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REVIEW ARTICLE

# Some handy notes on phonology

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**J. A. Goldsmith (ed.),** *The handbook of phonological theory*. Cambridge, MA & Oxford: Blackwell, 1995. Pp. xiv + 986.

## 1. General

To assess a book of nearly a thousand pages with thirty-two contributions is something of a tall order, if not an impossible task, if one wants to do justice to all of it. For this reason, I have chosen to approach it from my own standpoint on phonology and see to what extent the issues I see as crucial to the current vigorous debate in this area have been addressed and to examine how they have been dealt with. First, however, I will present a general description and discussion of the book as a whole.

To commit oneself to a series of handbooks costing around £60 to £70 in hardback only, as Blackwell has done, is a very brave step in the current climate of institutional cut-backs against a background of financial stringency. Although it is important to have such reference books available, one wonders whether the gathering together of so much material in one volume is the wisest way to disseminate the information, or, indeed, the 'handiest' format. This particular volume covers such diverse topics as: underspecification and markedness (Steriade), dependency relations (Ewen), sign language phonology (Brentari), the syntax-phonology interface (Inkelas & Zec), language games and related areas (Bagemihl), tone in East Asian languages (Yip), and yet there are many approaches to the many topics not represented, for example, Government Phonology and declarative phonology.

Many of the papers contain invaluable overviews of the issues involved, for example, underspecification by Steriade, and the skeletal tier by Broselow. These are extremely helpful in that they gather together strands of argument that are widely dispersed in the literature. They thus provide a suitable background for the current positions being presented.

The language index and the extensive bibliography are especially welcome. The latter is an indispensible source of some quite obscure references. But, unfortunately, there is a serious problem with the references. It is a pity that



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the assiduity that has obviously been expended on putting together and checking the bibliography at the end of the book has not been extended to correlating the references in individual contributions with the full list. Thus, Kiparsky (1981) in the main text may be (1981a) or (1981b) in the bibliography; Goldsmith (1987) could be (1987a), (1987b) or (1987c)! This is not a sporadic inconvenience; in Chapters 3 and 4 alone there are sixteen instances involving different author/year references, in addition to omissions and non-matching dates. Hardly a minor irritation! Generally, however, the presentation and printing standards are fine.

I shall start with an examination of Goldsmith's introductory chapter, simply entitled 'Phonological theory', in which he gives an overview of what he sees as the main aspects of current phonological theory, its problem areas and the proposed solutions, and the way they are approached by the book's contributors. In a cellular representation (3) he gives 'the three traditional questions of phonological theory' (2): phonotactics, alternations and contrasts along one axis, plotted against three different types of theoretical construct: representations, levels and rules along the other, in order to pinpoint issues raised and discussed in the particular papers in the book. Each of these questions is given a section of uneven length. This disparity is, no doubt, in part a reflection of the different number of papers dealing with each one, but in the third section Goldsmith takes the opportunity of having an extended discussion of underspecification theory, presenting his own misgivings about it. Since this is the only place where any criticism of the contributions occurs, it gives a somewhat unbalanced sense to what is, after all, an introduction to the rest of the book. In other words, Goldsmith's introduction falls between two stools: a summary introduction to the contributions, grouping them by theme, and a forum for the debate of core issues in phonology. One can only wonder why underspecification was singled out for particular attention.

Furthermore, in a book which has as its aim 'to provide the readers with a set of extended statements concerning mainstream conceptions of phonological theory' and 'to offer a new synthesis of the ideas that are currently in the field' (x), might we not reasonably expect a discussion of the following: segmental versus non-segmental phonology, phonetic implementation of phonological units, monosystemicity and biuniqueness versus polysystemicity, and derivational versus declarative phonology? The last of these is particularly current – witness the final section of Durand & Katamba (1995). If we find no serious discussion of these matters, then what use is a collection of papers, excellent though many of them are, which are merely classified according Goldsmith's cellular taxonomy?

For instance, in the discussion of Kiparsky's justification of radical underspecification (13–17) Goldsmith points out that a commitment to underspecification must be justified in terms of simplicity of the grammar



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(17). There is, however, a case for underspecification in relation to a declarative approach which needs to be investigated more thoroughly than it is in this Handbook, pace Steriade, whose contribution I return to in section 5. This matter also relates centrally to the discussion of vowel harmony by Harry van der Hulst & Jeroen de Weijer in Chapter 14, which I shall also return to below. Basically, the opportunity for such an investigation has been missed.

I now want to turn to the issues that I feel have an inadequate treatment in the book and deal with them one at a time, although there are certain connections between them in several respects. For instance, the matter of segmentation relates to issues of abstractness and phonetic implementation.

## 2. Phonetic implementation and abstractness

The issue of the abstractness of phonological representations seems to be consistently avoided in the literature generally. There is an assumption in many papers in the Handbook that phonetic implementation is not a problem and need not, therefore, be addressed. There are specific references to the phonetic implementation; for example, in a footnote Clements & Hume point out that their place features '*labial*, *coronal* and *dorsal* have specific phonetic correlates just as other features do' (fn. 6, 301): LABIAL involves the lips, CORONAL the tongue front, and DORSAL the tongue body.

Elsewhere it has been pointed out (Local & Lodge 1996) that there is a problem with such naïve phonetic interpretations of phonological features. Consider a simple example, the phonological status of the feature ALVEOLAR, or its binary equivalent [+cor +ant], as a definition of English /t d n/. As is well-known, these three putative phonological units are subject to (at least) place of articulation assimilation with a following obstruent or nasal; in other words, their exponents in this respect vary in terms of articulatory place: bilabial, labiodental, dental, palato-alveolar, palatal and velar, as well as alveolar. The only thing these features have in common is that they are all indeed place specifications. Clearly, in such cases as this phonological /alveolar/ cannot be equated with phonetic [alveolar].

Fudge (1967) is an early attempt within the framework of generative phonology to introduce phonological primes with no implicit phonetic content. He states: 'It is ... dangerous and misleading to say that either articulatory or auditory features ARE the phonological elements, unless they correlate so closely that no facts of language are obscured by treating them as if they were the same' (1967: 4, original emphasis). The two reasons he gives to support his claim that facts are obscured if one assumes identity of phonetic and phonological features are the matter of biuniqueness (discussed



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also by Chomsky (1964: 75–95)) and morphophonemic patterns, some of which are 'counter-phonetic'. It is important to note in particular what Fudge considers one serious disadvantage of distinctive feature notation, namely that 'systematic phonemic elements and their systematic phonetic counterparts are treated in terms which are formally indistinguishable, and this often forces us to imply that one systematic phonemic element has been changed into another [...]. This is not only undesirable, but also unnecessary, since we do not require complete biuniqueness in our phonology' (1967: 6). I shall return to biuniqueness in the next section, but after nearly thirty years it is time that more than a few phonologists paid attention to these reservations voiced very clearly by Fudge.

Related to this central issue is the separation of phonetics from phonology. Again quite generally, not just in the Handbook, there is inconsistency and confusion in the discussion of the two levels. This is reflected in the inconsistent representations of linguistic forms, so that it is often quite difficult to know what is being discussed, phonetic or phonological forms. The problem is exacerbated by the fact that in many cases the forms being discussed are (at least) secondhand, from another analysis of the data. In such cases it is more a matter of the philological interpretation of written texts than anything resembling a discussion of speech forms. For instance, in Blevins' paper on the syllable (Chapter 6) some transcriptions have square brackets, others do not. What are we to make of representations like Beijing Chinese ní-kè-xùn (= English *Nixon*) without square brackets (228)? Presumably x is not intended to represent a voiceless velar fricative, though ŋ in ní-kè-sōŋ, an alternative pronunciation, represents a velar nasal. And again, what are we to make of statements like '[fəɹməmɪnʔt] can be realized as [fəɹmmɪnʔt]' (233)? One phonetic form realized as another? 'byuutiful peintərz' is a phonetic representation to Mohanan (31). Steriade (Chapter 4, 154 and footnote 41, 172) refers to the feature [retroflex] in Latin (perhaps I'm out-of-date with my Latin phonology!), but in the footnote the discussion slips into being about the feature [rhotic] without comment. Tautologous statements also ensue from this blurring of the distinction between phonetics and phonology referred to by Fudge: Macken informs us 'that the surface [d] is distinct from the affricate /ʤ/' (688). How could it not be; the former is represented as a phonetic entity, the latter as a phonological one? It is equally true to state that surface [d] is distinct from /d/. Furthermore, the metaphors used in describing linguistic structure become mixed up with the discussion of physical reality, for which different metaphors – phonetic descriptions of an articulatory or an acoustic nature – are available. This can lead to claims that are misleading: a good example is Goldsmith's claim (6) that autosegmental analysis removed the abstract underlying segments in cases of absolute neutralization. In the case of Hungarian vowels, 'if we posit a [+back] autosegment as part of a root that associates with affixes, though it fails to associate to one or more vowels in



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the stem, the autosegment is not abstract, since it *does* quite simply appear on the surface.' But surely this is sleight of hand? Even if we accept the phonetic content of autosegments, the abstractness now resides in the non-association with the root vowel(s), rather than with counter-phonetic underlying feature specifications.

All this confusing use of terminology, in particular, 'phonetic' and 'surface', makes one wonder whether there is any consensus as to the meaning of these terms. Rather than being a label for those aspects of speech that are physical, 'phonetic' has come to be associated with a level of linguistic organization. Steriade assumes that surface features are the same as underlying ones and refers to 'phonetic representations' (117) and for Clements & Hume surface structure has the feature organization they propose for phonological representations (250). This way of thinking may have been encouraged by Keating's (1988) paper on 'phonetic under-specification'. To this reviewer this indicates a non-physical interpretation of the word 'phonetic', but by using it Keating and others side-step the issue of phonetic implementation of the phonology. As a phonetician, I would want to ask what such a view assumes about the position of the tongue, lips and velum, for example, during the articulation of [ʔ], [h] and [ə], which, we are told, are 'permanently placeless' (Steriade, 135–136; also see 152–153). Such claims certainly suggest that at least some phonological features have no relevance to phonetics. At what level do we lose interest in the configurations of the articulators and/or their acoustic qualities? If there is a cut-off point, what criteria are there to help us with our decision? There is no indication of this in the Handbook. However, interest in the functioning of the speech organs is used when needed: Steriade, for example, claims: 'The unmarked value of any feature corresponds to the normal neutral state of the relevant articulator' (119); unfortunately, she claims that the default position of the velum corresponds to [−nas], despite the fact that in normal breathing the velum hangs down to allow air to pass through the nasal cavities.

Similarly, van der Hulst & de Weijer discuss the physiological correlates of [ATR] and tongue height (510), though their survey of the literature on investigations into tongue-root activity is far from adequate (see Local & Lodge (1996) for a fuller survey). Their desire to equate phonological entities with phonetic exponents leads them to concentrate their efforts on features of vocoid articulation only (though pharyngeal opening is applied to contoids such as the Arabic emphatics by Lindau (1978: 553)). On the other hand, they go on to establish some kind of 'ideal' or, perhaps, basic ATR-harmony system (their (26), page 511) from which actual systems are derived by (in some cases counter-phonetic) mergers (512–514). They do not state which language has such a system, only that 'a harmony system based on this distinction ([ATR]/[RTR]; KRL) MIGHT have the vowel system in (26)' (my emphasis). I reproduce their (26) as (1) below.



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(1)

| advanced tongue root | | retracted tongue root | |
|---|---|---|---|
| front | back | front | back |
| i | u | ɪ | ʊ |
| e | o | ɛ | ɔ |
| a | | ɑ | |

([RTR] is defined as 'retracted or neutral tongue root position' (511); a case of phonetic agnosticism or does the difference between 'retracted' and 'neutral' matter?) In their discussion of the variant systems, many of which have only one low vowel, they fail to make the point that the relationships between the forms are the basis of an abstract phonological system which is realized by different phonetic characteristics in different languages and dialects. The system is not the same thing as the realizations; therefore, the mergers are of historical interest, but not surprising. Also, in the Nilo-Saharan group at least, it would be important to investigate the consonantal exponents, too, given what Local & Lodge (1996) have demonstrated in the case of Kalenjin.

However, there are a few occasions where a different view of phonological features is entertained. For instance, Blevins (208) refers to 'ballisticity' as a phonological feature of Otomanguean Amuzgo and Chinantecan. The term refers to a cluster of regularly recurring phonetic features, namely fortis initial Cs, voiceless nuclear Vs, final voiceless sonorants, syllable-final aspiration, which Blevins groups together as 'aspiration'; rapid crescendo to peak intensity, with sudden decrescendo; accentuation of vowel length contrasts; tonal gliding; tongue-root retraction. Non-ballistic syllables are 'unaspirated', show even rises and falls of intensity, have unexaggerated vowel length contrasts, do not show tonal gliding, and have no tongue-root retraction. This seems entirely parallel to [ATR] in Kalenjin, as discussed in detail in Local & Lodge (1996). Both ballisticity and ATR are convenient labels for a complex set of distinctive phonetic features spread throughout the syllable. Such phonetic exponency of these phonological features is unlikely to be universally applicable. The study of phenomena like 'ballisticity' and their phonetic implementation is clearly a step in the right direction with regard to the clear separation of phonetics and phonology.

A real opportunity has been missed in the Handbook to address the question of abstractness in more detail. An important point is made by Blevins in a footnote (fn. 31, 239) in which in the context of discussing the abstract nature of the syllable, which is independent of acoustic and articulatory properties of the vocal apparatus, she refers to Brentari's discussion of the syllable structure of American Sign Language (Chapter 20). If ASL has a phonology, then how can phonological elements be phonetically based? Indeed, the inclusion of Brentari's paper is one of the most significant aspects of the collection, not only because of the greater understanding of the nature of sign languages generally, but also as a recognition of the truly



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abstract nature of phonological structure. However, it is hard to see how Blevins can reconcile her astute observation about abstractness in phonology and her discussion of sonority as a crucial factor in determining syllable structure.

## 3. Monosystemicity and biuniqueness

In the previous section I referred to Fudge's discussion of abstractness in phonological primes. The most important reason for distinguishing between phonetics and phonology is what Chomsky (1964) calls biuniqueness. The argument was originally made against a background of structuralist phonemics which equated the two. Any speech sound is interpreted as a segment and classified with a universal set of descriptive articulatory labels. This process of interpretation classifies the sounds as segments of the same type: stops, fricatives, front vowels, nasals, etc. Once such an identification is made, it is fixed for the language in question and all similarly identifiable segments are associated with it. Thus, in English onset [d] and coda [d] are identified as being the same on all occasions; the same applies to all onset and coda realizations. Phonetic differences, for example, delayed onset of voicing in the onset position, pre-release cessation of voicing in the coda, are ignored. This fixing of the phonetic descriptions of sound segments on a once-and-for-all basis occurs before the fixing of the phonemic contrasts. The contrasts are established in the usual way, on the basis of difference of meaning, and also on the basis of the phonetic similarity. Thus, in English both occurrences of bilabial + nasal in the assimilated form [tem men] *ten men* are associated with the phoneme /m/ (see Gimson 1962: 270–271). A word like *ten* would have three phonemic forms: /tem/, /ten/, /teŋ/ and the one ending in /n/ would have the allophonic variants [teɱ] and [teⁿ] besides [ten]. All these forms are the product of assimilation of place before obstruents and nasals. Structural phonemicists were not concerned with native speaker knowledge, so there seemed to be no problem with one word having three different phonemic forms, since they were not forms stored in anyone's mental lexicon. Chomsky (1964) demonstrated that if phonemic forms of words and morphemes are psychologically real for native speakers, then they represent the stored knowledge about each distinct lexical item in the language, and it makes no sense to allow some items to have three distinct forms. Even within a structuralist approach, if phonemes carry distinctions of meaning, then it is still inconsistent to assume one word can have distinctive alternate forms predictable by context. Lexical items must have one distinct form in the lexicon representing the many predictable phonetic variants. For this to be the case, the exclusive association of phonemic and phonetic forms (biuniqueness) must be abandoned. The practical result of this is that occurrences of (similar) constellations of phonetic features do not have to be identified as 'the same'. This applies at both the phonetic and the phonemic



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level. So, to return to our English example, /n/ has the positional variants [m, ɱ, n̪, n, ŋ], as exemplified by the assimilated forms of *ten*, despite the fact that English has the phonemes /m/ and /ŋ/. There is no longer a problem with the claim that the first [m] in [tem men] is an allophone of /n/, whereas the second one is an allophone of /m/.

The reason for this somewhat lengthy excursus into the background of Chomsky's arguments is that the principle of biuniqueness seems still to be assumed in many analyses and is the implicit support of a monosystemic approach to phonology. I will now take a few examples for discussion from the Handbook.

To start with, Goldsmith takes monosystemicity for granted in his introduction. In his discussion of five kinds of contrast (9–13) he refers to cases of neutralization as examples of MODEST ASYMMETRY of contrast:

> 'This involves pairs of sounds, *x* and *y*, which are uncontroversially distinct, contrastive segments in the underlying inventory, but for which in at least one context there seems to be a striking asymmetry in the distribution of segments, judging by the relative number of words with the one and words with the other, or by some other criterion'. (11)

(It is not clear to this reviewer how 'modest' is squared with 'striking'.) This would presumably cover cases such as so-called final obstruent devoicing in standard German (and other languages). Obstruents are contrastively voiceless or voiced in syllable-initial and word-internal position, for example, *Tank*/*Dank*, *Leiter*/*leider*, but in syllable-final position only voiceless obstruents are allowed. In the lexicon there is a set of stems which have alternating forms depending on whether or not they have a syllabic suffix attached to them, e.g. *Rad* [ʀaːt], genitive *Rades* [ʀaːdəs], and another set with non-alternating voiceless obstruents, e.g. *Rat* [ʀaːt], genitive *Rates* [ʀaːtəs]. The very notion of neutralization trades on monosystemicity, if there is identification of the sound occurring exclusively in the one context with one or other of the contrasting phonemes, *x* or *y* in Goldsmith's exposition. On the other hand, Trubetzkoy (1939), the originator of the archiphoneme, did not make this identification; indeed, that was the point of the archiphoneme, it was neither *x* nor *y*. In the case of German it is a straightforward matter to identify the alternating forms with the voiced obstruents, for example, *Rad* is phonemicized as /raːd/, *Rat* as /raːt/, with a devoicing rule in syllable-final position; this is a monosystemic solution. On the other hand, the archiphoneme principle rests on underspecification and an implicit polysystemicity: /t/ is a voiceless alveolar stop, /d/ is a voiced alveolar stop, syllable-final /T/ is an alveolar stop, and all three are distinct from one another in their definitions.

The same applies mutatis mutandis to the vowel system of English referred to by Goldsmith (11–12). What reason is there for identifying as the same the



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stressed vowels of *Canada* and *sanity*, or those of *Oberon* and *verbose*? The latter of each pair alternate according to Trisyllabic Shortening, the former do not. The only answer can be the automatic imposition of monosystemicity based on biuniqueness. Interestingly enough, he refers later (page 18) to Itô & Mester's contribution (Chapter 29) in which they distinguish between four phonologically motivated types of vocabulary in Japanese, comparable to the distinction in English between learned and Germanic vocabulary. Itô & Mester's analysis is essentially polysystemic: the distribution of contrastive sounds is different in the various types of vocabulary. In their constraint-based approach, constraints such as the one on the sequence [si-] apply variably according to the type of vocabulary: native, Sino-Japanese, foreign or mimetic. But, once again, the opportunity of a more general discussion of polysystemicity, a crucial aspect of Firthian prosodic analysis for fifty years (see, for example, Firth 1948 and Palmer 1970), has been missed.

Steriade, too, takes monosystemicity for granted. For instance, she identifies suffixal /a, i, u/ with root /a, i, u/, which also contrast with root /e, o, ɨ/, in Bantu and Chumash (156–157). This can only be justified on the assumption of monosystemicity. And there is a similar case with Hungarian (164–165), where the roots *nal* 'at' and *töl* 'from' as stems in, for example, *nal-am* 'at me', *töl-em* 'from me', are identified with the suffixes which undergo harmony, as in *ház-nal* 'at the house', *ház-töl* 'from the house' versus *kép-nel* 'at the picture', *kép-töl* 'from the picture'. The latter can be unspecified for [back], the former lexically specified as [+back] and [−back], respectively. Allowing vowels to be underspecified in some cases and not others explains the difference between the two types of morpheme perfectly well, provided we give up our insistence on monosystemicity.

In discussing the morphophonemic alternations of voice in *twelve/twelfth* and *dogs/cats*, as opposed to positionally determined allophonic variation, such as aspiration in English, Mohanan (33) claims that the former type of alternation 'is clearly phonemic, because in English, *f* and *s* contrast with *v* and *z* respectively.' He also classifies as this type of alternation the optional assimilation of [hɔrs] in *horseshoe* [hɔrššuu] (Mohanan's transcriptions). What is the important criterion for him in distinguishing between phonemes? Surely, it is difference of meaning (= 'contrast'). But in the case of the morpheme *twelv/f* and the plural morpheme in English there is NO contrast, the variant realizations mean the same thing in each case and are entirely predictable. So Mohanan relies on biuniqueness in the same way Gimson (1962) does. In the case of *horseshoe* the same argument would apply, if we accepted that the two occurrences of [š] were reliably identifiable as the same, but in this case some speakers may round the lips for both occurrences, whereas others have lip-rounding only for the second fricative. In the latter case identity is even more questionable. A simple solution in the latter case is that the unrounded palato-alveolar fricative is an allophone of /s/ before palatal and palato-alveolar phonemes. (On the phonetic difference of



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assimilated articulations see Nolan (1992) and Local (1992), and further references.)

## 4. Segments

I now want to turn to the matter of segmentation. Criticized by Firthian prosodists (see Palmer 1970) and more recently reviewed by Bird & Klein (1990), segmentation has for the most part been taken for granted in the tradition of Western linguistics. This is importantly related to our system of writing with the Roman or other related alphabets (see, for example, Morais et al. (1986), on segmentation and literacy). Even in the approaches that assume a geometry of the kind presented by Clements & Hume in Chapter 7 with autosegments that are claimed to capture syntagmatic relations in the speech chain, the notion of segments as cross-parametric slices is preserved (see also Goldsmith, 1990: 274–298).

At the heart of all linguistic theorizing are the notions of paradigmatic and syntagmatic relations in language. But there is nothing that tells us a priori that paradigmatic relations that establish the meaningful contrasts of a language have to be between segment-sized entities at the phonological level any more than at any other level. In syntax, for example, a 'segment' is usually word-length; the 'segment' is the smallest bit of the speech chain suitable for describing the patterns of a particular level. We segment speech in different ways for different purposes.

In speech it is quite clear that parametric features take up varying amounts of time, so it is difficult to understand the reason for Clements & Hume's statement (268) that 'single segments commonly constitute entire morphological formatives in their own right, while SUBPARTS OF SEGMENTS RARELY DO' (my emphasis), which forms part of their argument in favour of the root node. But isn't plurality in English realized as alveolar friction, and past tense realized as alveolar occlusion? Which phonemes should we identify these features with? Bendor-Samuel (1960; also in Palmer 1970) shows that in Terena nasality has the grammatical function of signalling first person singular, as in (2).

(2) [emoʔu] 'his word' [ẽmõʔũ] 'my word'
[ajo] 'his brother' [ãj̃õ] 'my brother'

Bendor-Samuel shows the inappropriateness of alternative segmental analyses which are rendered unnecessary, if nasality is extracted as a whole.

German provides us with a different problem of segment identification. All phonemic analyses of German establish /r/ in the inventory; however, the phonetic realizations of many standard German speakers make the identification of this segment difficult in anything but onset position.



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Although there is variation between speakers, typical pronunciations are those given in (3).

(3) [baːt] *bat* 'offered'
[bɑːtˠ] *Bart* 'beard'

(For some speakers the whole syllable would have back resonance, which I have indicated with the back vowel symbol and the velarization symbol, but the argument remains the same.) Phonemicizations would be /baːt/ and /baːrt/, respectively. The feature of back resonance is (at least) rhymal in all such cases and yet it is equated with a post-nuclear coda segment. There is, no doubt, an element of history in such an analysis, reflected in the orthography (significantly) and also influence from those cases where a post-nuclear back vocoid articulation occurs with or without a following consonant, as in (4).

(4) [fuːʌ] *fuhr* 'travelled'
[pfe̞ːʌtˠ] *Pferd* 'horse'

In these cases [ʌ] can be interpreted as a segmental allophone of /r/.

Of course, it could at this point be argued that establishing /r/ as the phoneme is an abstraction from the phonetic substance and that this is what was being argued for in section 2. This is true, but unless we have a clear statement of phonetic implementation, it is nothing more than lip-service to abstractness. The focus of our analysis should be on the contrastive patterning in the speech chain, in this case back resonance versus its absence in the rhyme. Because there is something that can be identified as a segment in some cases, as in (3) above or in cases where uvular friction occurs after a short vowel, for example, [haχtˠ] *hart* 'hard', [hɛ̞χtˠsˠ]] *Herz* 'heart', it does not mean that the back resonance does not affect the rest of the rhyme, as I have indicated in the transcriptions, where the vocoid articulations are retracted in comparison with those without such resonance, for example, [geːt] *geht* 'goes', [nɛts] *Netz* 'net'. In other words, just because we can identify [ʌ] and [χ] as segmented entities, why should we ignore the back resonance of the following contoid articulation and the retraction of the preceding vocoid? The contrast is rhymal in all cases. Whether we want to associate this contrast with the contrastive uvular trill in onset position is a matter of our preference for monosystemicity or polysystemicity, though there are some cases where such identification seems justified, for example, [fuːʌ] *fuhr*, singular and [fuːʀən] *fuhren*, plural, in which we have the same stem.

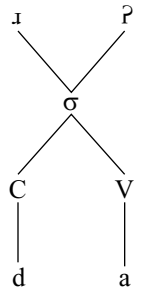
On the matter of identifying the location of retroflexion in a number of Australian languages Evans (739–740) has some interesting observations, in particular that it is usually syllabic in nature and proposes an interesting 'autosegmental' analysis of the alternating forms from Mayali: [ɖaˈʔ],



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[ɖaɹaʔ], [ɖaʔ], 'piece of stringybark' without the encumberance of multiple association lines, as in (5) (= right-hand representation of his (14)).

(5)




Insights of a Firthian nature may well have informed such an analysis (see fn. 1, 759–760); the distribution of the features of retroflexion and glottal closure is variable, but in each case they are represented by the same phonological structure. Further investigations along these lines is certainly called for, as Evans seems to hope.

A point at which further discussion of this issue of segmentation could have taken place is in Macken's contribution on the acquisition of phonology (Chapter 22). Only one page is devoted to phonological units (688–689); we are told that 'from the earliest stages of word use, in most cases, children behave in accordance with the hypothesis of segmented underlying representations'. There is a nod towards Firthian prosodic analysis with a brief reference to Waterson (1987), but the point of Waterson's contribution to the study of phonological acquisition is missed, namely, that adults as well as children have non-segmental phonology. Furthermore, in relation to the notion of monosystemicity, is it necessarily the case that children make the identification of onsets and codas that most linguists assume? What evidence have we that English children associate coda [ʔ] with onset [p t k], as appropriate, until they learn to spell? Or that Greek children analyze [b] in [ti borta] *την πόρτα* 'the door' (accusative) as /-n p-/ or [g] in [ti gori] *την κόρη* 'the daughter' (accusative) as /-n k-/, until they, too, learn to spell?

The length of relevant features of speech varies and need not be determined a priori as essentially segmental. The skeletal tier of many geometries, equivalent to consonants and vowels in other classifications, is clearly a suitable segmental level of organization. We need to refer to onsets, nuclei and codas in making our statements about linguistic structure but the way in which features are associated with this tier does not have to be on a one-at-a-time basis. The example of nasality in Terena and back resonance in German show this clearly. So-called vowel harmony systems typically show similar non-segmental characteristics, for example, Waterson (1956; in Palmer 1970) on Turkish and Local & Lodge (1996) on Kalenjin. It is interesting to note van der Hulst & de Weijer's proposal (509) that in a



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syllable-head approach harmonic features associate with syllable nodes. This could be a clear representation of the syllabic nature of many such features. However, van der Hulst & de Weijer go on to say that such features percolate only to the heads of these nodes, namely the vocalic root nodes. Again this is a missed opportunity: far from trying to single out vowels as opposed to consonants as segments affected by harmony, they should assume that the percolation applies to all syllable places. This is certainly the case with features like lip-rounding and ATR. In Turkish the whole syllable is either rounded or unrounded: the operation of the harmony rules means that all three have the same value in cases like [tytyndʒy] 'tobacconist' and [denizdʒi] 'sailor'. In Kalenjin, as I have already noted, the phonetic exponents of the ATR harmony system affect both consonants and vowels. Van der Hulst & de Weijer propose a perfectly good representation of the prosodic nature of harmony and then continue to discuss the phenomenon as something that affects vowels (as per the title of the chapter). The only kind of harmony that would not necessarily affect consonants would be one based exclusively on tongue height (see van der Hulst & de Weijer, 517–519). What is basically a very clear presentation of harmony systems turns out to be undermined by a flawed view of the phonetic exponency of phonological systems.

As a final comment on the issue of segmentation I would simply say that the many gains of autosegmental phonology in terms of an appreciation of syntagmatic features seem to have been thrown away by an insistence on segments; see, for instance, Clements & Hume's concern with segmenthood (257). This insistence on seeing things as segmental leads linguists to claim a higher status for some phenomena than is warranted. Take, for example, Clements & Hume's discussion of intrusive stop formation (272–273). Even though we are told that the possible oral stop phases in the codas of *dense* and *false* are shorter than those in *dents* and *faults*, with a supporting reference from Fourakis & Port (1986), the phenomenon of oral occlusive transitions is treated as the insertion of a segment. But this is a matter of timing the various articulatory movements involved, just like aspiration (= delay of voice onset time until after the contoid obstruction is removed), or cases of so-called deletion, as in the lack of velic closure in a sequence such as /-nd + C/ in English. In many instances of this last sequence the length of the articulation (= 3 consonants) is maintained, but the velum simply isn't raised, so the resultant realization is [-nnC]. It is, therefore, inappropriate to interpret lack of velic closure as segment deletion in all cases and it is equally inappropriate to interpret early velic closure and cessation of vocal cord vibration as segment insertion in all cases. To put the question in another form: why raise the timing of velic closure to a segmental level and not delayed or early voice onset/offset? Why is [t] more segment-like than [h]? All these matters are from the realm of phonetic implementation of the phonological structure, and implementation is variable (see Local (1992) for a discussion of timing variation in the context of speech synthesis).



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## 5. Declarative phonology and underspecification

One chapter reviews underspecification from a non-derivational standpoint (Steriade), but there is little reference to declarative approaches outside this chapter. Goldsmith, for example, refers to Kaye's work (1990) in a footnote (fn.10, 21) as a rule-less phonology, but makes no comparisons with derivations. For the most part there is an implicit assumption that phonological forms undergo some kind of derivational processing and there is one short contribution on rule ordering by Iverson (Chapter 19), which concludes 'the machinery [of derivation, KRL] available in present phonological theory appears to provide sufficient descriptive capacity without the stipulation of special constraints on rule ordering' (614). But one might reasonably ask why Iverson's contribution should be so short, when a thorough discussion of the issue would not be out of place. Coleman (1995) makes out a strong case for constraining grammatical theory by ruling out structure-destroying rules a priori. It would, therefore, have been stimulating, if the Handbook had included at least one comparative analysis of data in derivational and in non-derivational terms, similar to the discussion in Part 3 of Durand & Katamba (1995).

I have made a comparison of analyses of harmony in Kalenjin in Lodge (1995) and the following are the main points of my argument in favour of underspecification and non-derivational phonology. In the case of Kalenjin with a dominant recessive harmony system there are three types of morpheme:

(6) (a) dominant morphemes are always [+ATR]; any neighbouring adaptive morpheme(s) will take on this value;
(b) adaptive morphemes vary their [ATR] value according to the specification of their neighbouring morpheme(s);
(c) opaque morphemes are always [-ATR] and do not alter this feature, even next to dominant morphemes, and they stop the spread of [+ATR] to other adaptive morphemes beyond them.

The most straightforward way to deal with this is to have the adaptive morphemes unspecified for [ATR], a proposal made by Martin (1985) and by Ringen (1988). [+ATR] will always spread into unspecified slots, blocked only by the lexical specification of [-ATR] in the opaque morphemes, and the default specification is [-ATR]. The blocking is ensured, if delinking is ruled out from the start. In the Handbook no discussion of this issue is presented; in a derivational approach spreading can delink given specifications (see, for example, Goldsmith (18) and Steriade's discussion in Chapter 4), which means that delinking has to be licensed in a language-specific and even a phenomenon-specific way.

Steriade's contribution to the Handbook is interesting in that it presents a critique of underspecification in a derivational approach (e.g. Archangeli



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1984, 1988). She takes up Stanley's (1967) point about opportunistic underspecification, that is, its use for convenience rather than on the basis of any general principles. It is the search for these general principles which interests her. Her main concern is to show that what she calls temporarily underspecified segments are unnecessary, since they imply some kind of derivation involving the features or feature-values being filled in at a later stage to give the fully specified surface forms. She distinguishes between trivial and non-trivial underspecification (117). The former refers to cases of permanent underspecification (which I have discussed briefly above in the context of phonetic implementation in section 2), whereas the latter refers to cases usually dealt with derivationally, e.g. place assimilation and so-called vowel harmony. To avoid derivation she proposes language-specific licensing statements which filter out impossible feature arrays (158–165). Such statements stipulate the domain of the features involved, e.g. adjacent coda and onset, rhyme, word. Indirect licensing seems equivalent to spreading, especially if spreading is only allowed as a filling-in mechanism where no other specification obtains. In fact, her licensing statements seem to be the equivalent of the redundancy rules exemplified by (6) and (7), plus the spreading convention in cases where the domain is longer than one syllable slot.

(7) [$\alpha$son] $\rightarrow$ [$\alpha$voice]
(8) [ ] $\rightarrow$ [coronal]

In this sense 'rule' is to be interpreted not as a statement of a process but as a statement of an implication: X $\rightarrow$ Y means 'if X, then Y'. But Steriade does not appear to make the point that underspecification does not necessarily involve derivation. Certainly the combination of underspecification and some version of constraints-and-repair is unnecessary duplication; why have both structure-building and structure-changing rules in the same theory? (See Mohanan's (1991: 295) comments on underspecification) However, a combination of underspecification and non-derivational phonology is worth proper investigation (see Local & Lodge 1996).

## 6. Concluding remarks

I have taken this opportunity to elaborate on my main concerns in the field of phonological theory generally. In so doing I may have moved too far away from the Handbook at times, but if it is the intention of such a book to represent the state of the art, then wider issues are bound to be involved.

However, it must be stressed that I regard such books, perhaps somewhat more modest in scope than the Handbook, as essential in bringing together strands of argument both in a historical overview and a synthesis of ideas from slightly different starting points. The fact that I have not been able to



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refer to all the contributions in the course of this discussion does not mean that they are less important or less well presented than the ones I have mentioned. I have simply taken particular exemplifications of the issues I have concentrated on, some of which pervade all the contributions in the book.

Of those contributions I have had no time or space to refer to seven are on stress, tone and intonation (Kager, Halle & Idsardi, Odden, Yip, Selkirk, Newman, Harris) and ten, two of which I have referred to, are on specific topics in particular languages or language areas (Odden, Yip, Evans, Newman, Hudson, Tranel, Itô & Mester, Hoberman, Rubach, Harris). Others will have to devote their energies to these and other contributions to provide the proper commentary they deserve.

As a final comment I would like to reiterate a point I made earlier in section 2: one of the most significant contributions to the Handbook is Brentari's chapter on the phonology of American Sign Language. Further investigation of a non-phonetic phonology par excellence should give us further insights into the nature of phonological structure.

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