

Reflections on usage-based phonology

Review article of

Joan Bybee, *Phonology and Language Use*. Cambridge: Cambridge University Press, 2001, xviii + 238 p.

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The basic aim of this new, stimulating and provocative book by Joan Bybee (henceforth B) is to defend the idea that language use plays a crucial role in shaping the form and content of sound systems. It is an excellent and well composed synthesis of her ideas on this topic, in which many detailed studies by herself and others find their place in a clearly articulated model of morphology and phonology. Phonologists and morphologists will be challenged by this book to reconsider their own ideas about the nature of phonology and morphology.

The starting point and theoretical core of B's model of phonology is her view of the mental lexicon as defended in a number of B's earlier publications, in particular Bybee (1988). The mental lexicon is the place where the experience of each individual language user with the words and phrases of the language is represented. This means, according to B, that words are listed in their concrete phonetic forms, including all kinds of phonetic detail. The representation of each word is affected by its frequency of use, which receives a psycholinguistic interpretation in terms of level of activation. The different forms of a word (in the sense of lexeme) are all listed individually, in their concrete phonetic forms. That is, there is no notion 'underlying form' in the sense that underlying forms form the phonological representation of the morphemes out of which a word is composed. There are no phonological or morphological rules in the sense of 'symbolic rules': "generalizations over

forms are not separate from the stored representation of forms, but emerge directly from them" (p. 7).

Before I will deal with phonology proper, it is useful to first give some attention to B's view of morphology in relation to the lexicon given in Chapter 2 of her book. B's morphology is word-based: the words of a language that are stored in the mental lexicon, form the basis for morphological generalizations (the discovery of patterns) that may lead to the formation of new words and word forms. This view of morphology (lexeme-based morphology) is shared by many morphologists: morphology is not the concatenation of morphemes into complex words (morphology as 'the syntax of morphemes'), but the extension of patterns of existing systematic form-meaning correspondences between words. This is, for instance, the view of morphology in the Dutch morphological tradition, as in the work of Schultink (1962), Van Marle (1985), Van Santen (1992), and Booij (2002). It is also the view underlying Aronoff's word-based morphology (Aronoff 1976), and Bochner (1993). This view of morphology may also be qualified as 'paradigmatic morphology', because it is the paradigmatic relations between words that form the starting point for the analysis of the morphological system. The psycholinguistic evidence for the reality of paradigmatic relations between words is abundant, for instance from the recent work by Harald Baayen, Andrea Krott, and their colleagues on family size effects (De Jong et al. 2000, cf. also Hay 2001, and the references given there). This evidence converges with the linguistic (systemic) evidence for paradigmatic relations between words: the need for paradigms in linguistic analysis (Stump 2001), and all kinds of paradigmatic word formation (Van Marle 1985, Becker 1990, Booij 2002).

This does not mean that such morphologists would agree with the more specific theoretical positions that B takes in this book, and previous related work. First, B denies the existence of autonomous symbolic morphological rules, and suggests that the formation of new words can be accounted for in terms of subsymbolic connectionist modeling, or in terms

of (output-oriented) schemas, emergent generalizations over complex representations. In line with the latter position, linguists such as Skousen (1989) and Becker (1990) have proposed to model this kind of morphological competence in terms of analogical operations on symbolic representations, and there is strong evidence for the role of analogy in morphology, for instance, in the choice of linking elements in compounds (Krott 2001), and even in the case of productive and fully regular past tense formation in Dutch (Ernestus & Baayen 2001).

However, this does not necessarily mean that all word formation is based on analogy. One may assume that, in addition to analogical word formation, very productive patterns of word formation cause the language user to conclude to rules, in addition to analogical patterns (Bauer 1993, Pinker 1999, Clahsen 1999). The choice between these positions is obviously a very complicated issue which cannot be dealt with thoroughly in this review (cf. the many reactions to Clahsen (1999) in the same issue of *Brain and Behavioral Sciences* in which Clahsen (1999) has appeared). The only point I want to make here is that one can agree with much of what B argues for as to the nature of the lexicon, and yet not give up the idea of symbolic morphological rules.

Starting from this view of the lexicon as a network of relations between words, B develops her view of the phonological representation of words. Two essential claims of B are that, in contrast to the position taken by classical generative phonology, (i) lexical phonological representations are not redundancy-free, i.e. they also contain predictable information, and (ii) they contain lots of phonetic detail.

As to the first claim, it is convincingly motivated. Note that, as far as morphology is concerned, this position has also been taken by Jackendoff (1975) and Bochner (1993). In B's model, the assumption that phonetic details are stored in representations is a natural consequence of her preference for an exemplar-based model of representation, in which the different phonetic renderings of a word by different speakers are stored in memory. The best

exemplar is that with the highest frequency. Note, however, that this cannot be the full story: there must be a more abstract speaker-independent phonetic representation, which abstracts away from the experienced individual realisations of a word, for the purpose of perception: the hearer is able to recognize a word uttered by an unknown speaker. Knowledge of the individual variation in the phonetic realisation of words clearly does not impede recognition. B is not very explicit on this point which, I feel, requires further research.

Let me add, that B provides convincing linguistic evidence for the claim that allophonic details which are predictable by rule are nevertheless stored. There are many cases of language change in which a once productive and general process gets lost, but the effect of that process is kept in a number of words. For instance, open syllable vowel lengthening, once a productive process in Dutch, has disappeared, but there is still a number of words with vowel length alternation. Hence, the, at that time allophonic length differences must have been encoded in the lexical representations of these words in order to be able to survive (Booij in press). This argument also applies to variable rules, characteristic of casual speech, such as vowel reduction and schwa-deletion in English. Such processes exhibit lexical diffusion in that high frequency words are the first to be affected. Since there is a correlation between word frequency and the readiness with which a word is subject to such processes, the reduced forms must be stored in addition to the unreduced ones, and will receive a higher level of activation, the more they are used. The fact that high frequency of use leads to a higher level of activation of reduced forms can only be accounted for if we assume that these reduced form receive their own representation in memory. Similar observations can be made about vowel reduction in Dutch (Van Bergem 1995, Booij in press). Actually, it has always struck me that native speakers of Dutch have such clear intuitions about the feasibility of applying vowel reduction to specific words. For instance, when you ask a Dutch speaker if he/she is able to reduce the /i/ (in the unstressed syllable) of the words *minuut* 'minute' and

piloot 'pilot' to schwa, you get the same answer time and again: yes for the relatively high frequency word *minuut*, and no for the relatively low frequency word *piloot*. So speakers have intuitions about such facts, which is only possible if their experience with such words is somehow stored.

This also implies, as B correctly points out, that one word may have more than one phonetic representation. As Ernestus (2000: 141) has shown, the number of different phonetic forms for words of high frequency is sometimes quite spectacular. The Dutch word *natuurlijk* 'naturally, of course', for example has at least 14 different phonetic realisations. The remarkable thing is that apparently the hearer is able to deal with this enormous variation without any problem. As B points out, the number of different realisations may also depend on meaning and context. This generalization is supported by facts from Dutch: the Dutch inflected adjective *rode* 'red', can be pronounced with an intervocalic [d] or [j], but in specific uses it is only the form [ro:jχ] that is correct, as in *rooie rakker* 'lit. red crook, socialist'. Many more of such examples from Dutch could be provided (cf. also Ernestus 2000).

One aspect of phonological structure that would perhaps have deserved more attention is prosodic structure. B claims that syllable structure "emerges from the inherent nature of the organization of gestures for articulation" (p. 85). Again, the question arises if, though this may be true from an evolutionary point of view, this is the full story: such a position does not necessarily exclude that the syllable has developed as a formal category, with its own, autonomous systematics, through the interaction of phonetic substance and the cognitive capacities of human beings. Here, we touch a very general point in the discussion between functionalists and formalists. As Newmeyer (1998, 2000: 390) and others have pointed out, there is not necessarily an incompatibility between autonomous formal grammars and a functional explanation for why grammars have the properties they have.

A related question is to which degree the abstract prosodic structure of a word forms part of its lexical representation. B is not very explicit about this point, but I think there is indeed psycholinguistic and linguistic evidence for this assumption. The independence of prosodic structure from segmental structure manifests itself in speech errors in which segments are exchanged but keep the same syllabic position. Thus, consonants of two words in a phrase that occur both in onset position may be exchanged, as in the classical example of a spoonerism *the bean is dizzy* instead of *the dean is busy*, and in the error *peel like flaying* instead of *feel like playing*, but not two consonants occurring in different syllabic positions. It is certainly true, as B points out, that the phonetic realisation of onset consonants (before a sonority peak) and coda consonants (after a sonority peak) is different, and hence phonetic differences form cues for syllable structure, but it is not clear to me how this can explain the 'identity of syllabic position constraint' in speech errors. Models of speech production such as that defended in Levelt (1989, 1992) assume therefore that the syllable structure is present in lexical representations in the form of an abstract skeleton, besides the sequence of segments. This implies that the prosodic structuring of words is not just something that the speaker can compute on the basis of observed patterning, but is itself part of the lexical representations of words. The prosodic skeleton and the segmental content will then be brought together in the phonological encoding stage of speech production.

The presence of prosodic structure in lexical representations can also be motivated on internal linguistic grounds. For instance, in linguistic descriptions of exceptional stress patterns we find statements such as: a final light syllable is extrametrical (that is, does not count for stress assignment), a statement which also presupposes that the syllable is part of the lexical representation, even though its presence as such follows from the presence of the corresponding segments. Similarly, in a proper account of Dutch word stress we must be able to state that a superheavy syllable at the end of a word always bears the main stress, where

'superheavy' means: either a long vowel followed by a consonant, or a short vowel followed by two consonants. That is, we need an abstract 'geometric' definition of the notion 'heavy syllable'.

B's model of phonological representation also differs from classical generative phonology in that there are no abstract underlying forms: the phonetic form of a morpheme is represented as part of the word in which it occurs. For instance, both members of the Dutch singular-plural noun pair *hoed* [hut] - *hoeden* [hudχn] 'hat - hats' will be represented in their phonetic forms, and an abstract underlying form /hud/ does not play a role: the effects of the automatic rule of devoicing of obstruents in coda position are stored in the lexical representation. What remains to be accounted for in such an approach is how the (first) morpheme of the two words will be lexically connected and recognized as the same morpheme, notwithstanding the phonetic difference with respect to the feature [voice] of the third segment. Such a connection must be assumed because these words are lexically connected, as is also clear from cumulative frequency effects (cf. Baayen et al. 1997). Note that, for such an account to be possible, the model must allow for the storage of regular inflected forms, which it actually does. However, it is not fully clear to me how in B's model phonological constraints such as Coda Devoicing serve to indicate that in both words we have to do with the same morpheme. Moreover, we may expect the lexical connection between these two words to be stronger than in cases of 'real' allomorphy in which the variation in the phonetic shape of morphemes is of a lexically restricted nature, and does not follow from a general phonological constraint. The nice point about the classical distinction between automatic phonological rules (leading to abstract underlying forms) versus allomorphy rules (redundancy rules with respect to listed allomorphs) is that it correctly predicts that paradigmatic leveling eliminates real allomorphy, but not the phonetic variation that is due to automatic phonological rules. The alternations of the type [hut] – [hudχn] mentioned above

that are governed by a transparent phonological generalization are remarkably stable, and we do not see leveling into [hut] – [hutχn], whereas leveling occurs frequently in the case of lexically governed alternations. Somehow, therefore, this distinction should also find a place in models of phonology that list both kinds of phonetic variation in the mental lexicon, unless there are other explanations for this difference.

B's position with respect to the treatment of alternations, which is in line with the claim that effects of phonological rules are stored, has a number of advantages. One interesting argument given by B on p. 143 is that alternations within words are more common than across word boundaries, exactly because words are stored in the lexicon, and hence such alternations can lexicalize easily. Another strong argument is that it makes it explainable how phonological alternations can be come associated with morphological categories, and hence may differ in their application depending on the morphological context (p. 102 ff).

If each word receives its own phonetic representation in the lexicon, as B proposes, the classical distinction between word phonology and sentence phonology, already advocated by structuralist phonologists such as Trubetzkoy and Van Wijk (cf. Van Wijk 1939), can be seen as a derived effect. In particular, the ordering of lexical phonology before post-lexical phonology follows if the effects of lexical rules are stored in the lexical phonological representations which then form the input to the sentence phonology (processes which apply across word boundaries). For example, as argued in Booij (1997), the rule of Coda Devoicing of obstruents in Dutch is a lexical rule. In verb-clitic sequences, which are formed in the syntax, the stem-final obstruent of the verb is voiceless even though it stands in onset position (the clitic fuses prosodically with the verb (as in *had-ie* [hΨti] 'had he' with the syllabification pattern hΨ.ti), and hence the verb-final obstruent stands in onset position.) If the phonetic form of *had* is lexically represented, it is explained why the attachment of a vowel-initial clitic (with resyllabification effect) does not bleed Coda Devoicing.

This approach also solves a problem for Levelt's (1989, 1992) theory of phonological encoding. Levelt argued that the segmental composition of words and the prosodic skeleton (syllable patterning) should be assumed to be mapped onto each other at the level of connected speech, because lexical boundaries and prosodic boundaries do not necessarily coincide. Nevertheless, in a sequence such as *had-ie* the /d/ is systematically devoiced although it does not occur in coda position: the syllabification pattern is [hΨ.ti] (Baumann 1996). This observation would force us to conclude that phonological encoding takes place in two steps, first at the lexical level, and then at the postlexical level, unless we assume that the effect of Coda Devoicing is already available in the lexical representation of *had*.

It should be noted that the abolition of abstract underlying forms is not an exclusive property of usage-based phonology. It has also been proposed by Burzio in a number of publications (cf. Burzio 2000), who makes use of output-output correspondence constraints which require the phonetic shapes of a morpheme in different morphological contexts to be the same. In Burzio's analysis (Burzio 2000: 63) of Coda Devoicing in Dutch, the phonetic shape of the singular noun *hoed* ([hut] instead of [hud]) follows from the ranking of the phonological constraint of Coda Devoicing above Output-Output Correspondence constraints. Hence, the form [hut] is preferred to [hud].

The latter analysis illustrates an important point: if we assume phonetic forms of words to be (part of) their lexical representation, we nevertheless need a computational mechanism that tells us what the phonetic shape of a *new* word will be. For instance, if we coin a Dutch plural noun by means of the suffix *-en*, the addition of this vowel-initial suffix to a monosyllabic noun stem that ends in a consonant will change the prosodic status of that stem-final consonant from a coda consonant to an onset consonant (as in *zoon* 'son' with the plural form *zo.nen* 'sons'). This can be computed by a phonological module in which phonological constraints such as the No Empty Onset constraint take precedence over the

correspondence constraint that requires morphemes to have the same phonetic shape in different words. In other words, assuming lexical representations with concrete phonetic forms does not mean that we do not have a phonological computational module as part of the grammar. It is this kind of transparent phonological generalizations that B refers to as “a set of neuromotor production schemas” on p. 64 of her book, and which may be identified with the ‘automatic phonological rules’ discussed above.

The role of abstract phonological categories might also be seen in erosion phenomena. As observed in Booij (1999) with respect to Dutch, when polysyllabic complex words have become opaque and are subject to phonological reduction, the erosion patterns are such that the reduced forms move into the direction of the trochaic form, the trochee being the unmarked foot of Dutch. Thus, the highly frequent complex word *natuurlijk* ‘naturally’ can be realized as [ty:rlɣk] or even [tylɣk], with stress on the first syllable.

The question as to the presence of abstract structure in lexical representations might also be asked with respect to morphological and syntactic properties. To give a simple example, the fact that the language learner has to learn that a specific word is a noun on the basis of regularities in its distributional patterning and morphological behaviour does not preclude that the learner will assign the formal category label noun to that word, so to speak as a handy summary of one's experience with that word. For the same reason, it is a defensible position that morphologically complex words are lexically represented with their morphological structure. For instance, *eater* may be lexically represented as [[*eat*]_{ver}]_N. Assigning this structure to *eter* means that the paradigmatic relationships between words is projected onto the syntagmatic axis of language structure. There is a wealth of evidence that the internal morphological structure of a complex word, once formed, remains accessible to the language user, that is, evidence against ‘seamless morphology’ (Carstairs-McCarthy 1993, Booij 2002). B’s paradigmatic representation (that is, through lexical relationships) of

morphological complexity of a word can certainly do justice to this kind of observations. Nevertheless, the question remains if we also need to assign a syntagmatic structure to complex words, for instance for the proper assignment of stress in languages where stress assignment to a word is co-determined by its morphological structure. For example, nominal compounds in Germanic languages usually have main stress on the first constituent, and in order to express this generalization we have to refer to the syntagmatic morphological structure of compounds.

Complex words, even though their base word have disappeared from the language, and thus lack a crucial paradigmatic relation that serves to establish their being complex, may still behave systematically as if they are complex. This is the case for prefixed verbs in Dutch (Booij 2002) with a verbal stem that does not occur as independent word anymore, and yet take the past participle without the prefix *ge-*, which is the past participle form of prefixed verbs. This may be taken to suggest that the presence of internal morphological structure in a word is not completely a derivative of its being linked to other, morphologically related words, although it is certainly the case that the level of activation of the internal word structure of a complex word correlates with the existence of related words and their frequency (Hay 2001). On the other hand, the identification of *be-* in the Dutch verb *beginnen* ‘to begin’ as a prefix is certainly made easier because whenever a Dutch verb begins with the phonological sequence /bχ/, it is indeed a prefixed verb, and hence the lexical connections between *beginnen* and all other verbs with the same initial segments may suffice to establish its being a (formally) complex verb.

As B convincingly argues, not only words, but also phrases and constructions may have to be stored in the lexicon, with (part of) their phonetic form. For instance, in the phrase *I don't know* there is considerable reduction of the phonetic form of *don't* (p. 162). As B puts

it, "when words occur together frequently, they begin to behave phonologically as if they constituted a single word" (p. 161).

There is indeed a lot of evidence for the notion 'construction', a notion which is defended in particular in the framework of Construction Grammar. Note, however that accepting the necessity of the notion construction (multi-word combinations whose properties cannot fully accounted for compositionally) does not force us to accept the claim that the grammar is nothing but a set of constructions, as some linguists may want to conclude. B's contribution here is that she presents phonological evidence for the notion construction. I fully agree that constructions may exhibit specific phonological properties that non-grammaticalized syntactic configurations do not have. Thus, B is able to give a convincing account of the French liaison phenomena by decomposing the relevant patterns into a number of constructions with specific liaison consonants. However, B also takes a more radical position in claiming that phonological cohesion as the effect of frequency of cooccurrence of words is also the source of syntactic constituency. Thus, there would be complete isomorphy between syntactic and phonological structure (p. 185 ff). For instance, since the combinations of subject pronoun + auxiliary is often reduced phonologically, B wants to treat such combinations as one grammatical word and concludes that this "suggests an analysis of English much like that of Quileute, where the forms of subject pronouns are determined by modal functions" (p. 186). This would also mean that German phonological words such as *am* and *zum* are no longer treated as representing the syntactic sequence preposition + determiner, but as one grammatical word, a kind of inflected prepositions. Although this is certainly a logical option, I feel we should be reluctant to take such a step since it may make the syntactic analysis of a language much more complex. Take another example: in Dutch the vowel-initial clitic *-ie* [i] 'he' forms one prosodic word with a preceding complementizer such as *dat* 'that'. If we follow B, the sequence *dat-ie* is also one grammatical word, and Dutch becomes a

restricted pro-drop language in which subjects need not be overt under the condition that the complementizer is 'inflected' for person and number, and thus licences the drop of an overt subject. Thus, the syntax of Dutch would be made considerably more complex than it is in an interpretation in which we do allow for an asymmetry between syntactic and prosodic structure. B shows herself to be sceptical about approaches that make a distinction between grammatical and phonological words:

"Thus, phonological rules are said to apply within 'phonological words', which are often arbitrarily defined. In fact, often the only evidence that a certain sequence of words or morphemes constitute a phonological word is that phonological rules apply within it". (p. 157)

This statement ignores the result of many careful studies in which different and independent kinds of evidence for the phonological word are presented (a good summary of this kind of evidence is given in Hall & Kleinhenz (eds. 1999)). As Levelt (1992) pointed out, it is quite reasonable to assume that the prosodic structure of an utterance does not fully align with its lexical structure, because the fluency of connected speech may require this.

In the last chapter of her book, B defends the claim that universals are primarily the effects of language change. For instance, "the cross-linguistic patterns of syllable structure are considered to be the result of a diachronic tendency by which syllable-final consonants gradually weaken and delete" (p. 195). B praises Hayes for not being content with putting cross-linguistic generalizations directly into the innate language acquisition device, but opposes Hayes' view that biological evolution and language acquisition are the temporal factors that shape the grammars of natural languages. According to B, "an essential component to any explanation for universals is reference to the mechanisms of change that

create these paths of change" (p. 196). My problem with this last explanation is that language change is not an autonomous mechanism, but the effect of reanalysis and language acquisition mechanisms including imperfect learning (the interpreting mind of the language user and the language acquirer) in which the psychological factors (the tense between ease of perception and ease of production), in combination with the cognitive capacities of the human mind play a major role. Therefore, I do not see how diachronic tendencies as such can function as independent explanatory factors for universals (cf. Newmeyer 1998: 238ff for similar objections). In fact, on the last page of her book, B also states that language change is not the ultimate explanation for cross-linguistic tendencies. She speaks about "trajectories of change, which themselves are fueled by the human neuromotor ability to produce complex motor sequences by storing and automating recurring stretches of behavior and by the cognitive ability to categorize and organize stretches of linguistic experience in memory " (p. 215). This point is well taken: it is certainly the case that the enormous capacity of the language user to store her/his experience with language use in memory and to categorize this experience into more abstract categories is a hitherto strongly underestimated driving force in language use and language change. Grammaticalization phenomena in general indicate the importance of storage (in combination with reinterpretation), and B's study convincingly shows that the storage by individual speakers of experience with the phonetic form of expressions of a language may co-determine that language's phonology. Thus, this book also contributes to revitalizing the important issue of the proper balance between storage and computation in the human language faculty (cf. Nootboom et al (eds.) 2002), a topic which should indeed be high on the research agenda of present-day linguistics.

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