REVIEW ARTICLE

Morphology: A study of the relation between meaning and form. By JOAN L. BYBEE. (Typological studies in language, 9.) Amsterdam & Philadelphia: Benjamins, 1985. Pp. xii, 234. Cloth $33.00, paper $20.00.

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Since the rest of this review will be concerned mostly with Explanation and Description, I will dispense first with that most trying of the linguist's muses, Evaluation, by declaring with confidence that this is one of the most thought-provoking books about morphology to have appeared in the decade since morphology experienced its latest renaissance. Not all the ideas here are new: some come from articles on morphology which Bybee has published since her earlier book (Hooper 1976), which is in many ways a phonological counterpart to this one, and some come from other people—notably Henning Andersen, Joseph Greenberg, and David Zager. There is also a large unacknowledged debt to Zipf 1949. But novelty is not necessary to success. B has here tied together all her sources, with some new ideas of her own, to come up with an extremely provocative work.

The book appears in a series, 'Typological studies in language', whose volumes—according to a statement facing the title page—are 'Functionally and typologically oriented, covering specific topics in language by collecting together data from a wide variety of languages and language typologies. The orientation of the volumes will be substantive rather than formal, with the aim of investigating universals of human language via as broadly defined a data base as possible, leaning toward cross-linguistic, diachronic, developmental and live-discourse data.' B adheres so closely to this program that we could simply substitute 'verbal inflection' for 'language', delete 'and live-discourse data', and have a reasonable description of her book. This brings me to my first quibble: the title is too general, since this book is almost exclusively about verb inflection. But the subtitle, 'A study of the relation between meaning and form', is good: in reversing the normal order, it subtly presages the main point of the work, which is that meaning determines (at least certain aspects of) form.

There are ten chapters; two form an introduction and a conclusion, and the rest are divided into two distinct parts. Part I comprises four essays on the form of morphemes: Chap. 2, 'Semantic determinants of inflectional expression'; 3, 'The organization of paradigms'; 4, 'The lexical/derivational/inflectional continuum'; and 5, 'Two principles in a dynamic model of lexical representation'. Part II is essentially semantic in orientation, and consists of a presentation of the results of a survey of the meanings of the categories of verbal morphology in a carefully constructed sample of 50 languages: Chap. 6, 'Aspect'; 7, 'Tense'; 8, 'Mood'; and 9, 'Aspect, tense and mood as grammatical categories'. Part II is much more tentative, and B is currently working

* Frank Anshen and S. N. Sridhar read an earlier draft of this review. My revisionist view of Zipf 1949 comes from reading Horn 1985.

on a more comprehensive treatment; so I will devote less attention to it. Part I, though, contains enough interesting observations, ideas, and claims to satisfy any number of reviewers.

I often begin a book by running through the bibliography, in part out of vanity, but also to place the author, to know what he or she considers worth reading. In this case, I was surprised at first by the almost complete absence of reference to the recent generative literature on morphology that I know best. Thus, if I compare B's bibliography to that of Scalise 1984, the total number of common items is eight, of which only one was published after 1980 (Anderson's 'Where's morphology?', 1982); two are standard works of the 1970's, which receive a passing nod on the first page of the book; and the rest are classics that would be cited in almost any linguistics book. Nor is there much in the way of reference to the classic American structuralist literature on morphology (no Harris, no Bloch, no Pike). But after a little reading, the bibliography began to make sense, since this book lies outside the American mainstream. In fact, it presents a direct challenge to it.

The mainstream is the descriptive tradition, rooted in Bloomfield, whose central analytical concern is the writing of grammars and whose central theoretical concern is the justification of particular kinds of grammatical descriptions. For Chomsky, just as for Bloomfield, these are the main questions of linguistics. They are also the questions that have attracted the attention of most morphologists in the last ten years. B, by contrast, is quite simply not interested in that tradition. To quote her first page: 'The approach offered in this work is quite different. The goal is not to propose a descriptive model of morphology, but rather ... to explain the recurrent properties of morphological systems ... in terms of the general cognitive and psychological characteristics of human language users.'

The title of Chap. 1 is 'Toward explanation in morphology', and B here contrasts description with explanation. Descriptions, for her, cannot constitute explanations, and she is not interested in them. This point runs through the book, and is important enough to be repeated in the closing chapter as one of the two main theoretical points implicit throughout: 'No explanation for linguistic phenomena is complete until a causal relation can be shown to exist between the principle proposed as explanation and the linguistic phenomena to be explained'. Thus the kind of theoretical issues with which Scalise is concerned, most of which deal with what sorts of descriptions are appropriate, are simply beside the point for B. Hence the lack of overlap.

Here one could embark on an excursus into philosophy of science. Are descriptive theories in fact explanatory? When Watson and Crick proposed a structure for DNA, were they providing a description or an explanation? Instead, I would like to call a truce. I believe that certain descriptions are explanatory, but I will accept that some explanations are not descriptive. A number of questions now naturally arise. What sorts of things count as explanatory for B? Are her explanations valid? And, at least for those of us who remain tied to description, are these explanations useful to the descriptive enterprise?

For the most part, B's explanations are pragmatic, diachronic (or at least dynamic), and not language-particular. The two explanatory principles to which she devotes most attention are Gricean in both scope and simplicity (and, confusingly, in one case at least, in name): they are RELEVANCE and GENERALITY. Relevance is not Grice's (1975) 'Be relevant' maxim, but is rather defined as follows: 'A meaning element is RELEVANT to another meaning element IF THE SEMANTIC CONTENT OF THE FIRST DIRECTLY AFFECTS OR MODIFIES THE SEMANTIC CONTENT OF THE SECOND' (13). B goes on to explain that two expressions are said to be relevant to one another to the extent that their combined meanings are likely to be expressed by a single expression.

There are a few unclarities in B's discussion of the principle. Most obviously, how do we distinguish a 'meaning element' from its 'semantic content'? Does B mean 'meaningful element'? Probably not, since her principle extends to entire categories of elements. Relevance is also apparently dependent on culture as well as on cognition: 'Two semantic elements are highly relevant to one another if the result of their combination names something that has high cultural or cognitive salience' (14). Both these notions are problematic, since the cognitive salience of something varies with attention, to at least some degree; and cultural salience is a black hole. Luckily, the examples which B provides for cultural variation do not support her point. Thus she notes that Talmy 1985 has identified three major lexicalization patterns for motion verbs in the world's languages: Romance, Semitic, Polynesian, and Nez Perce follow one; Chinese, English, and Caddo follow another; and Navajo and American Sign Language follow a third. It is hard to see any cultural unity in these categories. Far more plausible is the claim that three types exist and that different languages follow different types, regardless of culture.

There is also the vexing problem of circularity. How do we know what is relevant? B provides justification for relative ranking only in individual cases, and usually by comparing individual examples. Thus she argues that 'walk through water' is lexicalized as *wade* because 'Whether one has one's feet on dry land or in water is quite relevant to the act of walking' (13). She argues that aspect is more relevant for verbs than is person, because 'aspect represents different ways of viewing the internal temporal constituency of an action or state' (15), while subject agreement 'refers to an argument and not to the action or state described by the verb itself'. However, she does not attempt to develop any specific or generally applicable criteria for relevance, let alone an explicit general theory. In the absence of these, having to make individual cases every time weakens B's whole argument; nor are all her individual arguments equally plausible. Nonetheless, B's principle of relevance, which I think should be named FUSION, is useful, as we shall see. But her circularity and lack of explicitness will disturb many readers; and her basic claim, that structural regularities result from pragmatic principles, remains essentially unproved.

B's other major pragmatic principle is LEXICAL GENERALITY; she does not define this explicitly, except to say that all inflectional categories, which she says are general, must have 'only minimal semantic content', and that the semantic content of an inflectional morpheme must be 'general enough to be appropriately combinable with any stem of the syntactic category' (17). Again, B does not tell us how to identify 'a meaning that is widely applicable', except that inflections have such meanings. She also does not tell us whether generality is gradient, or how to tell what is more or less general. In any case, she doesn't use the notion much.

What, then, do B's principles buy us? Her major claim is that there is a hierarchy of inflectional categories—valence, voice, aspect, tense, mood, and agreement—and that the properties of these categories in various dimensions are predictable in large part from relevance. First, she states that the order of

this hierarchy is determined by relative relevance, with valence the most relevant to the verb stem, and agreement the least. Whether B's individual arguments for relative relevance are successful I will leave up to the reader, though I find some a little shaky. For example, why should voice, which relates argument structure to syntactic subject, be more relevant than aspect, which is confined semantically to the verb itself?

Second, according to B, relevance predicts that the categories in the middle of the hierarchy are more likely to be inflectional. Those at one end are more likely to be too relevant, and hence to result in lexicalized combinations; those on the other are less relevant to the verb, and hence less likely to attach to it.

Third, B claims that the hierarchy is directly correlated with linear order: more relevant categories will be closer to the stem.

Fourth, 'The categories that are more relevant will have a greater morphophonemic effect on the stem than the less relevant categories' (24).

All these predictions are tested in an interesting way. B has taken a sample, consisting of 50 languages chosen by Perkins 1980, such that no two are from the same phylum or from the same cultural/geographic area, as defined by Kenny 1974. Because the original sample did not take into account the availability of reliable descriptions, B has made a few substitutions. Nevertheless, her basis for selection means that this sample is better than others used in previous surveys—although, as B notes, it includes too many isolates and excludes some phyla. She tested her four hypotheses by collecting information from grammatical descriptions about the inflectional categories marked on verbs in each language in the sample, and then tabulating all the information.

B briefly describes the criteria used in categorizing the information, and then defines each morphological category. Her criteria and definitions seem reasonable; but are any such criteria foolproof? Consider tense and aspect in well-analysed languages. A colleague of mine, as an assignment in a course on the structure of English, routinely asks his student to find two handbooks that agree on the number of tenses in English. In the standard works, this number ranges from two to around forty. Does anyone know a sure-fire argument for classifying the Eng. perfective as tense or aspect? And if it is an aspect, is aspect inflectional in English? (It is not obligatory.) Maybe it is both, depending on syntactic and pragmatic context. But what about *will*? It is sometimes a tense marker, sometimes a modal, and on occasion may be somewhat aspectual (compare *He won't leave* with *He won't be leaving*, where the modal reading is apparently tied to aspect). In French, is the imperfect a tense or an aspect? Or is the aspect distinction context-sensitive? Is the Fr. perfect an aspect, since it co-occurs with tenses, e.g. in the future perfect (*j'aurai fait*)? Or is it a tense ('passé composé'), which it acts like when no other element exists in the auxiliary? Or do we have here a case of the well-known connection between perfect aspect and past tense?

I don't pretend to know the answers to these questions. Someone, someday, may be able to categorize these things properly; but if we don't have firm answers for well-known languages like English and French, then how much can we trust data drawn from sometimes sketchy descriptions of little-known languages? There is some safety in numbers, of course; but even very large numbers cannot solve the old in/out problem. Conclusions can never be firmer than data, no matter how good one's methods. I am, of course, using the same stick with which typologists are always beaten; but they shouldn't think that they can escape beating by ignoring the stick. All this criticism does not mean that I believe B's data or conclusions are without merit, but merely that they are not so firm as she would have us think. She might have gained a more sympathetic reader by adding a few hedges, but she does not. Thus she says that the results of her survey are 'extendable to the universe from which the sample was drawn. That is, if 52 percent of the languages in the sample have inflectional aspect, then we can predict that inflectional aspect occurs in approximately 52

percent of the languages of the world' (26). It would take a chapter of statistics to clarify B's 'approximately'; but the point is that things aren't as direct as B seems to be saying.

B's conclusions are based on a sample. The goal of any sampling is to make it representative. The difficulty is that we do not know in advance what the correct dimensions are. What we do is make more or less well-informed guesses, but we can never know that our sample is truly representative. In the present case, B has chosen two dimensions, language family and area—obviously good candidates, but they do have problems. Families are not morphologically homogeneous, especially with respect to the categories of verbal morphology; cf. English, Russian, and Hindi within IE, or Classical and Modern Semitic languages, or Hawaiian and Tagalog within Austronesian. Nor is genetic classification so well grounded that we can always take it as given. The experts are still fighting over Japanese, and families like Central New Guinea are not exactly established. The notion of cultural area is even less firmly grounded, as any anthropologist will tell you.

My point is not that B's dimensions are wrong, but that we do not know if they are right; and they are not as firm as one might like them to be. Even if they were firmer, we would still not be permitted to claim, as B does, that the sampling is so good that the results can be extended directly to the universe from which the sample was drawn. There is also the larger question, not raised by B, of what this universe is. Is it the languages of the world at the moment, or is it the open set of all possible languages? Is there reason to believe that the former is representative of the latter?

With all these caveats about the data, let's look at the results. B displays these in Figures 1–2 (pp. 30–31), which I have reproduced here. Before discussing the numbers in these figures, let me clarify two points of presentation. First, in the line that is labeled 'person', it is clear that B means 'subject'.

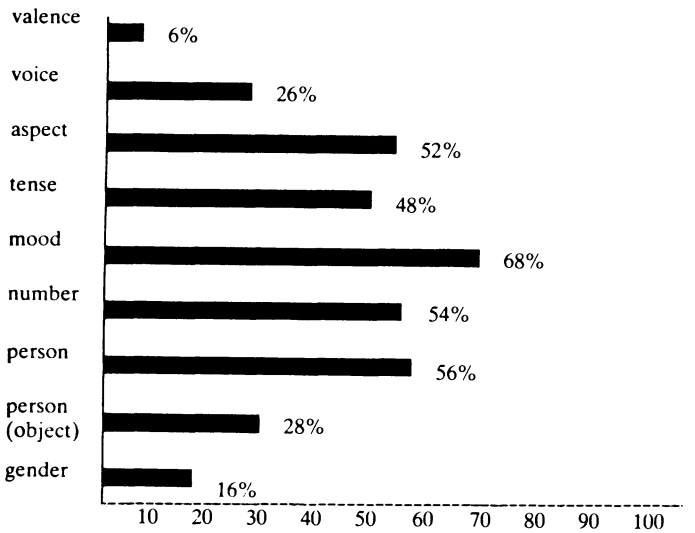

FIGURE 1. Inflectional categories for verbs.

Second, in discussing the statistics, B uses the term 'frequency' more or less interchangeably with 'percentage'. The careful reader should change all occurrences to 'percentage'; but the usage raises another problem—there is no way to tell how robust a phenomenon must be in order to be counted, especially when it is derivational. Thus B mentions that one language, Kwakiutl, has tense as a derivational category. On p. 161, we discover that the language has a few temporal suffixes used with both nouns and verbs. Since they occur with nouns,

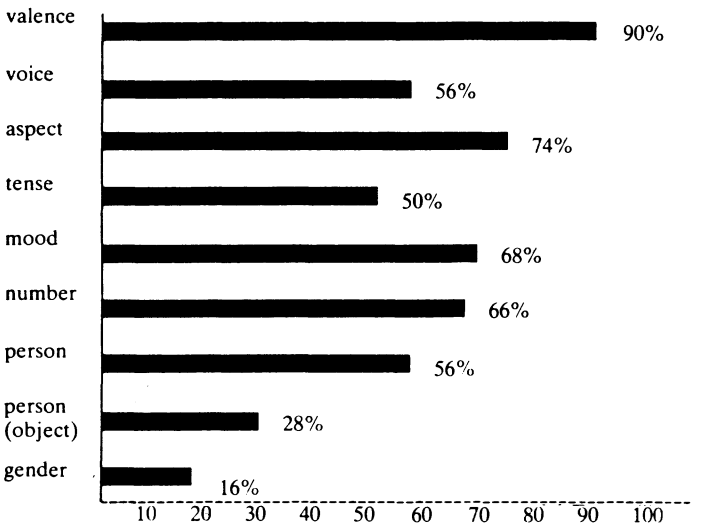

FIGURE 2. Derivational and inflectional categories marked on verbs.

one might wonder whether they should be counted as tenses in the first place; but even more serious is the fact that these few problematic suffixes are treated on a par with full tense systems: both count for 2%.

Fig. 1 contains only the percentage of languages in the sample which show the relevant inflectional categories. It is supposed to be bell-shaped, and it is; but it does not conform as well as one might wish to B's hypothesis. Subject agreement (B's person and number), at 56%, is the second highest category—below only mood, and above tense and aspect. B states on p. 23 that agreement categories are less relevant to the verb: 'Thus we expect subject and object agreement categories to be less frequent than categories that more directly affect the verb.' But that expectation proves false. B attempts to clean things up by grouping tense, aspect, and mood together in comparing them with subject agreement, which gives her 72% vs. 65%. But that's post hoc; it only weakens her case for the centrality of tense and aspect, by showing that all but two languages have an implicational relation between tense/aspect and mood. Mood, which B had argued earlier to be less relevant than tense and aspect—and which should, she says, 'occur less frequently as an inflectional category of verbs than aspect and tense' (22)—is instead, at 68%, the key to the universe.

B's first claim for Fig. 2 is that the categories that are higher on her relevance hierarchy should appear in a greater number of languages. Does this prove true? Clearly, the numbers are not perfect, both voice and tense being out of line; but are they good enough? There is a statistical test, rank order correlation, that could be performed to see how far out of line things are, but B doesn't use it. In fact, she uses no tests at all here, contenting herself with this statement: 'In general, both sets of results conform to the predictions made by the hypothesis, although neither conform perfectly' (29).

Looking at the two figures together, i.e. subtracting the numbers in Fig. 1 from those in Fig. 2, we find large differences in the first three categories (valence 84%, voice 30%, aspect 22%), with very little difference in the remaining six categories (tense 2%, number 12%, and none in any of the rest). I have noted above that B's one example of derivational tense is suspect; that leaves us with number. B discusses all six putative examples of derivational number of Chap. 4. Two languages, !Kung and Ainu, have no inflectional agreement on verbs, but contain a few verbs whose form varies according to the number of the absolutive noun phrase. In the other four languages, however, 'plurality' includes such notions as 'distributive' and 'iteration'; and in at least three of these languages, the plural marker is used when there are either plural objects or subjects. But inflectional number agreement and subject person agreement almost always co-occur in B's sample—Diegueño alone has it for person but not number; no languages have inflectional number agreement but not for person; and 27 languages show agreement in both person and number with the subject. Therefore it seems best to consider at least three of B's putative cases of derivational number agreement as something else. She herself says: 'This type of verb plurality is more like an aspect' (104). We are left with two good cases, for neither of which B has much semantic information. Even if we admit these, what the figures seem to indicate is that there is a very sharp break: the categories of tense, mood, subject agreement, and object agreement are almost always inflectional, but valence, voice, and aspect may also be derivational. B actually notes this difference in the chart on p. 24. This break is fairly strong counter-evidence to B's proposed relevance continuum, which predicts a gradual drop-off.

B also says that relevance should predict the order of morphemes: more relevant implies closer to the stem. Here again, although she summarizes the results, she performs no statistical tests on the numbers given. However, the numbers go the same way most of the time, showing an apparent tendency toward this order, outward from the verb stem: aspect, tense, mood, and person/number.

To my mind, this is the most important finding in the book. From this order, we can also predict a ranking of fusion: the categories that are generally closer to the stem are also most likely to fuse with it. B discusses this prediction only informally—though she notes, based on admitted guesswork, that 'stem modifications associated with aspect are about twice as frequent as those associated with other categories' (37). If true, this point would be remarkable: it would separate aspect from tense as well as from mood and person, in a fashion exactly parallel to the break between aspect and the other categories that was found in the discussion of the Figures. Unfortunately, B does not go beyond conjecture on this important issue: exactly how to interpret this parallelism, or the break itself, is unclear. B does not directly discuss the stem changes in association with valence and voice. If they are less prevalent than those associated with aspect, that would be a problem for her relevance-based account of stem changes.

B feels that she has demonstrated that 'Some categories occur more frequently in the languages

of the world, and these same categories tend to occur closer to the verb stem, and exhibit a greater degree of fusion to the stem' (38). I am not sure how to interpret this claim. The Figures show that mood is the most common inflectional category, at 68%; yet it neither occurs close to the stem nor exhibits much fusion. Tense, which does come closer to the stem, is much less common at 48%; subject agreement, at 56%, is more common than tense but appears further out. The prediction works only for aspect, and then only if we are willing to accept 74% as the relevant figure (52% from inflection plus 22% from derivation).

B next proposes a diachronic account of how relevance influences the evolution of morphological categories. She reviews the idea that (iconic) morphology is entirely fossilized (iconic) syntax; however, using data on morphological restructuring, she rejects this in favor of 'universal synchronic principles of linguistic organization' (41) implemented partly in diachronic terms. One of these, according to B, is the Relevance Principle. Regardless of whether this is true, B's argument for universal (really panchronic) principles is an important one to which I will return.

Chap. 3 deals with an interesting question that has had little attention in the descriptive literature: the organization of verbal paradigms (but see Williams 1981, Carstairs 1984). B claims—based largely on data from child language, experimentation, and language change—that the traditional view of a paradigm as a structured list of full surface word-forms, some of which are more basic than others, is correct. She argues further that the structure of the paradigm is essentially based on semantic factors. B begins with the usual question: What is the basic form of a verb? Most modern descriptions would choose the stem; but B, because of her commitment to the surface, must pick some fully inflected forms—which makes the question more interesting. She says that the most common basic form is the 3sg. present indicative—which, she notes, is also the form generally used first by the child, and usually occurs most often. In fact, following Mańczak 1980, she identifies the singular as the most frequent number, present as the most frequent tense, indicative as the most frequent mood, and 3rd person as the most frequent person. She therefore picks each of these as basic, thus reducing the notion of basic form entirely to frequency. B notes, following Jakobson 1939, that zeros tend to occur in unmarked or basic members of categories. She then uses the distribution of zero inflections in her sample as a kind of check against her frequency-based determination of what is basic—with very nice results (54).

B next attempts to account for how zeros arise. The most obvious account is Zipf's Law of Least Effort, according to which the most frequent items will be the shortest; but B nowhere cites this greatest of all functionalists. She notes (54) that zeros exist 'because markers arise for one member of a category, creating an opposition with the other member, for which no marker arises'; but she also discusses several examples of restructuring in which an old 3sg. marker is reinterpreted as a tense marker, with the result that the 3sg. is marked by zero. She attributes this development to the over-all frequency of the 3sg., thus nicely reducing the acquisition and restructuring facts to frequency. B argues, though, that a paradigm may have more than one listed form—and that two other factors besides frequency determine what form is basic or listed, or whether a particular word form has a separate lexical representation. The first factor is semantics: 'A semantically basic or unmarked word is likely to have a separate lexical entry, while a word that is semantically derived or restricted in function is less likely to have a lexical entry of its own' (57). The second factor is morphophonemic irregularity. B also invokes frequency in more complex ways, noting that a very frequent form (even if it is semantically and morphologically regular) may be listed separately, and that a more frequent paradigm may thus have more than one form listed. She also notes that a more frequent paradigm tolerates more irregularity (again cf. Zipf). Lexical listing is

thus a function of both frequency and irregularity, with the latter at least partly reducible to the former.

B then proposes that paradigms are structured hierarchically according to Relevance: aspect, followed by tense, then mood, then agreement. Frequency (markedness) will determine, she says, what is the basic item (node) on each branch of the paradigm. She argues further that lexicalization is predictable from tree structure. For example, B gives data from Spanish (reproduced here as Table 1), in which forms are first grouped into tenses (present, past, future); then tenses are grouped into moods, with individual person and number forms given for each mood of each tense. She notes that most of these forms can be derived easily from the 3sg. present indicative or 2sg. familiar imperative, which are the most frequent and semantically the most basic. Those which are not easily derived are, says B, themselves highly autonomous (frequent and semantically important), e.g. 1sg. present indicative or 1sg. and 3sg. preterit. For Latin (see Table 2), B first divides a paradigm aspectually, noting that there is often a different stem for the imperfective and perfective (e.g. *caed-* vs. *cecid-* 'cut'); then by tense (present, imperfect, future); then by mood; and finally by person and number.

TABLE 1. Spanish *cantar* 'to sing'.

PRESENT

Indicative

cánto	cantámos
cántas	cantáis
cánta	cántan

Subjunctive

cánte	cantémos
cántes	cantéis
cánte	cánten

PAST

Preterite

canté	cantámos
cantáste	cantásteis
cantó	cantáron

Imperfect

cantába	cantábamos
cantábas	cantábais
cantába	cantában

Past subjunctive

cantára	cantáramos
cantáras	cantárais
cantára	cantáran

SUBSEQUENT

Future

cantaré	cantarémos
cantarás	cantaréis
cantará	cantarán

Conditional

cantaría	cantaríamos
cantarías	cantaríais
cantaría	cantarían

Infinitive: cantár

Present participle: cantándo

Imperative:

familiar: cánta cantád

formal: cánte cánten

Past participle: cantádo

TABLE 2. Latin *caedō* 'to cut, kill'.

PRESENT

Indicative		*Subjunctive*	
caedō	caedimus	caedam	caedāmus
caedis	caeditis	caedās	caedātis
caedit	caedunt	caedat	caedant

IMPERFECT

caedēbam	caedēbāmus	caederem	caederēmus
caedēbās	caedēbātis	caederēs	caederētis
caedēbat	caedēbant	caederet	caederent

FUTURE

caedam	caedēmus
caedēs	caedētis
caedet	caedent

PERFECT

cecīdī	cecīdimus	cecīderim	cecīderimus
cecīdistī	cecīdistis	cecīderis	cecīderitis
cecīdit	cecīdērunt	cecīderit	cecīderint

PLUPERFECT

cecīderam	cecīderāmus	cecīdissem	cecīdissēmus
cecīderās	cecīderātis	cecīdissēs	cecīdissētis
cecīderat	cecīderant	cecīdisset	cecīdissent

FUTURE PERFECT

cecīderō	cecīderimus
cecīderis	cecīderitis
cecīderit	cecīderint

Imperative: caede caedite

Infinitive: Present: caedere; Perfect: cecīdisse; Future: caesūrus

Participle: Present: caedēns; Passive: caesus; Future: caesūrus; Gerund: caedendī

There are problems with both paradigms. For Spanish, B classifies both future and conditional as 'subsequent', to make the conditional parallel to the subjunctive. Can this be justified? What sort of tense is the conditional anyway? B has also listed both preterit and imperfect as past tenses without further comment; but if the difference between these two is aspectual, then we have a counter-example to B's system.

As for Latin, where are the passive forms? As a voice, passive should be superordinate to aspect, and inside it; but here it is not. In fact, for all conjugations, passive is morphologically external to aspect, and to tense. The imperfective aspect has a separate set of word-final passive person and number endings, outside both the tense and mood markers. In the perfective, the passive is periphrastic. Exactly what all this means is not clear; but it is problematic, and the data are familiar enough to make one wonder why the passive paradigm was omitted from B's purview. Indeed, the proper structure of the Latin verb paradigm has been a controversial question for millennia,

and has even been discussed in *LI*. One page of discussion cannot do justice to the complexity of the data, but serves only to raise the specter of oversimplification in the reader's mind.

Even with the limited Latin data that B provides, there are problems. She says (63) that 'The forms of each aspect are more closely related to one another than they are to forms in the other aspect.' It is true that the future perfect of *caedo* is closer to the perfect than to the future in form (both being perfective); but the pluperfect is exactly parallel in its person and number markers to the imperfect, cutting across aspects. In other conjugations (e.g. the first), future and future perfect also share person and number markers. Other formal similarities are simply bizarre, e.g. the use of the infinitive *-er* stem in the imperfect subjunctive, perfect subjunctive, pluperfect indicative, and future perfect indicative. This leads to a fundamental puzzle, which I will call Beard's Problem (see Beard 1981): What do we do with a morphological category that seems to have no meaning at all? Bybee assumes that all morphological categories must have meaning, since that is what determines their relationships.

B presents some interesting experimental results to support her paradigmatic arrangements. She shows, e.g., that Spanish speakers, presented with nonce verbs with alternating vowels, are more likely to use a mid vowel in the 1sg. preterit if they have heard a mid vowel in the 3sg. preterit than if they have heard it only in the infinitive.

Next comes a long section on the mysterious velar augment of Spanish (*salir* 'to go out', *salgo* 'I go out'), which occurs in the 1sg. present indicative and throughout the present subjective of certain verbs. B says that the subjunctive is based on the 1sg. indicative for these verbs, and discusses this point at some length. However, to my mind, B never really provides any evidence for this specific claim.

Chap. 5 picks up where Chap. 3 left off, so I will return later to Chap. 4. B is concerned with the topic of storage in a psychological lexicon, and the first question she asks is which forms are stored and which produced by combination. Her position is basically Bloomfield's—that all arbitrary (irregular) combinations are stored, and that regular forms are stored under certain conditions. According to Bybee, all stored forms, regardless of their regularity, may be strengthened by repeated access; thus stored forms differ in their LEXICAL STRENGTH, largely as a function of frequency. Related word forms, to the extent that they are regular, will be interpreted or derived by accessing a single lexical entry: *playing, played, plays* will thus all be interpreted by accessing *play* and invoking the appropriate morphological rules, thus strengthening the stem *play*. B also suggests that the strength of an entry may decline through disuse, though she does not directly address this interesting problem of forgetting.

B proposes that lexical entries may be connected to one another semantically, phonologically, or morphologically: 'If two words are related by both semantic and phonological connections, then a MORPHOLOGICAL RELATION exists between them' (118). She claims that some morphological relations are stronger than others, and that their strength varies as a function of degree of semantic relatedness, degree of phonological similarity, and the frequency of the members of the pair. The importance of word frequency has been abundantly documented by psychologists, but studiously ignored by linguists of all stripes. B should be applauded for incorporating it into her work, though it is odd that she cites none of the psychological literature, contenting herself with referring to 'numerous experiments'. She translates frequency into lexical strength, noting that irregularity is directly correlated with frequency: the more frequent a form,

the more likely it is to be irregular, and the likelier it is for an irregular form to survive over time (Zipf again).

B claims that, if a form is more frequent, it will be more weakly connected to others, i.e. more autonomous. This statement results in a paradox, since low-frequency forms are also claimed to be dependent on autonomous, high-frequency ones. The paradox can be resolved if we assume that B's notion of lexical connection is asymmetric, making low-frequency forms more dependent on high-frequency ones. This would result in a kind of tree, with lower frequency forms on the lower branches. The dependency might also be variable in strength: the less frequent the form, the stronger the dependency. This type of tree actually fits well with some of B's ideas about the order of categories; but her own diagram (124) has the least autonomous forms closest to the base. She also says explicitly that inflected words are listed in increasing frequency away from the base. Furthermore, at least in this partial diagram, the forms are ordered quite strictly—although B provides no exact justification for the order given.

B also allows a single form to have different phonological and semantic dependencies: thus Sp. 1pl. present indicative *dormimos* 'we sleep' is phonologically dependent on the 3sg. preterit *dormí*, but semantically dependent on another form, presumably *duerme*. B says that this type of situation is not unusual; but if true, it would greatly complicate matters, resulting in a formal network rather than a tree. One wishes, throughout this section, that B had been more explicit and thorough in following up the consequences of her proposal, both negative and positive. Reading this chapter, I sometimes began to see virtue in the style of the Pisa lectures.

There is no stem in B's lexicon: each basic form is a fully specified, unanalysed surface form of a complete word. Yet a single lexeme may have more than one of these basic forms. B proposes that basic forms 'strengthen each other, since they share a number of phonological features' (126). Thus the basic forms of *dormir* all share the stem-like sequence *d*[back vowel]*rm*. This raises the central question of B's earlier book—the abstractness of phonological representation. For example, she says: 'The consonant structure of a Semitic root is the strongest part of the representation.' In Masoretic Hebrew, as well as in Aramaic, the spirantization rule—which turns stops into fricatives postvocalically—leads to the existence of roots in which all three consonants vary across the paradigm. How, one wonders, would B represent these roots?

B's words are not analysed into morphemes; but people do understand and use complex words that they have never heard or seen before. She offers the following explanation:

> 'To the extent that words have internal structure recognizable by the speaker/hearer, this structure can be represented using lexical connections which make segmentation unnecessary. For example, the internal structure of *unforgettable* can be described as a set of phonological and semantic connections to other words in which the three morphemes *un, forget*, and *able* occur. The appropriateness of this mode of representation derives from the fact that it is precisely by recognizing these morphemes in other words that one realizes that *unforgettable* has internal structure.' (128)

I don't see how this proposal differs from a morphemic theory like that of Harris 1951. Again, one wishes that B had paid greater attention to implications. In fact, on the next page, B suggests that her unanalysed forms with lexical connections are not really different from a standard treatment.

The last topic of this chapter is productivity. B notes that the more productive of two rival morphological patterns tends to have a lower token frequency, but a higher type frequency. According to B, this follows from the greater likelihood for low-frequency words to be analysed morphologically; so the morphology of low-frequency words is stronger, leading to greater productivity.

Again, I don't think B is completely wrong, but she makes a complex problem too simple. Frequency is certainly relevant to morphological productivity, but the relevance may not be direct: less productive affixes are sensitive to the frequency of their bases, as B herself shows, but more productive ones are not (Aronoff 1982). Transparency is also relevant (Cutler 1981); but transparency and frequency don't always coincide (Anshen & Aronoff 1981). The difference between analogy and rule may also be a factor in productivity, as B has shown; but some investigators (e.g. Bradley 1980) have argued for a dichotomy between the storage and retrieval of rule-derived and

analogized words. B devotes two pages to productivity, which would be fine if she didn't claim to have an answer to the whole problem.

Chap. 4 is devoted to an attempt to demonstrate 'that derivational morphology is transitional between lexical and inflectional expression, and that the differences that can be observed between inflectional and derivational expression are just more prominent instances of the differences identifiable among inflectional categories' (82). I find this chapter the least interesting in Part I. It contains many examples and arguments, but the nature of B's point is such that conclusive arguments are difficult to find.

In fact, in her zeal, B sometimes contradicts herself. Thus, on p. 85, she notes that number may be a derivational category because 'a change in number produces a change in the entity or entities being referred to'; but on p. 93, she says: 'Ordinarily the occurrence of one vs. the occurrence of many does not change the inherent quality of the entity.' On p. 99, in a discussion of generality, B says: 'In cases where similar conceptual content is expressed in the two different ways, we find that the inflectional expression requires a fully general meaning, while the derivational does not.' On p. 101, in distinguishing 'imperfective/perfective' from 'habitual/continuous', she says that the latter distinction is never derivational. On p. 143, however, we learn that 'habitual/continuous' is more specific than 'perfective/imperfective'. If 'more specific' means 'less general', then we have contradictory explanations of the same phenomenon.

Part II is much less theoretical in tone; but it does contain one interesting claim, that grammatical morphemes evolve from lexical morphemes by a process of semantic and phonological erosion. In three separate chapters—one each on aspect, tense, and mood—B discusses the various meanings of these categories as they appear in the languages of her survey. However, since her survey is restricted to inflectionally marked categories, her discussion of semantics does not include (except peripherally) cases where a grammatical category is marked by a free form—e.g. an auxiliary verb.

In her summary Chap. 10, B deals with the conceptual uniformity of the categories mood, tense, and aspect. She draws three important conclusions from her survey:

(a) 'The meanings coded as verbal inflections are very similar across languages of diverse genetic and areal affiliation and distinct word-order typology.'

(b) 'The mode of expression associated with particular grammatical meanings is also highly consistent across languages. There are correlations between meaning and mode of expression when bound and non-bound grammatical morphemes are considered, as well as when inflectional vs. derivational morphemes are considered. There are also correlations between meaning and the pre- vs. post-position of an affix.'

(c) 'The order of inflectional morphemes (usually with respect to the verb stem) is consistent across languages. There are also correlations between the meaning of a morpheme and the degree of fusion with the verb stem it exhibits.'

If these results are true, then, as B notes (201), there must be 'strong universal factors governing the development of inflectional morphology'. Thus, if she is correct in claiming that grammatical categories and their markers develop historically from lexemes, we are dealing with a teleological system, in which 'The sorts of morphemes that undergo semantic generalization and grammaticization are similar across languages, as are the grammatical meanings that result from this process. That is, there are certain universal paths of semantic change

leading to grammaticization.' So the end of the path, as much as its beginning, is important: change is at least partly directed toward the specific systems that B has isolated. This is not to say that all morphological change is teleological. Frequency and pragmatics are also important factors—though it is interesting that B herself argues for universal rather than language-particular causes, based on 'communicative needs'. As B emphasizes, no category is present in all languages. Even tense, an apparently very useful category, occurs inflectionally in less than half the world's languages; among those without tense, many have gone for millennia with no obvious signs of deprivation. In the absence of a demonstrable lack, the explanatory value of communicative need is severely diluted.

In the end, B cannot account for this teleology, precisely because her explanations are very general, of the pragmatic sort usually associated with radical behaviorism; but the teleology is very particular. There are, of course, theoretical views for which results like B's are not at all problematic; e.g., a parameterized innatist theory would be quite happy with them. Proponents of such theories should read this book.

Bybee's book is fairly easy to read, the design is clear, and an index is provided. There are some discrepancies between the text and the references, with respect to dates of publication, and I found a few minor typos. But overall, by recent standards, this is a well-put-together volume.

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