argument and evidence, for the following reasons:

The implied difference between hunter-gatherers and small-scale agriculturalists is often virtually nonexistent in reality, although the dichotomy may continue to be convenient for anthropologists. Today we know that the term "hunter-gatherer" does not imply any specific type of social organisation, economy, political system, etc., and the same can be said for "small-scale agriculturalist." Without any clear-cut criteria of classification or means of distinguishing between types, it seems less than logical to infer that mode of subsistence is the primary factor determining the size of folk biological taxonomies, although the result is probably correct.

Brown is aware of the pitfalls of a nonsystematic sampling procedure (n. 4) but does not seem to fear that a possibly strongly biased sample may affect both the evidence compiled and the explanations offered.

In short, I agree with the data put together by Brown may show some pattern and that this could be of importance to us in the study of language and culture, but I suggest that the evidence and the explanatory framework are far from satisfactory, mainly because the individual parts are rather ambiguous and partly because many of the inferences and presuppositions can be neither verified nor falsified from the evidence offered.

by BRIAN MORRIS
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In an early article of mine (1976) on the natural taxonomies of the Hill Pandaram, a foraging community of South India, cited by Brown at the beginning of his paper, I tried to express two essential ideas—both of which provoked critical comment. One was to question the universality of the "savage mind" paradigm, interpreted as an ideologic that unites into a totality, and in a systematic way through classificatory symbolism, diverse cultural domains. The Hill Pandaram, I suggested, had no such formalization of culture, compared, that is, with other tribal communities. Some anthropologists strangely interpreted this as implying that the Hill Pandaram had no culture and that I had somehow discovered a group of nonhuman humans (cf. Myers and Gangloff 1978).

The other idea was that the Hill Pandaram had a relative lack of interest in folk classifications and, compared with the communities I knew well (the Chewa and Lomwe peoples of southern Malawi), a fragmentary and unsystematic knowledge of their natural environment, as this was expressed in formal taxonomies. My ideas were somewhat impressionistic, but since I had lived in Malawi for more than seven years and spoke Chewa fairly fluently, they were not entirely groundless. In a lengthy critique of this paper, Berlin (1978) argued that the Hill Pandaram did have a "structured system of ethnobotanical classification" and utilized the same cognitive strategies as other peoples in classifying the biological universe. Such tenets I had simply assumed in addressing an anthropological audience, though I took pains—lest I be misunderstood—to stress that the Hill Pandaram "have a fairly detailed taxonomic system which orders the natural world" (1976:552). Entirely overlooked in this critique was the substance of my paper, namely, the *contrast* between the folk taxonomies of a foraging community like the Hill Pandaram and those of the agricultural peoples of central and southern Africa. It is, then, gratifying to read Brown's interesting paper, in which he demonstrates in a detailed and wide-ranging survey what I had hinted at in my paper. Indeed, I have recently reiterated these thoughts, having since undertaken further ethnobotanical researches among the Chewa. Contrasting the botanical knowledge possessed by African peasant communities with that of the !Kung, I wrote (1984*a*:5),

> Like the Hill Pandaram, who are also hunter-gatherers, the !Kung are "superb botanists and naturalists, with an intimate knowledge of their natural environment" (Lee 1979:158). Their knowledge of edible plants is particularly impressive. Yet only one species of mushroom is eaten by them, and of the two hundred species of plants known to them, only seventy are noted as having medicinal value.

In contrast, over 60 species of edible mushroom are known and named by the Chewa (Morris 1984*b*), and in an unpublished study of Chewa *medicinal* plants I recorded 1,550 vernacular names. With respect to the 1,406 plant species recorded, the following details may be of interest: no name or cultural use, 304 species; no local name but cultural uses in Malawi or neighbouring territories, 174 species; named but no cultural uses recorded, 89 species; named and used locally as medicine, 551 species; named but not used locally as medicine; used medicinally elsewhere, or used otherwise, 288 species.

Having undertaken research studies in both a foraging and a peasant community, I find Brown's paper of particular interest beyond the fact that it confirms, by means of a substantive cross-cultural analysis, my own tentative thoughts. I am thus in substantial agreement with his main thesis and the various suggestions he puts forward to account for the significant differences which clearly exist between the taxonomies of foragers and small-scale agriculturists. This discussion, I think, though it draws widely on earlier studies, is both seminal and important. But I will make three points.

First, although Brown stresses the importance of food gathering among small-scale agriculturists, particularly with respect to problems of seasonal scarcity and famine, such foraging, it seems to me, is centrally focussed on the almost *daily* gathering of proteins or such relish vegetables as make the staple—rice, millet, maize, cassava—more palatable. The importance of insects, small mammals (rodents), fungi, and leaf vegetables as part of the regular diet is often overlooked. The Chewa, for instance, have an extensive taxonomy relating to edible insects and small rodents, and with respect to the digging and trapping of the latter there is a rich and complex vocabulary. This is why trapping and snaring are crucial to cultivators and generally lacking among foragers, whose nomadic wanderings generally provide an ample supply of protein (see Morris 1982:79–80). In another article I stress the importance of mushrooms among Bemba and Chewa women and note that during the height of the rains as much time is spent on gathering these as on hoeing gardens and leisure activities.

Secondly, it is worth highlighting Brown's suggestions regarding the importance of plants as medicines among agricultural peoples. In many African communities "plant" is a polysemic term meaning also "medicine," and in his study of the Azande Evans-Pritchard mentions walking along a path for about two hundred yards and collecting about a hundred plants used by the Azande to treat diseases and lesions—more than the entire !Kung and Hill Pandaram pharmacopoeia.

Finally, I think it important not to overstress the subsistence mode in undertaking comparative analyses of different ethnobiological taxonomies. In terms of botanical life-form categories and in their seeming disinterest in the fungi, the Hill Pandaram and the Tzeltal are similar, and both contrast markedly with the Chewa. The degree to which functional criteria have their "impress" on folk taxonomies and the extent to which symbolic classifications unite different folk domains also seem not to correlate with the divisions in terms of productive mode.

by DAVID REASON
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Brown's is a bold and imaginative study. It discusses imaginatively an explanatory framework for the relation between classification and mode of subsistence which derives from the ecology and pathology of small-scale agricultural societies. I welcome the hints of an illuminating treatment which may be obtained by considering these issues in terms of an economy of time, and I was pleased to have my attention drawn to the chronic precariousness of small-scale agricultural surpluses: here indeed is a material support for the development of institutions of power and inequality.

The paper is bold in its cavalier handling of philosophical/methodological niceties. As the author acknowledges, there are many problems and uncertainties over the figures that he uses as data. Some of these problems relate to the provenance of the data, to what they refer to: amongst the issues here, that of the gender division of knowledge is crucial, as regards both methods of data collection and the summarising of data into "the biological taxonomy." Moreover, the variety of elicitation techniques employed by the various sources that Brown uses strictly warrants ordinal manipulation of his data at best, certainly not the interval-level arithmetic employed here. The gross difference between types of society would remain unaffected by more appropriate data analysis techniques, but it would have helped to avoid the errors of mistaking arithmetical for empirical findings in the aggregation and disaggregation of averages and might have prevented the reporting of spurious levels of significance in statistical tests which are inappropriate because pertaining to no random or other sample with known selectional characteristics.

In thinking about classifications of the natural world, we are forced to recognise two principles, which may be paraphrased thus: it is not we who classify but the animals and plants that classify themselves; and men make classifications, but not in conditions of their own choosing. It is both the greatest benefit and the greatest disadvantage of studies of biological classification that we have available a conceptual scheme and a series of observational practices derived from the central knowledge-producing institution of our society against which we may compare, and in terms of which we may conceive of, the cognitive responses of other peoples to their natural environments. This conduces to a situation prone to "epistemological chauvinism" (Bousfield 1979) on two related counts. First, it is the nature of concepts of the world in general that they may be used to produce classifications in appropriate situations. Secondly, as a discipline such as ethnobiology crystallises its interests, techniques, and concepts into a paradigm of comparative research, it reduces its view of consciousness to one consisting of classifications and taxonomies (I note that the majority of Brown's sources arise from studies published after 1970). I contend that other cultures' views of the world may be less chauvinistically grasped if we attend more carefully to the social structuring of knowledge and see classification as the outcome of an interactive situation, rather than reify classification as the icon of knowledge.

In Brown's paper, the tendency to emphasise an a priori universalistic and reified conception of cognition in the form of a taxonomy results in the appearance of a concept of "salience" which attempts to condense within itself all the terms which are seen as distinct but in need of being related elsewhere in the paper. "Salience" here covers a semantic domain that ranges from objective conspicuousness to subjective interest (much the same range as is covered in the Weberian concept of "value") and thus incorporates both the *explanans* and *explanandum* of Brown's case, whilst being presented as an independent explanatory term. Often "degree of salience" is used to refer to the formal and abstract information content of an item. The working assumption "that vocabulary is to a large extent reflective of the long-term interests and endeavors of the people who use it" is always either vacuous, tautologous, or false. The data that Brown adduces do not test that hypothesis but rather are constituted as data because it is presupposed.

In effect, I suggest that for an alternative explanation of these data, we should look both to an account of the conditions of their production and to an examination of other discourses in which people relate to their natural world and which form a foundation upon which classifications may be erected. I am grateful to Brown for so clearly formulating the problem to be addressed.

Reply

by CECIL H. BROWN
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Clearly a difficulty with the present investigation is that the data utilized are not uniform with respect to several variables mentioned by commentators. Ideally, cross-cultural studies such as this will encourage development of a standardized methodology in ethnobiological research, leading to more uniform results. Despite present deficiencies, I am not convinced that the data are so divergent that they do not inform us of a "patterned diversity" across cultures, to borrow Hays's phrase. Several critics focus on ways in which these data lack comparability. A few words are in order with regard to how they are comparable.

A repeated point is that the hunting-gathering way of life has been severely altered through acculturation, resulting in erosion of traditional knowledge of plants and animals that are collected and/or hunted. While I know of no studies documenting foragers' loss of ethnobiological knowledge, I do not doubt that such attrition occurs. What my critics fail to mention is that small-scale agricultural groups have also undergone acculturation. Is there any reason to assume that this has not involved attrition of their traditional knowledge of wild plants and animals? Should the effect be any less extensive for farmers than for hunters and gatherers? If we assume for the moment that ethnobiological attrition is proportionally the same for both types of people, then we might expect relative size of taxonomies after attrition to reflect differences in size existing before acculturation. Thus, while there may be an acculturation effect, it is not necessarily the case that this would be a principal factor accounting for the profound difference in magnitude of taxonomies of the two groups.

A similar argument can be made concerning other ways mentioned in which the data may lack comparability. For example, commentators note that some reported taxonomies are aggregated lists while others represent only shared ethnobiological knowledge, that some taxonomies are exhaustive and others are not, and so on. It is probably the case that this lack of uniformity is not biased towards either foragers or small-scale agriculturalists. In other words, there is no reason to assume that reported taxonomies of the latter tend to be aggregated lists while those of foragers tend to be shared systems, or that taxonomies of farmers are exhaustive while those of foragers are not, and so forth. Given this, it is difficult to understand how these factors could influence the situation in such a way that taxonomies of agriculturalists would regularly be found to be considerably larger than those of hunters and gatherers. Since these factors affect data from both groups, it seems implausible that they play any significant role in producing the profound differences in taxonomy size apparent in tables 1–3.

Logically, these factors might be expected to increase rather than decrease the range of variation with respect to size of taxonomies. As Howe notes, the range of variation within each of the two categories is impressive. While variation for both categories may plausibly be traced in part to lack of uniformity in the data, what is especially interesting about these two ranges is that they overlap very little (see tables 1–3). This is precisely the kind of distribution that would be expected if biological taxonomies of small-scale farmers were typically much larger than those of hunter-gatherers.

Anderson raises the question of "appropriate sample," especially with regard to the inclusion of Mandarin. Since my project focuses on *folk* biological taxonomy, I attempted to include only those reported data that constitute knowledge shared by most mature speakers of languages rather than just by a few specialists. Application of this standard is particularly difficult when dealing with nation-state languages such as Mandarin. As Anderson notes, we could almost certainly find Chinese peasants who know hundreds of traditional plant names. On the other hand, it is also almost certainly the case that folk biological taxonomies of urban Mandarin-speakers are significantly smaller than those of peasants. Thus for Mandarin there appear to be at least two appropriate samples, peasants and city-dwellers, despite the fact that only one language is involved. Clearly, the Mandarin sample I used derives from urbanite rather than peasant knowledge.

Some criticisms focusing on lack of data comparability are in fact explanations of findings. For example, Bulmer argues that foragers have smaller biological taxonomies than small-scale farmers in part because the former typically live in very small groups while the latter live in very large ones. While he promotes the idea that population density directly and positively affects size of vocabulary, he offers no explanation of why this should be the case. I also argue that population density positively influences size of folk biological lexicon but only indirectly. However, in contrast I present a plausible and principled explanation of this influence.

I am especially interested in the comments of Ellen and Morris, who provide additional but noncontradictory explanations of taxonomy size differences. Ellen correctly points out that a sedentary and cultivating mode of living elevates the salience of certain creatures (vermin and pests) and plants (weeds) which otherwise are of no particular importance for hunters and gatherers. Morris suggests that the blandness of cultivated staples such as rice, millet, etc., may motivate the gathering of more palatable supplementary foods such as edible insects, small rodents, and wild vegetable relishes. I am also persuaded by Ellen's argument that the famine-food explanation for large plant taxonomies of small-scale cultivators applies only where cultivation is "chronically unreliable."

Bulmer, Ellen, Hays, and Headland focus discussion on binomialization. Ellen's comments indicate some misunderstanding of my position, possibly because of insufficient clarity in my presentation. For example, he writes that I accept "that binomialisation may also occur among closely related high-salience plants once domesticated." In n. 18 I do argue that domesticated biological classes at the "generic" rank (cf. Berlin et al. 1973) are typically high-salience categories and that closely related "specific" classes partitioning the latter are usually of low salience (and, hence, often binomially labeled). I did not intend the interpretation arrived at by Ellen, since a major corollary of my argument is that high-salience classes, encompassing either wild or cultivated organisms, are rarely if ever denominated by binomial terms.

Headland suggests that a possible reason for the dearth of binomials in taxonomies of hunter-gatherers may be a tendency of investigators to fail to elicit classes at the "specific" ethnobiological rank (following Berlin et al.'s [1973] usage). As he points out, Berlin's research has shown that specific categories are typically labeled binomially. Headland implies that specific taxa are somehow more elusive than classes of other ranks. To my knowledge, no one has argued in print before now that specific taxa are harder to elicit than other biological categories. Indeed, my own attempts at eliciting folk biological taxa in several different languages have involved no such difficulty. In addition, it is very doubtful that competent and thorough fieldworkers such as Eugene Hunn, Dulcie Levitt, Gary Palmer, Nancy Turner, and others from whom hunter-gatherer data have been obtained have failed to elicit most binomial labels actually occurring in languages. Furthermore, to revive a line of argumentation used above, if specific classes are "elusive," there is no reason to believe that they would be any more so in taxonomies of hunters and gatherers than in those of small-scale farmers.

I find Headland's argument concerning binomial percentages somewhat difficult to follow. He argues that my position can be substantiated only if it can be shown that specific classes of hunter-gatherers consistently are not labeled by binomials. I think it is now fairly clear that biological taxonomies of foragers typically have very few specific taxa. A dearth of specific taxa could contribute to the low binomial percentages found for those taxonomies. An important question, then, is why specific classses are rare in taxonomies of hunters and gatherers. I suspect that the answer to this question relates in part to taxonomy size and constraints on memory: plausibly, smaller taxonomies are more easily remembered than larger ones and thus do not need to utilize to any great extent memory-enhancing strategies such as recoding (cf. Miller 1967; Brown 1974:61–71), for example, by subgrouping specific classes under generic categories. In any case, I see no way in which my arguments hinge logically on the percentage of specific classes in languages that are labeled binomially. Nonetheless, my guess is that when specific classes occur in taxonomies of foragers, they will almost always have nonbinomial lexemes as labels. Indeed, Headland's (1981) own data for Casiguran Dumagat appear to bear this out.

In view of the fact that I have decided to deal only with obligatory binomials, critics raise the question whether or not most sources provide sufficient information to distinguish the latter from optional binomials. My impression is that the vast majority of sources consulted have attempted to make this distinction. More importantly, it is apparent in the literature on folk taxonomies of hunters and gatherers that both obligatory and optional binomials are extremely rare. Thus, even if I had included optional binomials in my survey, findings pertaining to binomialization would have not changed in any significant way. In addition, despite Hays's opinion to the contrary, the hunter-gatherer literature for the most part deals with nomenclature in sufficient detail to allow unambiguous distinction between binomial labels and descriptive phrases.

I am grateful to Thomas and Janet Headland for undertaking additional field research bearing on the question of the magnitude of Casiguran Dumagat folk botanical taxonomy. Their findings, of course, render parts of my n. 8 unnecessary. More importantly, they have presented us with a language case which deviates significantly from expectations. What are the implications of this case for my study? Critics might use it to dismiss the investigation altogether. On the other hand, cross-cultural research very rarely involves perfect or even near-perfect correlations. Even with new figures for Casiguran Dumagat, the present correlation involving botanical taxa is exceptionally strong, and, consequently, dismissal of the study at present would be premature and unwarranted.

So-called miss cases in cross-cultural research, especially when correlations remain strong as in the present example, can point to ways in which investigations can be fine-tuned to make apparent discrepancies, such as the Casiguran Dumagat case, understandable. For example, Casiguran Dumagat is spoken in Northern Luzon in the Philippines. The only other language of this area included in my sample (table 1) is Ifugao, a language of small-scale agriculturalists. As it happens, Ifugao is the largest reported botanical taxonomy, having an astonishing 2,131 labeled taxa. Obviously, compared with the plant taxonomy of Ifugao, that of Casiguran Dumagat (with 595 labeled plant classes) is relatively small. In addition, the second and third largest botanical taxonomies, Hanunóo (with 1,879 taxa) and Eastern Subanun (with 1,400+ classes), also pertain to Philippine languages spoken by cultivators. This distribution suggests that the association of small plant taxonomies with foragers and large taxonomies with small-scale farmers is perhaps best interpreted as a relative phenomenon; that is, it should be understood in terms of world regions rather than from a global perspective. Thus, for example, Casiguran Dumagat plant taxonomy is small compared with those typically found in Philippine languages of farmers (or, perhaps, more specifically, in agricultural languages of Northern Luzon), and this may turn out to be the meaningful comparison for my investigation.

Maintaining a global perspective, however, I suspect that the unexpectedly large size of Casiguran Dumagat plant taxonomy relates to the fact that these people are considerably more involved in agriculture than other hunting and gathering groups included in my study. From Headland's description (1981:5–7), cultivation appears to be well entrenched, if only on a small scale, and among the economic pursuits of Casiguran Dumagat–speakers possibly has been so for several hundred years. This is clearly reflected by the rather large number of labeled cultivated plant classes (179) that Headland mentions. Thus roughly 30% of labeled botanical taxa in Casiguran Dumagat entail domesticated plants. While I have not undertaken a systematic survey regarding the number of cultivated plants labeled in languages of other foragers considered, a conservative estimation is that on the average fewer than 10% of their labeled plant taxa are domesticated. Consequently, it would appear that Casiguran Dumagat–speakers have a relatively large folk botanical taxonomy compared with those of other foragers because they are more involved with farming than other hunting and gathering groups.

The figures for Casiguran Dumagat supplied by Headland have important implications for my explanation of binomialization. Given the size of Casiguran Dumagat plant taxonomy (595 classes), the percentage of taxa labeled binomially should be in the range of binomial percentages found for botanical vocabularies of small-scale farmers. It now appears that considerably less than 2.4% of labeled plant classes in Casiguran Dumagat are designated binomially. This figure contrasts sharply with, for example, 23.3% of 382 labeled plant categories in Chuj or 35.2% of 341 labeled plant classes in Amuzgo (cf. table 1). The Casiguran Dumagat case, then, contradicts my suggestion that extensive use of binomial labels is required when taxonomies are large. Indeed, it suggests, given my explanatory framework, that even when taxonomies are quite large, most labeled taxa can be of relatively high salience for foragers, while this apparently is not so for small-scale agriculturalists. Put another way, hunter-gatherers may be able to sustain higher levels of interest in a larger number of biological classes than can farmers. Why this may be the case is far from clear at present. In any event, it now appears that taxonomy size does not necessarily influence class salience and, hence, degree of binomialization pertaining to a biological taxonomy.

Postscript. Roy Ellen (personal communication) has informed me that the original figures supplied by him for Nuaulu animal classes (table 2) are incorrect. The total number of Nuaulu zoological categories is 448 and the total number of binomials is 252, yielding a revised binomial percentage of 56.3.

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