LOOKING BACK, MOVING FORWARD

Why underlying representations?[1]

LARRY M. HYMAN

University of California, Berkeley

Phonology is a rapidly changing and increasingly varied field, having traveled quite some distance from its original structuralist and generative underpinnings. In this overview I address the status of underlying representations (URs) in phonology, which have been rejected by a number of researchers working in different frameworks. After briefly discussing the current state of phonology, I survey the arguments in favor of vs. against URs, considering recent surface-oriented critiques and alternatives. I contrast three straightforward abstract tonal analyses against the potential arguments which accuse URs of being (i) wrong, (ii) redundant, (iii) indeterminate, (iv) insufficient, or (v) uninteresting. Identifying two distinct goals in linguistics which I refer to as determining 'what's in the head?' vs. 'what's in the language?', I suggest, responding to some rather strong opinions to the contrary, that URs are an indispensable and welcome tool offering important insights into the typology of phonological systems, if not beyond.

KEYWORDS: abstractness, alphabetic writing, goal of linguistics, representations, tone

> *Phonology is changing rapidly ... Some phonologists collect the evidence for their theories using introspection, fieldwork and descriptive grammars, while others trust only quantitatively robust experimentation or corpus data. Some test phonological theories computationally ... whereas others prefer to compare theories on conceptual grounds ... As the field grows and diversifies, it is becoming harder for phonologists to talk to each other, for who can be a computer scientist, phonetician, neurolinguist and expert in adjacent fields such as morphology and syntax at the same time as having a command of the extensive literature on phonology-internal argumentation and phonological typology?* (Gouskova 2013: 173)

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1. THE CURRENT STATE OF PHONOLOGY

As seen in the above quote, phonology has been undergoing considerable change over the past decade or two. The current state of the field can be characterized as diverse and disjointed with unclear boundaries and disparate goals and methodologies. Despite a healthy diversity of views and agendas, there has been an unmistakable trend for phonology to turn away from abstract representations and become largely oriented towards the surface. This can be attributed to the output-driven nature of Optimality Theory, as well as technology, which is better suited to investigate what speakers produce and listeners hear rather than traditional abstract representations. Thus much of the cutting-edge work in phonology tends to be experimental, instrumental, quantitative, statistical and computational.

Along with this development one finds an increasing rejection of the basic concepts and methodologies of the structuralist-generativist heritage, ultimately denying that phonology is anything like we used to think – if it exists at all. This can be seen in at least two ways: First, there has been increasing focus on the phonetic underpinnings of phonology. Second, there has been a decrease in interest in abstract underlying represents (URs) and the derivational approaches that support them. Up until recently traditional phonological theory has directed practitioners to ask: What are the underlying representations (URs) of individual morphemes? What are the rules that convert these URs to surface representations (SRs)? What kinds of constructs and mechanisms best capture the relation between URs and SRs, e.g. features, tiers, syllables, prosodic domains, lexical strata, cyclic vs. non-cyclic derivations, etc.? In this overview I will refer to this heritage as 'traditional phonology', whose basic tenets are summarized in (1).

(1) (a) Phonology = grammar (i.e. distinct from phonetic substance)
    (b) Structuralist commitment

        i.   two or three levels of representation (morphophonemic, phonemic, phonetic)
        ii. rules or constraints to relate these levels
        iii. discrete segments, distinctive features, and prosodic constituents (syllable, foot etc.)

    (c) Central role of 'contrast' (cf. the original phoneme concept)

Virtually all of the above has been questioned by someone, e.g. whether there is a distinction between phonetics and phonology, whether there are discrete consonant and vowel segments, whether there are underlying representations distinct from surface representations, etc. This is not a new issue, as there have been proposals in the past that phonological representations should be surface-based, e.g. in Natural Generative Phonology: 'I would like to suggest that the "pronunciation in isolation" form of a word is its lexical representation' (Vennemann 1974: 364). The surface-oriented models developed in subsequent years have been much more sophisticated and grounded in computation and/or language usage. Thus in Declarative Phonology, rather than a single abstract form, 'an underlying representation would be a set of distinct surface representations'

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(Scobbie, Coleman & Bird 1996: 697). 'In the exemplar model of representation, all perceived tokens of a word are categorized and stored with information about their contexts of occurrence' (Bybee 2001: 138). There are however still approaches to phonology which have not lost faith in abstract representations, e.g. continuing developments of Government Phonology such as Scheer (2004, 2012).

The central question I want to address in considering the recent positions different phonologists have been taking is whether we can still maintain the kind of robust structural patterning in phonology traditionally assumed in (1). The doubts have come from different quarters:

(i) *Are there discrete, productive phonological rules of the sort $A \rightarrow B/C$?*

A good starting point is Hayes (1995: 67–68), who notes that '[o]f the Ilokano rules [I] studied . . . either they seemed phonetic in character, so that my conventional phonetic transcription represented an over idealized categorization of continuous data, or they struck me as not fully productive, lexicalized rules. At the time I occasionally wondered, "Where is the normal phonology that I was trained to study?"'. He considers (ibid.) the possibility that '*all* phonology might ultimately be redistributed between the theory of phonetic rules and the theory of lexical organization . . . insofar as rules apply postlexically, they are phonetic and gradient, and insofar as they treat discrete categories, they are part of the lexicon rather than applying to the output of syntax'. He thus raises two questions: Are there productive phonological rules at the lexical (word) level? Are the productive rules at the postlexical (phrase) level PHONOLOGICAL?

(ii) *Are there phonemes?*

While justifying vs. questioning the 'psychological reality' of the phoneme has been much discussed from the very beginning of structuralist phonology (Sapir 1933, Twaddell 1935), both the question and doubts continue: 'the phoneme is not an entity on any level — functional, phonetic, psychological or even metaphorical. Rather, at best, "phoneme" is merely a terminological expedient' (Silverman 2006: 215).

(iii) *Are there morphophonemes ('underlying representations')?*

Similarly, the more abstract URs of generative phonology continue to be questioned: 'the notion of UR is neither conceptually necessary nor empirically supported, and should be dispensed with' (Burzio 1996: 118).

(iv) *Does (formal) phonology exist at all?*

The logical endpoint of all of the above doubting would be the claim that phonology itself is unreal, perhaps because the discrete elements that are assumed do not exist: 'There is no way to make an alphabet do the job of providing a phonological description of the lexicon of a language . . . There is no discrete universal phonetic inventory and thus phonology is not amenable to formal description' (Port & Leary 2005: 952).

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What the above comes down to is an assault from two directions, according to whether the proposal is to reassign phonological properties to the phonetics or to the morphology – thereby potentially squeezing phonology out of existence.

In this overview I am concerned with the issue of why we should accept abstract (non-surface) underlying representations as part of the study of phonology. Although recent handbook treatments acknowledge the changing views on the question of phonological representations, e.g. Harris (2007), Cole & Hualde (2011), Albright (2012), and Krämer (2012), the issue is far from settled. In the following sections I will discuss why URs were originally proposed (Section 2), then address the reasons why different scholars have increasingly rejected URs and other elements of traditional phonology (Section 3). In the course of the discussion I will present three rather clear cases where a structural analysis in terms of URs is both motivated and insightful. The final section (Section 4) argues that URs should be maintained as a/the central tool of phonological analysis.

2. WHY UNDERLYING REPRESENTATIONS?

In order to address the issue of underlying representations we need to consider the following questions: (i) What are URs? (ii) Why were URs originally proposed? (iii) Why do some linguists reject URs now? (iv) Why should we keep URs? I take up the first two questions in this section and the second two questions in Sections 3 and 4. First, however, I should point out that I will be avoiding English and instead present three tonal examples to make my points. The reason for avoiding English is that it has very little straightforward and exceptionless categorical phonology of the type that is most relevant in the current context. Supporting Hayes' (1995) observations cited above, it is easy to object to one or another proposal of *The Sound Pattern of English* (Chomsky & Halle 1968):

(i) In many cases, related roots or words might reasonably be argued to be listed as allomorphs, rather than being derived by phonological rules. One such example is velar softening (Chomsky & Halle 1968: 219), whereby $k \rightarrow s/\_\_ + i$, e.g. *critic* [k] vs. *critic-ism*, *critic-ize* [s] (the latter from /kritik-īz/). Major problems are that the triggers and targets of velar softening are restricted to certain morphemes, and a one-step change of /k/ to [s] is 'of questionable phonetic/phonological plausibility' (Scheer 2015: 317). Whether adopting what Kenstowicz & Kisseberth (1979: 180) refer to as 'the morpheme-alternant theory' or other mechanisms, morpheme-specific processes are not the best examples to cite in favor of phonological URs: 'any process commonly held to be "morphophonological" is actually part of the morphology and is to be analyzed in terms of language-specific morphological constraints' (Green 2007: viii).

(ii) In other cases the issue arises whether speakers 'know' that the words showing alleged alternations are related, e.g. *particle* and *particular*, *extreme* and

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extremity, the last in the sense of a body part (Ohala & Ohala 1986, Ohala 1987).

(iii) There are many gradient and variable processes that look more phonetic than phonological, e.g. timing of gestures in homorganic nasal assimilation across words: *phone* [m] *book*, which incompletely neutralizes with *foam book* (see Hayes 1995: 62–63 and Silverman 2006: 13–18 for discussion and further references).

While many segmental changes across words are undoubtedly phonetically gradient, a number of recent phonetic studies have argued that there are segmental phrasal assimilations which can be categorical, although typically with individual variation (see e.g. Ellis & Hardcastle 2002, Ladd & Scobbie 2003, Kochetov & Poulier 2008, and Celata et al. 2013, among others). On the other hand, tone offers a wide range of unambiguous and productive categorical phonological processes occurring across words which argue for abstract URs. The three short illustrations I will provide all involve a contrast between H(igh) and L(ow) tone heights which undergo completely productive alternations when words are concatenated at the phrase level.

2.1 *What are URs?*

With this established, we turn to the first question mentioned above: What are URs? In short, these are the representations one arrives at following the morphophonemic principle:

(2) *The morphophonemic principle*

One underlying representation per morpheme. I.e. one should derive allomorphs from the same UR, wherever possible and 'motivated'.

A common example taught in phonology classes concerns the English plural suffix, which has three allomorphs: *cat*[s], *dog*[z], *bush*[ɨz]. By the morphophonemic principle one would likely propose an underlying /-z/ suffix and a phonological rule /z/ → [s], [z], or [ɨz], depending on the final sound of the base to which it is added. However, an alternative is available to list the three output allomorphs, each with an indication of the class of sounds after which it occurs. While this is workable, an allomorphy approach is much less appealing when the phonological rules are both general and apply across words.

This is the case in my first tonal example from Hakha Lai, a largely monosyllabic Tibeto–Burman language spoken in Myanmar and NE India. The examples in (3) illustrate the three underlying tones /L/, /HL/ and /LH/ as they are realized contrastively after the toneless pronominal proclitic *ka* 'my' (Hyman & VanBik 2004):

(3) (a) L tone: (ka) ràŋ [_] '(my) horse'
    (b) HL (falling) tone: (ka) râal [⌐\ ] '(my) enemy'
    (c) LH (rising) tone: (ka) kǒoy [_/] '(my) friend'

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As schematized and illustrated in (4), there are important tonal alternations in Hakha Lai:

(4) (a) L + HL (ka) /ràŋ/ + /râal/ → ràŋ ràal '(my) horse's enemy'
        ↓   L   HL   L <u>L</u>
        L

    (b) LH + L (ka) /kǒoy/ + /ràŋ/ → kòoy ràŋ '(my) friend's horse'
        ↓   LH   L   <u>L</u>   L
        L

As seen, both HL and LH alternate with L, merging with /L/: Falling /HL/ becomes L after L tone, while rising /LH/ becomes L before L. An analysis positing /HL/ and /LH/ and rules which change them to L is clearly 'motivated': (i) All /HL/ and /LH/ words undergo these rules – there are no known exceptions. (ii) The rules productively apply to novel combinations of words – thus, word combinations reflecting the tone changes cannot be said to be lexically stored. (iii) The rules operate as a 'conspiracy' responding to the same constraint NOJUMP: 'Do not change pitch level between syllables'. Thus, heterosyllabic sequences such as *L.HL and *LH.L are ungrammatical, while LH.HL and HL.LH satisfy the constraint and do not change. (The NOJUMP constraint is also responsible for a third tone rule: /LH/ → HL/LH ____.) (iv) These are not phonetic rules, i.e. not rules of phonetic interpretation due to the timing of H and L gestures, rather to the categorical deletion of a H (and tonal metathesis in the LH → HL case) in order to minimize the ups and downs. (v) If this were allomorphy, every HL and LH word would have to be stored with a L alternate, which is highly uneconomical. Finally, note in (5) that the /LH/ rising tone is realized HL after pause, as underlined in (5).

(5) | | UNDERLYING | AFTER *KA* 'MY' (which is toneless) | | AFTER PAUSE (citation form) | |
|---|---|---|---|---|---|
| (a) | /L/ | ka ràŋ | 'my horse' | ràŋ | 'horse' |
| (b) | /HL/ | ka râal | 'my enemy' | râal | 'enemy' |
| (c) | /LH/ | ka kǒoy | 'my friend' | <u>kôoy</u> | 'friend' |

For this reason, forms were cited with the preceding (phonologically toneless) proclitic *ka* 'my' in (3) and (4) above. Generalizing on the fact that Hakha Lai tone rules are in general motivated by the NOJUMP constraint, Hyman & VanBik (2004) posit an initial %H boundary tone which results in the change of /LH/ to HL after pause. While this would violate Vennemann's (1974) notion of an UR as the 'pronunciation in isolation', it would not be a problem for Albright's (2008: 164) 'single surface base hypothesis', where the UR is the surface form from which other alternating allomorphs can best be predicted. It is clear that HL can be predicted from LH, but not vice versa. See Hyman & VanBik (2004) for

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further discussion. In the next section an example is discussed where abstract URs are required which diverge more dramatically from surface realizations.

2.2 *Why were URs originally proposed?*

In the above example we see one of the values in positing single URs from which surface allomorphs are derived, namely to arrive at an 'elegant' and 'economical' solution. In fact there were originally two goals in positing URs in early generative phonology:

(6) (a) To capture generalizations, i.e. 'what's in the language'.

  (b) To capture the speakers' knowledge, i.e. 'what's in the head'.

The first goal is what we have just seen in the Hakha Lai analysis. The second is to capture the knowledge of native speakers, in this case what Hakha Lai speakers know about their tones. As indicated in (6), I have paraphrased these two goals as determining 'what's in the language' vs. 'what's in the head', i.e. the minds/brains of speakers. In early generative phonology the assumption was often made that the two were the same, based on the Chomskyan assumption that the most simple and general analysis is the one that will be constructed in language acquisition. However, in pursuing these goals, phonologists have to determine which generalizations speakers 'know' as well as how to model the knowledge we think they have. This in turn raises the question of how different URs can be from their surface realizations, hence the abstractness debate of the late 1960s and early 1970s. Fortunately tone can help us out again.

  The following example illustrates what speakers have to know when the relation between URs and their surface realizations is quite remote. In Giryama, a Mijikenda Bantu language of Kenya, the underlying rightmost /H/ is realized on the penultimate mora of the phonological phrase (Volk 2011: 17):

(7) ALL L TONE H TONE ON PENULTIMATE MORA

  'I want...' 'he/she wants...'
  ni-na-maal-a a-na-maál-a
  ni-na-mal-a ku-guul-a a-na-mal-a ku-guúl-a '...to buy'
  ni-na-mal-a ku-gul-a ŋguuwo a-na-mal-a ku-gul-a ŋguúwo '...to buy clothes'

  H

In these examples, L tone is unmarked, while phrase-penultimate lengthening is indicated by doubling the vowel. As seen, the forms on the left consist of words and phrases that are all L tone (assigned by default). The forms on the right, on the other hand, have a H tone on the phrase-penultimate mora. It is quite clear that the only difference between the two sets of forms is the identity of the subject prefix: *ni-* 'I' vs. *a-* 'he, she'. The penultimate H in the forms on the right therefore can only be attributed to a special property of *a-*. We therefore set up this morpheme

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as /á-/, i.e. with an underlying /H/, which shifts to the penult. (An alternative is that the H shifts to the final vowel and then is attracted back onto the lengthened penult – see Volk 2007, 2011 for discussion.)

It is important to underscore that this is phonology, not phrasal 'pitch-accent' or intonation (a third person singular subject intonation would be quite odd). One argument is that there can be more than one /H/ per word or phrase. In (8a) the underlying /H/ of the lexical verb /-gúmbuhizik-/ 'be wiped out by utter destruction' shifts to the penult as expected (Volk 2007: 17):

(8) (a) ni-na-gumbuhiziík-a 'I am wiped out by utter destruction'

        H
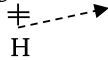
   (b) a-ná-gumbuhiziík-a 's∕he is wiped out by utter destruction'

        H   H
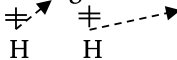
In (8b), where there are two underlying /H/ tones, the /H/ of the lexical verb again shifts to the penult, while the /H/ of /á-/ shifts one position onto the tense marker *-na-*. Finally, in Riβe$_2$, another Mijikenda dialect/language, a single underlying /H/ can have several non-contiguous surface H outputs if there are intervening (non-prenasalized) voiced obstruent 'depressor consonants', each one requiring a H before it (Volk 2011: 54; see Cassimjee & Kisseberth 1992 for a spreading + delinking analysis):

(9) 'I am beating drums'   's∕he is beating drums'

    ni-na-pig-a ma-bumbuumbu a-na-píg-a má-bumbuúmbu

                                 H
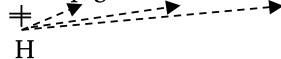
In this case as well, speakers 'know' that the multiple surface Hs owe their existence to the subject marker /á-/, not just the penultimate H which corresponds to mutually intelligible Giryama.

Although the Mijikenda long-distance displacement of the H tone is rather dramatic, the UR analysis with /á-/ is both general and simple. How, then, would it be captured in a framework that insisted on allomorphy? One would presumably have to recognize two classes of morphemes, those like *a-*, which assign a penultimate H tone, vs. those like *ni-*, which do not. (In Riβe$_2$ such morphemes would also assign H tones to any mora preceding a depressor consonant.) Presumably one would not want to list a H tone allomorph of every morpheme that could conceivably be realized phrase-penultimately. Given this, it is hard to see any conceptual or empirical difference between /á-/ and a more surfacey /a-/ with a procedural instruction 'assign a H to the phrase-penultimate mora'. Finally, I cannot fail to note that no other phonological feature or property has this ability to 'wander' long-distance across words. Tone is particularly clear about this. As I like to point out, 'anyone who is interested in the outer limits of what is possible

in phonology would thus be well-served to understand how tone systems work' (Hyman 2011: 198).

3. WHY NOT UNDERLYING REPRESENTATIONS?

So, if URs do such nice work for us, why do some linguists reject URs now? I have identified five potential reasons to which I respond in the subsections to follow: (i) URs are wrong; (ii) URs are redundant; (iii) URs are indeterminate; (iv) URs are insufficient; (v) URs are uninteresting. I now take up each of these in turn.

3.1 *URs are wrong*

There are at least three types of arguments that URs are wrong: (i) URs are not real; (ii) URs assume non-existent units; (iii) URs require a commitment to innateness and Universal Grammar (UG). The form that the first argument usually takes is that URs are not 'psychologically real' (see e.g. Eddington 1996): speakers do not 'know' them, that's not the way speakers store, access, produce or perceive lexical entries. To some who hold this view, speakers only know what they hear or produce, and do not have mental representations that depart from this. Some of this sentiment has been in reaction to excesses in positing abstract forms in early generative phonology, but as we have already seen (and will return to below), certain frameworks reject any two-level theory.

More potentially problematic are the claims that URs are wrong because they assume units that 'don't exist'. A chorus of linguists have advanced the claim that consonant and vowel segments do not exist, only the continuous speech stream created by overlapping features or gestures which may be organized into higher level prosodic constituents. Thus, Port & Leary (2005: 950) state: 'A small inventory of segment-sized, graphically represented phonological categories can provide a practical scheme for representing most languages on paper. But what is in speakers' heads is apparently not symbols analogous to graphical letters'. To some of these critics, consonant and vowel segments are only artefacts of alphabetic writing: 'Why then does the notion of the segment persist? . . . The most obvious answer seems to be that linguists by their very training are literate, usually in some form of alphabetic writing' (Lodge 2009: 12); 'it is only after alphabetic writing is learned that notions of segmentation . . . may develop (Lodge 2009: 43); 'IPA symbols . . . are mere visual expedients' (Silverman 2012: 6); '"phonemism" and "alphabetism" . . . may be mercifully deposited on to the junk heap of theoretic history' (Silverman 2012: 187). As seen, the claim is that linguists cling to the reality of consonant and vowel segments only because of the invention of alphabetic writing. Colleagues I have consulted who work in preliterate societies have universally found this claim rather puzzling, citing (admittedly anecdotal) evidence that speakers are aware of segments. The anti-segment position of course goes beyond rejecting URs, as it would also dismiss phonetic representations in terms of segments, in fact, the whole enterprise

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of the IPA as well as the long-standing practice of representing contrasts in segmental/alphabetic terms: 'it is necessary to have an alphabet which indicates only those broader distinctions of sound which actually correspond to distinctions of meaning in language' (Sweet 1877: 103).

Phonological counterarguments in favor of the segment come from alliteration, infixation and metathesis, the last occurring in adult language, language acquisition, borrowings, and speech errors (Harris 2007: 122; Buckley 2011: 1402; Eliasson 2014: 1313–1314; Ladd 2014: 22–23, among others): 'The need to refer to discrete segments even to characterize metathesis . . . presents particularly good evidence against suggestions that segments have no psychological reality, and are a mere artifact of an alphabetic writing system' (Buckley 2011: 1402). To this we can add the morphological evidence: Lots of languages with and without alphabets have affixes that consist of a single consonant or vowel, e.g. English *a-moral*, *consonant*-s. Why does this not count as evidence that speakers can manipulate single segments? As mentioned, much of the opposition derives from difficulties in segmenting the continuous speech signal. A particularly compelling response against this argument is presented by Ladd (2014). After enumerating a number of parallels between alphabetic scripts and phonemic analysis, Ladd points out that it is often difficult to determine where one handwritten letter ends and the next begins. However, it would be wrong to conclude from this that there isn't an input system of discrete units underlying the handwriting. He thus concludes: 'the difficulty of segmenting the speech signal is not, by itself, evidence against a phonological description based on categorically distinct segments' (Ladd 2014: 23).

Another way in which URs may be thought to be wrong is that URs require a greater role of UG in language acquisition vs. bottom–up 'emergent grammar': 'we will argue against the postulation of "a single underlying representation per morpheme", arguing instead for the postulation of a set of interconnected surface-based representations. We propose a surface-oriented model, building on the core idea that significant portions of a grammar are "emergent", that a phonological grammar should depend as little as possible on innate properties of a "Universal Grammar"' (Archangeli & Pulleyblank 2015a). As these authors indicate, this third argument also derives from the surface-oriented trend in current research agendas in phonology (see Archangeli & Pulleyblank 2015b and the response by Jones 2016). Whether URs can or cannot also 'emerge', traditional phonologists who do UR analysis are not necessarily actively involved, if at all committed to the UG enterprise.

3.2 *URs are redundant*

The second potential argument against URs is that they are redundant, hence can be dispensed with via Occam's Razor. The idea here is that whatever work URs do for us can be replaced by mechanisms that are independently needed. On the

morphological side, we clearly recognize bona fide cases of allomorphy which require the listing of allomorphs:

(10) (a) English *a/an*: deriving [ʌ] and [e] from /æn/ would be isolated, hence unmotivated.

(b) Korean nominative case markers bear no phonetic resemblance: *-ka* after V, *-i* after C.

(c) French adjective alternants before a vowel-initial noun, e.g. *vieu*x [vjø] vs. *vieil* [vjɛj] 'old': *un vieil ami* 'an old friend'; see Bermúdez-Otero (2014) and references cited therein.

We also obviously need phonetic implementation to account for the surface forms of words, e.g. timing of gestures (recall the [m] in *phone book*). So, the argument goes, we can do everything with allomorphy and phonetic implementation.

At the time of this writing, reanalyses in terms of allomorphs have addressed only a fraction of the complexity that we find in whole languages. However, even if allomorphy is adopted instead of single URs, this will not be enough: diacritics will have to be proliferated. To illustrate this, consider the following two representative nouns from Aghem, a Grassfields Bantu language of Cameroon, which consist of a H noun class prefix *kɨ-* and a H stem (Hyman 1979: 17–18):

(11) (a) kɨ-fé   'leg'   (b) kɨ-wó   'hand'
          H H   [¯ ¯]   H   H   [¯ ¯]

As indicated, the two nouns are pronounced identically as H-H in isolation. However, in context, their tonal properties differ. In the following examples, the prefix *kɨ-* is deleted when these nouns occur before the /H/ tone demonstrative *kɨn* 'this' (as before most other modifiers):

(12) (a) fé   kɨn   'this leg'   (b) wó ↓ kɨn 'this hand'
          H   H   [¯ ¯]   H L̲ H   [¯ ¯]

As seen in (12a), nothing happens in 'this leg', which is realized H-H, but in 'this hand' in (12b), *kɨn* lowers in pitch ('downsteps') to produce H-↓H. As seen, I have posited an underlying lexical floating L tone on the root /-wó `/ to condition the downstep.

Now consider the realization of the same two nouns before the /L/ tone possessive *kɨa* 'your.SG' (which also conditions prefix deletion on the noun):

(13) (a) fé   kîa   'your.SG leg'   (b) wó   kɨa   'your.SG hand'
          ∟-˙-▼| | |
          H   L   [¯ ⌐]   H L̲ L   [¯ _]

In (13a) the H of 'leg' spreads onto the possessive pronoun to produce H-HL in 'your.SG leg'. In (13b) nothing happens in 'your.SG hand', which is realized H-L. The SAME lexical floating L tone needed to produce the downstep in (12b)

blocks H tone spreading onto a following L in (13b). That is, positing distinct URs for 'leg' and 'hand' allows us to account for their differential behavior in a straightforward way. This analysis receives indirect reassurance from diachrony, as the difference derives from a tonal contrast on a second, historically lost syllable. Thus, Aghem /-fé/ is cognate with Proto-Bantu *-píndí 'leg', while /-wó `/ is cognate with Proto-Bantu *-bókò 'hand'. But is the UR analysis the best synchronic account?

At this point we need to ask what the alternative is to positing a floating L in /-wó `/ 'hand' as in (14a) below? The only viable alternative appears to be diacritics, recognizing either two kinds of H tone, as in (14b), or two kinds of H tone morphemes, as in (14c):

(14)   (a)   Floating tone analysis: /-fe/   vs.   /-wo/
                                              H   H L

       (b)   Two kinds of H tones: /-fe/   vs.   /-wo/
                                             H_1 H_2

       (c)   Two kinds of morphemes: $/-fe/_1$ vs. $/-wo/_2$
                                             H   H

However, the diacritics don't by themselves resolve the allomorph issue: Does *kɨ́n* become *↓kɨ́n* and *kɨ̀a* become *kɨ̂a* by rule (or Input/Output constraint ranking) or do all such words trigger H vs. ↓H and L vs. HL listed allomorphy on the following word?

Traditional phonology would argue that URs with floating tones are superior to diacritics (Hyman 2003: 160–161): (i) Floating tones capture our intuitions: tones are expected to be affected by other tones, not by diacritics (which – as elsewhere – are normally used only as a last resort). (ii) Floating tones capture generalizations, in this case the clustering of two properties: the floating L downsteps a following H and blocks the preceding H from spreading onto a following L, both of which are phonetically natural and expected. (iii) Floating tones are more constrained or restrictive: the diacritics could have conditioned an 'unnatural' mixed system whereby H_1 'leg' would both downstep a following H and spread onto a following low while H_2 'hand' would do neither. (iv) Floating tones raise interesting questions for research: What can a floating tone do? Not do? How is a floating tone different from a linked tone? How many floating tones can you get in a row? (Answer: At least three! (Hyman 2004: 28).) What else can float? (See especially Rubach 2016 on the superiority of floating autosegments.) The Aghem floating tone analysis has a number of advantages. However, it may be asked what such URs commit us to psycholinguistically. Do speakers 'have' floating tones? One can reasonably argue that the only facts are the alternations, not the abstract representations. They know that there are two kinds of H tones, so they have to lexicalize which H tones belong to one vs. the other class, H_1 vs. H_2. (There actually is a third class of Hs analyzed with stem /LH/ – see Hyman 1979: 17.) However, the diacritic approach is not attractive if our aim is to figure out what is possible in phonology: diacritics can do anything; a list can list anything.

It is hard not to conclude that the floating L analysis is the best way to capture the two kinds of H tone behavior in Aghem (and, in fact, many other tone systems).

3.3 *URs are indeterminate*

The third potential argument against URs is that they are sometimes indeterminate or pose analytical problems. While URs should allow us to express generalizations, they should not force us to make arbitrary decisions which may not even be resolvable, e.g. concerning non-alternating sounds. A frequently mentioned example concerns tautomorphemic flapping in American English: Is the underlying consonant of words such as *matter* [mæɾɹ] and *ladder* [læɾɹ] /t/ or /d/? (See Nevins & Vaux 2007: 55–56 for discussion of the problem.) The same problem arises concerning sounds which alternate in non-productive morphology, e.g. *metal* and *medal*, both of which are pronounced [mɛɾl̩]. Since they are related to *metalic* and *medallion*, with [tʰ] and [d], respectively, should one [mɛɾl] be set up with /t/ and the other with /d/? Such a move is of questionable motivation, as it is hard to find corresponding suffixed forms of *l*-final nouns from which one can generalize (*nettle*, **nettalic*; *pedal*, **pedallion*). More abstractly, Chomsky & Halle (1968: 546) had proposed /re=sɪgn/ as the UR of *resign* [rəzajn] to capture its relation to *resignation* [rɛzɪgneʃən] with the ad hoc = boundary suggesting a possible relation to *sign* [sajn], *signal* [sɪgnəl]. As part of the abstractness debate of the period, the question posed by Lightner (1971: 540) in setting up URs was where to stop? On the other hand, if URs are abandoned, neither problem would arise: Sounds which do not alternate would be entered with a representation reflecting their surface realization, e.g. with flaps; relations such as *resign/resignation* could be captured by allomorphy. Of course this does not detract from the fact that many flaps are derived from an underlying stop, e.g. [gɛr ʌp] 'get up!'.

3.4 *URs are insufficient*

The fourth argument can be summed as follows: Since URs can't do everything, they must be wrong. We have already seen that a single UR is not set up for suppletive allomorphy, e.g. Korean nominative -*ka* vs. -*i*. However, the main thrust of the insufficiency argument comes from another direction: Since URs do not tell us every detail about what speakers store about a lexical item, they are insufficient – and thus should be rejected in favor of a listing of these details: 'speakers can record in memory and control in their production far more detail than traditional linguistics suppose' (Port & Leary 2005: 953). URs do not encode whether a word is frequent or rare, and yet speakers have such knowledge, as well as memories of where they might have first learned the word, or heard it repeated. Perhaps worse, words claimed to have the same UR can show subtle phonetic differences, as in the case of allegedly homophonous *time* and *thyme* (Gahl 2008). However, this argument has its limitations. Cutler et al. (2010: 106) argue that 'listeners

need abstract prelexical representations of speech sounds in order to deal with variation in the speech signal'. As I will discuss in Section 4, the insufficiency argument carries weight only to the extent that alternative approaches such as exemplar theory can do everything that URs can do.

3.5 *URs are uninteresting*

This last argument is somewhat different from the others in that it represents a value statement rather than a position that can be evaluated empirically. Although I haven't heard it put this way (and I have encouraged colleagues to say this, just so I can respond), perhaps the field has learned everything it can learn from past work on URs and therefore 'needs' to turn to other questions, data, and methodologies. Perhaps traditional phonology is a victim of its own success. I would argue that we understand the issues involved in structural phonology better than most other subfields. In fact, I think it could be argued that investigation of more phonological systems will yield fewer surprises (e.g. phenomena we have not seen before) than, say, phonetics, morphology, syntax or semantics. Thus, for phonology to uncover new facts one would have to change our way of thinking, work more with large corpora or in laboratory settings. In other words, if we continue to do traditional phonology we may find ourselves mostly replicating what we already understand. However, the Mijikenda phenomena in (7)–(9) above suggest that we have not yet seen everything that tone can do – and tone provides rather clear-cut motivation for setting up rather abstract URs. Thus, although some phonologists abandon URs in favor of allomorphy and/or phonetic continua, perhaps legislating discrete phonology out of existence, I will now argue that URs should remain an important tool in phonological analysis.

4. WHY KEEP URs?

In the preceding sections I have presented five reasons why some colleagues have partially or wholly given up on URs. This raises the question: Why should we keep URs at all? Recall from (6) above that URs are designed to do two things: (i) capture generalizations (i.e. 'what's in the language'); (ii) capture the speakers' knowledge (i.e. 'what's in the head'). Re capturing generalizations, it may be instructive to return to the three tonal analyses I presented in Sections 2 and 3. In my view, the Hakha Lai, Giryama, and Aghem tonal URs have the qualities one would want URs to have. They are:

(15) (a) Simple: The URs are not complicated or unnecessarily abstract.
(b) Efficient: The URs describe the phenomena in parsimonious terms.
(c) Restricted: The URs do no more than what can be expected of them to do.
(d) Motivated: The URs were posited to capture productive alternations across words.

Although not terribly 'abstract', in each case I proposed an UR that was different from the way the form would be realized in isolation:

(16) (a) Hakha Lai: /LH/ words are pronounced [HL]
                       /kǒoy/ → kôoy 'friend'

      (b) Giryama: /H/ shifts to penultimate position
                       /á-na-mal-a/ → a-na-maála 's/he wants'

      (c) Aghem: Floating /L/ is not pronounced
                       /kɨ́-wó `/ → kɨ́-wó 'hand'

The above URs have been posited to capture generalizations in the language, hence no problem there. The controversy is over whether URs are 'psychologically real', i.e. whether the mechanisms best suited to capturing generalizations are valid in characterizing speaker knowledge, however incomplete. What if it can be confidently demonstrated that speakers don't 'know' the claimed structural generalizations? One might either seek a local fix, to arrive at a better analysis (which speakers do 'know'), or a global fix – throw out traditional phonology – if not also systematic phonetics and the IPA.

While one's response is often presented as a matter of right vs. wrong, some of the controversy derives from one's specific interests, e.g. accounting for morphophonemics vs. continuous phonetic output, or more globally from how one sees one's work fitting into the larger scheme of things. Although linguistics or 'the scientific study of language' has often been claimed to have an overarching 'goal', expressions of that goal have been quite varied. Some see the goal as characterizing languages: 'the goal of linguistics is . . . to explain why languages have the properties they do' (Evans & Levinson 2010: 2740); 'In its broadest interpretation, the goal of linguistics is to discover how human languages are alike and how they differ, and to propose and test theories that explain the similarities and differences' (Bybee, Perkins & Pagliuca 1994: 1). Others see the goal as formulating linguistically significant generalizations: 'The goal of linguistics is to formulate the most elegant hypotheses about how language works, consistent with the data' (Newmeyer 1983: 41). Finally, linguistics is often seen in support of cognitive goals: 'The central object of inquiry in linguistics . . . is the nature and structure of the cognitive faculty that supports Language. This is by no means all that linguists do, and I do not mean to denigrate the study of ways Language is used, the role of Language in society, and other pursuits. I do want to claim, though, that the central task for a "scientific study of language" is to arrive at an understanding of an aspect of human cognitive organization. It is this that, like it or not, makes cognitive scientists of us all' (Anderson 2008: 796).

Not only do linguists see 'the goal of linguistics' differently, but they also often feel that their goal is underrepresented. This is particularly visible in Hornstein's Lament (Hornstein 2014): 'most linguists take their object of study to be language not the faculty of language. Sophisticates take the goal of linguistics to be the discovery of grammatical patterns. This contrasts with the view that the goal of

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linguistics is to uncover the basic architecture of FL [the faculty of language].
I have previously dubbed the first group *languists* and the second *linguists* . . . The
description of different languages is not a goal in itself. It is valuable precisely
to the degree that it sheds light on novel mechanisms and organizing principles
of FL'.

It is clear that many, including myself, would disagree with this assessment
and the view that the cognitive enterprise defines the exclusive goal or motivator
of the vastly different kinds of work that we do in phonology and elsewhere
in linguistics. Recall from (6) the two reasons for setting up URs: to capture
generalizations ('what's in the language') and to capture the speaker's knowledge
('what's in the head'). Particularly in light of Hornstein's dichotomy, one might
ask whether linguistics is one vs. two enterprises: heads vs. languages? It is
not that one is superior to another, rather that there are two different issues to
unravel: the faculty of language in the above Anderson and Hornstein quotes vs.
synchronic, diachronic and typological properties of languages, which Hurford
(1977: 580) refers to as the origins of language: 'Every linguist, save only the
most absurd game-playing hocus-pocusist, is concerned at every stage and level
of his work with hypotheses. Most linguists do not describe languages just for
the fun of doing so. Even though some linguists are publicly much more cautious
than others about the purpose of their enterprise, hardly any linguist can doubt
that, at least in some small way, he is contributing to a wider study, either of
the peculiar genius of the human mind, as language reveals it, or of the origins
of languages'. Even there, however, I have to say that many, including myself,
do consider describing languages as 'fun'. However, even if one is interested in
describing only one language as a specialist, say, of English, Spanish or Japanese,
it is hard not to get involved in issues of typology, history, and Anderson's 'how
Language is used'.

Port & Leary (2005: 959) draw an apparently different contrast between 'lin-
guistic behavior' and 'linguistic descriptions', which they see in actual conflict:
'there is no assurance that a coherent static description of knowledge exists just
because that is what one wants to study. There is a risk that, for methodological
purposes, this mission may be implemented as: We care about how to write down
a description of a language. . . . If it is linguistic behavior that we want to account
for, then we must let go of the requirement that we also be able to write our
linguistic descriptions down'. Of course there is more to linguistic behavior at the
phonological level than phonetic behavior – phonology also has an interface with
morphology and syntax. The question for this section is: Where do URs reside?
In languages? In linguistic descriptions? In the heads of speakers?

What is important in this connection is to recognize that one's personal goal
may not be the only possible agenda for the field, in this case phonology. I have
sometimes felt that there is a recurrent confusion between research agenda and
'truth': It is often claimed that linguistics (and within it phonology) is a branch
of cognitive science, i.e. about 'heads': 'The goal of phonological . . . theory, as
a branch of cognitive science, is to categorize what is a computationally possible

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phonology, given the computational nature of the phonological component of UG'
(Hale & Reiss 2008: 171). However, language has crucial interfaces with culture,
social interaction, history, contact, population movements, etc., not to mention
the current preoccupation of our field with the documentation of endangered
languages. In all of these endeavors we still recognize each other. Linguistics
boasts of a number of concepts and methodologies in which only linguists partake
– and which continue to produce rich insights into the nature of language through
theoretical, typological, descriptive and historical investigation. URs fall into this
category. As I like to put it, anyone can measure or count, but only a linguist
can do a morphophonemic analysis. Whether interested in languages or minds,
phonological analysis with URs is a TOOL much like other tools that are available,
e.g. instrumental investigation, experiments, or corpora (Scheer 2014). Whether
one is interested in establishing the structure of a language or the generalizations
speakers establish in their heads, setting up URs is typically something that is
done before anything else – which is why all recent phonology textbooks are
essentially retro in their insistence that students understand the relation between
input and output and how to get from one to the other. However, the goals of such
phonological analysis should not be confused with what it is not designed to do –
URs are not a way to talk about phonetic implementation and speakers' memories
of outputs, which is what some of the above critics are instead concerned with.
(For discussion on determining the relevance of 'external evidence' for linguistic
claims, see Zwicky 1986.) Thus, contrasting with Port & Leary's (2005) stance
'against formal phonology', we can cite Ladd's more measured position: 'while
systematic phonetics is of doubtful validity as the theoretical basis for describing
utterance phonetics, it may be useful and important as a theory of phonetic
typology' (Ladd 2014: 49).

To this we can add that more abstract UR analyses are also valuable in the
area of typology, i.e. in characterizing how phonological systems are the same vs.
different. Viewed this way, URs are a tool for characterizing structural properties
of a language – and a useful tool at that: 'the categories traditionally applied to
the description of phonological representation . . . still have an important heuristic
value as descriptors to be used in the building and experimental testing of models
of phonological grammar' (Harris 2007: 137). They are useful in the same sense
as other constructs in linguistics such as syllable, morpheme, noun, verb, phrase,
sentence, and so forth, all of which reveal some squishiness and possible overlap
when examining linguistic diversity and actual language use. While it has not
been my concern to justify the psychological reality of abstract URs, from the
languages I have worked on I wonder if it is really possible to appreciate the
phonology in speakers' heads without first doing a morphophonemic analysis? Of
course the final word will depend on a careful comparison of what URs can do that
alternatives cannot, and vice versa. As indicated in the Gouskova opening quote
and first paragraph above, phonology has been expanding out to ever broader
coverage via a wide range of methodologies. My own view is that it would be
premature, if not folly to give up on URs.

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Author's address: Department of Linguistics, University of California,
Berkeley, CA 94720-2650, USA
hyman@berkeley.edu

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