

Life-Forms from the Perspective of "Language and Living Things": Some Doubts about the Doubts



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having withdrawn significantly from the market, does not have the political strength necessary to demand these changes.

notes

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life-forms from the perspective of *Language and Living Things*: some doubts about the doubts

Randall and Hunn (AE 11:329-349) call into question a number of hypotheses I have developed over the years concerning the growth and development of folk biological life-forms in languages. The broadest criticisms offered by them are that the data I have used are neither sufficiently reliable nor detailed to prove or disprove the hypotheses and that I have not provided definitions of life-form categories which are sufficiently clear to permit cross-language comparison of these classes.¹

As Randall and Hunn point out, I was the first to recognize problems inherent in using dictionary sources in my initial life-form studies (AA 79:317-342, 1977; AA 81:791-817, 1979). They also refer to recent work of mine in which proposed hypotheses are tested against more thorough and accurate nondictionary data. But they fail to mention that this later work recognizes problems of earlier treatments which they criticize, and that it amends definitions and hypotheses to resolve those problems. In short, I feel that Randall and Hunn have unwisely ignored the content of my later work, thereby rendering their published criticism untimely and inappropriate.

The later work in question is known to Randall and Hunn in the form of a book manuscript entitled "Folk Biological Life-Forms: A Cross-Language Study of Plant and Animal Classification," and cited as such by them in their paper. This manuscript was sent to Hunn by me in 1981, the date attributed to the work in their references cited. The work has been slightly revised and is now published under the title *Language and Living Things: Uniformities in Folk Classification and Naming* (Rutgers University Press, 1984), henceforth *LLT*.

In *LLT* I assemble data bearing on plant and animal life-form classification and nomenclature from 188 and 144 languages, respectively. These materials have been gathered from published and unpublished articles and monographs treating plant and animal categorization in various languages, and from personal communications with linguists and others who have specialized knowledge of the languages in question. In many cases persons in the field directly investigated folk biological classification for me by interviewing native (often monolingual) speakers. In addition, life-form data from several languages were collected by me personally, either in the field (Veracruz and Oaxaca, Mexico) or from native speakers of languages other than English residing in the United States.

Most of Randall and Hunn's paper is concerned with showing how my hypotheses do not accord with data gathered by them from three genetically unrelated languages—Sinama (The Philippines), Sahaptin (Oregon and Washington states), and Tzeltal (Chiapas state, Mexico). Specifically, they write that these languages have life-form concepts that "(1) fail to correspond to the developmental sequence favored by Brown; (2) are defined by form criteria quite unlike those cited as criterial by Brown; and (3) are not included by Brown in his set of universal life-forms" (p. 330).

The third criticism is the least serious of the three. Nowhere have I suggested that languages cannot or will not have life-form categories, either botanical or zoological, other than those dealt with in my hypotheses (i.e., "tree," "grerb," "grass," "vine," "bush," and "bird"; "fish," "snake," "wug," "mammal," and "combined wug-mammal"). Thus, Randall and Hunn's discovery that languages investigated by them have life-forms not included in the latter set in no way vitiates the hypotheses in question.

Their claim that life-form concepts of Sinama, Sahaptin, and Tzeltal do not accord with those predicted by my hypotheses is a more serious criticism. The following analysis, based on the framework provided in *LLT*, shows that there is little, if any, empirical basis for proposing that these language cases fail to conform with life-form encoding sequences.

Sinama (or Samal) constitutes case 89a in the section of *LLT* (p. 165) in which botanical life-form inventories of languages are described in detail. There I interpret the language as having three life-form categories: *kayu* ("tree"), *sagbot* ("grerb"), and *bahan* ("vine"). The sources for this case are Randall (AE 3:543-553, 1976; 1978: personal communication); the latter source provides lists of plants considered by Sinama speakers to belong to the three categories.

Randall and Hunn interpret only *bahan* as being an unequivocal life-form in terms of my framework. However, they admit that *kayu* may qualify as a "tree" life-form but that "both the range and focus of *kayu* differ from English *tree* because the criterial attributes are different" (p. 336). Thus, they conclude that Sinama may have

either one or two plant life-forms depending on one's definition, adding that "they are not the ones Brown predicts" (p. 336). By this they mean that the botanical life-form encoding sequence would not permit a singleton "vine" life-form in a language nor a combination of only two life-forms which are "tree" and "vine" (in *LLT* the pair "tree" and "gреб" and the pair "tree" and "grass" are the only twosomes that accord with the encoding sequence).

The problem with Randall and Hunn's analysis of *kayu* is that they are confused with respect to the criteria I attribute to the universal "tree" life-form. They are prepared to dismiss *kayu* as being "tree" because the apparent definitional features underlying the category are not the same as those underlying American English *tree*. For example, there are members of the category *kayu* such as the mangrove that are not equivalent to American English trees in "overall form" (the mangrove "has very crooked branches," and "is multi-stemmed"). In addition, Sinama informants rarely refer to large size in describing *kayu* while large size is apparently critical in the definition of *tree* for speakers of American English.

In no place have I suggested that the criteria underlying membership in botanical categories of languages other than American English must be equivalent to those underlying American English *tree* in order to qualify as being the universal "tree" category. Rather, in various publications I have provided an unambiguous definition of "tree": "large plant (relative to the plant inventory of a particular environment) whose parts are chiefly ligneous (woody)" (*LLT*, p. 13). I see nothing "vague" about this definition, as Randall and Hunn would have the reader believe. Furthermore, the membership of Sinama *kayu* as described by them clearly fits this definition.

Still, their discussion of *kayu* raises an interesting point: both Sinama *kayu* and American English *tree* fit my definition of "tree" despite an apparent difference in overtly stated definitional criteria. For Sinama informants "woodiness" is emphasized; and for speakers of American English "large size" is especially criterial (as suggested by the fact that large, nonwoody plants such as the banana plant are identified as trees by some speakers). In *LLT* (pp. 101-102) I note that languages tend to vary with respect to the definitional weight their speakers attribute to the two features underlying "tree" categories (i.e., "woodiness" and "large size"). Some tend to emphasize ligneousness, some large size, and some treat both features as equally weighted in describing "tree."

It may be that cultural practices influence the differential weighting of "tree" criteria by speakers of different languages. For example, from Randall's discussion it appears that most mature speakers of Sinama are involved on a day-to-day basis in collecting wood and manipulating it in various ways for various purposes, especially for fuel. As a consequence, as Randall and Hunn note, wood is especially salient for

them. By contrast, wood is not so salient for many speakers of American English whose lives rarely entail wood-getting and wood-manipulating activities on a daily basis. Given the high salience of wood for Sinama speakers, it is perhaps not surprising to find that they emphasize woodiness in discussions of *kayu*. Since speakers of American English typically lack the intimate relationship with wood characterizing most Sinama speakers, they focus instead on large size in defining *tree*.

Randall and Hunn also mention that *kayu* differs from American English *tree* since fruit trees are usually not regarded as belonging to the Sinama category. Among the 188 languages surveyed in *LLT* (p. 14) it is common to find that languages restrict "tree" (and also "gреб") to wild plants that are not cultivated or protected by humans.

Randall and Hunn apparently deny that Sinama *sagbot* is "gреб" on the grounds that "exemplary *sagbot* are useless and therefore not planted" (p. 336). Nevertheless the range of *sagbot*, as described by them, encompasses both useful and planted plants in addition to wild vegetation. The membership of *sagbot* unambiguously fits my definition of "gреб": "small plant (relative to the plant inventory of a particular environment) whose parts are chiefly herbaceous (green, leafy, non-woody)" (*LLT*, p. 13). The major point here is that *sagbot* encompasses most wild plants that are not *kayu*; that is, that are not large and woody. Thus, *kayu* and *sagbot* form a classic "tree"/"gреб" binary contrast along the dimensions "size" and "woodiness," as described in many of my publications on botanical life-form growth and development.

Sinama, then, unambiguously encodes a combination of three botanical life-forms, "tree," "gреб," and "vine," predicted by my hypotheses. In addition, the Sinama plant life-form system reflects an apparent Philippine language pattern. Of 15 Philippine languages surveyed in *LLT*, 13 have the combination "tree" + "gреб" + "vine."

A description of the folk zoological life-form inventory of Sinama is not found in *LLT*. My understanding of the latter is based solely on information supplied by Randall and Hunn in their paper. It would appear that the language encodes at least three animal life-forms dealt with in my hypotheses: "fish" (*daing*), "bird" (*manuk-manuk*), and "snake" (represented by both *soa* ["snake" or "large elongated creature"] and *olet* ["worm" or small elongated creature"]). This combination is in perfect accord with the folk zoological life-form encoding sequence as revised in *LLT* (p. 24).

Randall and Hunn write that "the Sinama category *daing* . . . matches Brown's 'fish' neither in focus nor in range" (p. 332). Given their description of creatures identified by the term, it is difficult to understand what motivates this conclusion. As formulated in *LLT* (p. 15) a "fish" is a creature "possessing a streamlined body and fins, usually having gills." This definition makes no

mention of category “focus.” Further information is provided to the effect that classes “encoding this discontinuity always include true fish. In their greatest extension they include true fish and fish-shaped mammals such as dolphins and whales” (*LLT*, p. 15). Randall and Hunn (p. 337) write that “most of the approximately 350 terminal taxa included in . . . [*daing*] are clearly what ichthyologists consider *fish*” (the category also extends to a few cetaceans, sharks, rays, and occasional squid). In addition, while most *daing* are apparently eaten, varieties that are not consumed are also included; thus, the category is not limited to “eaten fish.” Given this, I see no difficulty in identifying *daing* as a “fish” life-form.

Randall and Hunn also conclude that *manuk-manuk* “bird” is problematic. The definition of “bird” given in *LLT* (p. 15) is as follows: “large creature (relative to creatures such as bugs) possessing wings and usually having feathers and a bill or beak.” Further information is provided to the effect that classes “encoding this discontinuity always include birds. In their greatest extension they include birds and flying mammals such as bats” (*LLT*, p. 15). Randall and Hunn inform us that with three exceptions “all those animals zoologists would call ‘birds’ are unanimously classed by Samal as *manuk-manuk*” (p. 338). Also, some informants extend the class to bats. These authors find one further extension troublesome: the inclusion of moths and dragonflies by “a significant percentage of Samal” (p. 339). Consequently, they conclude that *manuk-manuk* does not label a “bird” life-form since the class “includes an adaptation to flying which is not correlated with overall form” (p. 339).

The above definition of “bird” from *LLT* identifies only two necessary criteria for membership in the category: large size (relative to creatures such as bugs) and wings. Birds and bats, of course, meet these criteria, but do moths and dragonflies? Randall and Hunn tell us that flies and mosquitoes are not considered to be *manuk-manuk* even though these insects fly. Of course, moths and dragonflies are typically larger than flies and mosquitoes and many other flying insects. Apparently, speakers of Sinama have extended their large winged creature category to the largest winged insects but not to all winged insects. In short, their concept of what is a “large winged creature” is somewhat more expanded than similar concepts in most other languages. However, such an expanded “bird” life-form (which includes the larger flying insects) is found in several languages surveyed in *LLT*, so that the Sinama situation is hardly unique.

Randall and Hunn’s denial of a “snake” life-form in Sinama is perhaps traced to their lack of close attention to the original animal life-form paper (Brown, *AA* 81:791–817, 1979) and to the contents of their manuscript copy of *LLT*. In the 1979 paper I note that the “snake” life-form is sometimes realized in languages by the binary contrast “large elongated creature” (snake)/“small elongated creature” (worm). In some cases languages encode only one-half of this

binary opposition, having a term for “snake” or for “worm” but not for both. In the 1979 treatment languages which encode either “snake” or “worm” are considered to have a “snake” life-form, an approach continued in *LLT*. Randall and Hunn have doubts about a Sinama “large elongated creature” or “true snake” life-form, yet they cite the term *olet*, to which they assign the gloss “worm,” and inform us that the category so labeled includes “numerous subtypes.” Assuming that this is a reasonably accurate gloss, it would appear that the “snake” life-form is at least represented in Sinama by a “small elongated creature” category.

Even if it were not, there are grounds for judging that a “large elongated creature” category is encoded by the language and that a “snake” life-form pertains to it. In *LLT* (p. 15) “snake” is defined as: “featherless, furless, elongated creature usually lacking appendages.” Further information is provided to the effect that classes “encoding this discontinuity always include snakes and/or worms” (*LLT*, p. 15). Randall and Hunn list four Sinama categories which encompass featherless, furless, elongated creatures—*baat* (“sea cucumber”), *olet* (“worm”), *soa* (“snake”), and *endong* (“eel”)—implying that my account is defective because any one of these four could be “snake.” This is nonsensical since the information provided in *LLT* insists that either true snakes or worms must be encompassed by a “snake” class, which, of course, rules out both *baat* and *endong* as candidates for “snake.”

Sinama *soa* (“snake”) is dismissed by Randall and Hunn primarily because it is a folk generic category rather than a life-form class by Berlin’s (*AE* 3:381–399, 1976; Berlin, Breedlove, and Raven, *AA* 75:214–242, 1973; *Principles of Tzeltal Plant Classification*, 1974) nomenclatural criteria. The category *soa* immediately includes only two labeled classes, *soa ma tana*, a rare land snake, and *soa ma tahik*, a very poisonous sea snake. Labels for these two classes are secondary lexemes; thus, they are identified as being folk specific categories. Since only generic classes immediately include specific classes, *soa* is necessarily a generic category rather than a life-form class. In *LLT* (pp. 19–20) nomenclatural criteria are no longer interpreted as being pertinent to the process of determining the occurrence of universal life-forms in languages. If the membership of a category meets the given definitions of a universal life-form’s constituency, then that class is judged to be that life-form. Under this convention *soa* is a “snake” life-form.

Since the initial triad of the encoding sequence “fish,” “bird,” and “snake” in fact pertains to Sinama, the language cannot constitute an exception to animal life-form encoding regularities even if the encoding of “mammal” and/or “wug” were in some way problematic. From information provided by Randall and Hunn it appears that both “mammal” and “wug” life-forms may be in the process of consolidating in the language. In both cases expansion of reference seems to be involved. The term *hinayop*, which apparently

originally meant “domestic animal,” seems to be expanding to “large residual animal” (or “mammal”), and an “ant” term, *sanam*, seems to be expanding to “small residual animal” (or “wug”).

In summary, the animal life-form categories found in Sinama are precisely the ones predicted by my hypotheses, despite Randall and Hunn’s disclaimers. In fact, I am happy to add this confirming case to the 144 cases presented in *LLT*.

Sahaptin constitutes case 7a in the botanical life-form data section of *LLT*. The source for this case is Hunn’s final report to the National Science Foundation describing ethnobiological findings for Sahaptin. This information and that presented in Randall and Hunn’s paper are essentially the same.

From Hunn’s report I have determined that only one botanical life-form, “tree,” pertains to Sahaptin (*LLT*, p. 135). This is the only singleton botanical life-form predicted by my hypotheses. In fact, Randall and Hunn do not claim that Sahaptin has a combination of plant life-forms which fails to accord with the encoding sequence. Rather, they use the Sahaptin case to show the functional nature of Sahaptin general plant categories, implying that I would have incorrectly attributed universal life-form status to them. This is a line of argument that also prevails in Hunn’s (AA 84:830–847, 1982) earlier criticism of my life-form investigations. It is, in fact, unwarranted; in *LLT* I have interpreted them in just the manner suggested by Hunn himself.

I judge Sahaptin to possess two zoological life-forms, “bird” and “fish” (*LLT*, p. 215), based on data from three sources (Hunn’s NSF report; *NARN* 14(1):1–19, 1980; AA 84:830–847, 1982). Randall and Hunn agree with me that the term *kakya* labels a perfectly good “bird” life-form class. Yet they deny that the language has a “fish” category. However, under either interpretation the combinations of animal life-forms found in Sahaptin are those predicted by my hypotheses.

Randall and Hunn conclude their discussion of Sahaptin animal categories by describing several classes that are special purpose and descriptively labeled (e.g., “all the milk makers” and “flesh eaters”), stressing that these are of considerably more significance to Sahaptin speakers than the general-purpose life-form classes treated in my work. While this is undoubtedly true, the tone of their discussion is unfortunate. For example, they write that “the limited inventory of standard Brownian life-forms in Sahaptin obscures the wealth of abstract concepts Sahaptin speakers use to organize their knowledge of natural history” (p. 343). I most definitely do not hold the view, as this implies, that special-purpose categories are of no importance in Sahaptin or in any other language. Furthermore, there is the unfortunate implication that my work somehow denigrates Sahaptin speakers. They also write that “the insistence on monolexemically labeled taxa diverts our attention from a Sahaptin folk zoological classification scheme of impressive perspicacity and high abstraction” (p. 343). The impres-

sion left is that I urge scholars to investigate general-purpose categories only and, for that matter, only monolexemically labeled ones. This is simply not the case. Randall and Hunn, of course, have every right to study special-purpose biological categories, and such studies may prove very significant. There is nothing in my work that suggests otherwise.

The botanical life-form inventory of Tzeltal is presented as case 154a in *LLT* (p. 195). These data are extracted from Berlin, Breedlove, and Raven’s (1974) pioneering study of Tzeltal plant classification and nomenclature. In terms of criteria outlined in *LLT*, Tzeltal has four plant life-form classes: “tree,” “grerb,” “grass,” and “vine.” This combination of four life-forms is predicted by my hypotheses.

Randall and Hunn argue that “if terms that incorporate notions of utility in their definitions are excluded from the life-form inventory, Tenejapa Tzeltal would have but a single botanical life-form” (p. 344). As it happens, the single life-form that would be left after such an exclusion would be “tree.” This is the only singleton plant life-form predicted by the botanical life-form encoding sequence. Consequently, the utilitarian interpretation urged by Randall and Hunn would not produce a language case that does not accord with my predictions.

Their utilitarian interpretation is seriously flawed, however. They write that plants not affiliated with botanical life-forms are excluded from these “on the basis of morphological aberrance and/or cultural significance (Berlin, Breedlove, and Raven 1974:415; emphasis added)” (p. 344). They go on to argue that “cultural utility is thus an important Tzeltal criterion for excluding a plant from life-form membership” (p. 344). Therefore, it is “clear” to them “that at least three of the four Tenejapa Tzeltal botanical life-forms . . . are defined by uselessness” (p. 344)—these being “grerb,” “grass,” and “vine.” The problem here is that Randall and Hunn misinterpret Berlin et al. (1974:415) and then make a logically unwarranted inference.

Berlin et al. actually write that exclusion of a plant from major life-form classes “relates to the cultural significance of the plant.” They are not claiming that plants included in Tzeltal life-form categories are culturally insignificant, only that those excluded are typically either morphologically aberrant or of especially high cultural significance, such as cultivated ones. Randall and Hunn would apparently have us believe that since some culturally significant plants are excluded from Tzeltal life-forms, then all those that are included are of no cultural importance whatsoever, an argument that clearly does not follow from Berlin et al.’s discussion. Astonishingly, they would also have us believe that all plants included in at least three Tzeltal life-forms are totally useless. The unwarranted inference here is that something that is not of high cultural importance is necessarily lacking in use.

The facts of the matter speak clearly on this point. A survey of plants described by Berlin et al.

which are included in Tzeltal “grerb,” “grass,” and “vine” reveals that most of these have some kind of use (e.g., as medicine, as a condiment, for decorative purposes). Empirically, then, plants included in Tzeltal life-form classes do have uses; but compared to plants excluded from life-forms, such as cultivated ones, they are not especially important.

Tzeltal animal life-forms are presented as case 110b in *LLT* (p. 248). The source for this information is Hunn (*Tzeltal Folk Zoology*, 1977). I judge Tzeltal as having four zoological life-forms—“bird,” “fish,” “snake,” and “mammal”—precisely those predicted by the animal life-form encoding sequence.

Randall and Hunn mention several problems concerning interpreting *čanbalam* as “mammal,” as I have done in *LLT*. It seems to me that further research may show that the term is polysemous, referring broadly to creatures in general and more specifically to large residual creatures (i.e., “mammal”), although I am prepared to concede that this may not turn out to be the case and that the language actually may not encode the life-form. Nevertheless, if it does not, Tzeltal’s inventory of animal life-forms still would not deviate from that predicted by my hypotheses.

Randall and Hunn note that many members of Tzeltal “snake” and “fish” categories are labeled by composite terms that are identical in nomenclatural structure to secondary lexemes normally labeling specific classes. Since generic classes typically include specific taxa, Tzeltal “snake” and “fish” may prove to have developed through inductive processes like generic classes rather than through deductive processes like life-form categories. As mentioned above, nomenclatural criteria are no longer used in *LLT* in considering the presence of universal life-forms in languages. Consequently, from the perspective of *LLT*, Tzeltal “snake” and “fish” are perfectly good life-forms despite the nature of psychological processes that may have been involved in their development. Randall and Hunn—and earlier Hunn (*AE* 3:508–524, 1976; *Tzeltal Folk Zoology*,

1977; *AA* 84:830–847, 1982)—have assembled no convincing evidence supporting their hypothesis of a deductive/inductive distinction between life-form and generic categories, respectively. Indeed, Atran (*AA*, 1985) argues persuasively that no such distinction is pertinent.

Finally, a most serious criticism in Randall and Hunn’s paper is that the plant and animal life-form inventories of languages they have studied in detail fail to correspond with those predicted by my hypotheses. However, when one looks at their extended discussion closely, they in fact make such an argument for only one language, Sinama, claiming that the plant life-form set in the language does not accord with the botanical life-form encoding sequence. In other words, they only claim that one of six sets of plant and animal life-forms in three languages are really exceptional. Evidence for the major criticism made at the beginning of their article is really not as abundant as these authors would have readers believe. Indeed, I present reasonable arguments to the effect that even the Sinama plant life-form data conform with my hypotheses and, consequently, that no exceptions whatsoever are apparent in the evidence assembled by Randall and Hunn.

notes

¹ This discussion is a much truncated version of a longer reply to Randall and Hunn’s critical piece. Readers who wish to see the original manuscript can do so by writing to me at the Department of Anthropology, Northern Illinois University, DeKalb, IL 60115.

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