of atoms, structures or derivations, there are strong arguments (both methodological and empirical) for defending the idea that the various modules making up the language faculty mirror one another in fundamental ways. The precise nature of the mirroring depends on which version of linguistic theory one subscribes to, but, as far as the derivational issue is concerned, a position advocating that phonology should have transformational power in the classical sense seems quite unwarranted.

## Acknowledgements

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**Chapter 9**

# Derivations and interfaces

*Jonathan Kaye*



## Introduction

In this chapter I will discuss the notion of derivation within the framework of *Government Phonology* (GP).[1] I will consider the lexical representation of phonological strings, how they are treated by the phonology, and their relationship to the speech signal. Such considerations inevitably lead us to the study of interfaces. I will present a proposal, first formulated by Jean-Roger Vergnaud and me (Kaye and Vergnaud (1990)), for a phonological-lexical interface. I will then argue that there is no phonological-phonetic interface for the simple reason that there is no linguistically significant level of phonetics as distinct from phonology.

A number of different proposals concerning the nature of phonological derivations will be considered. In particular, there is the mainstream view as found in Bromberger and Halle (1989) (see Durand this volume, Coleman this volume). They argue for language-specific rule ordering as a property of phonological systems. It has also been claimed by proponents of one or another version of *lexical phonology* that phonological processes may apply at different morphological strata. Once again, where a process applies within a stratum with respect to other processes and to what stratum (or strata) a process is assigned, is a language-specific matter. There are also certain claims made by proponents of the notational system or 'framework' known as *constraint-based phonology or declarative phonology*.[2] I will briefly show that these claims are largely irrelevant to our discussion and contain little if any empirical content.

I will be arguing for a reasonably simple view of phonological derivations. My view can be summed up by the minimalist

hypothesis formulated in Kaye (1992: 141), 'Processes apply whenever the conditions that trigger them are satisfied.' GP does not allow for *phonological* processes applying at different levels of a derivation. Derivations are assumed to be 'blind' in the sense that no process is aware of the history nor the future of any derivation in which it is involved. The view expressed here is to be distinguished from the Bromberger and Halle (1989: 58–9) position:

> Phonological rules are ordered with respect to one another. A phonological rule R does not apply necessarily to the underlying representation; rather, R applies to the derived representation that results from the application of each applicable rule preceding R in the order of the rules.

While I agree with Bromberger and Halle (hereafter B&H) that a phonological *process* (to use a more neutral term than their *rule*)[3] need not apply to an underlying representation, the sense of the minimalist hypothesis is that no process may be prevented from applying to a string by virtue of its position in a putative ordering relationship.

A view which, at first glance, appears radically different from the B&H position, is that of declarative phonology (see, e.g. Coleman this volume). The position of this group with respect to derivations is expressed by Coleman as follows (p. 335):

> All rules *R* that alter representations in a way that gives rise to derivations, so that applying *R* before some other rule *R'* brings about a different result from applying *R after R'* are to be prohibited. Such a grammar thus attempts to characterise the set of surface forms of utterances directly, not via representation-altering rules from lexical entries which differ from the surface.

In a later Section I will discuss to what extent Coleman's position differs from that of B&H.

To summarise, we have here three positions with respect to phonological derivations:

(a) The B&H position, which allows free interplay amongst the phonological processes.
(b) The Declarative Phonology position, which allows no derivations at all.
(c) The Government Phonology position, which states that processes take place whenever the conditions for their application are satisfied.



It remains to be seen what the empirical and logical consequences are of each view.

## 1 Derivations in Government Phonology

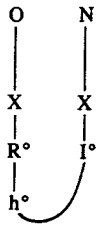
In this Section I will discuss how phonological derivations work within the theory of Government Phonology. As has been stated above, I am assuming a minimalist hypothesis repeated in (1) below.

(1)

Processes apply whenever the conditions that trigger them are satisfied.

Consider a process whereby the element I° in the nuclear head position is shared by the preceding onset which contains the elements R° and h°.[4] What (2) indicates is that any sequence of onset-

(2)




nucleus containing I° as the head of the nucleus must share this element with the preceding onset containing the expression, . . . h° • R°. This will be true for lexical forms as it will be for any such configuration encountered in the course of a derivation. My intention here is to give a neutral interpretation to this event. It may be viewed as a static constraint on well-formed lexical items, or it may be considered in a process model usually called spreading. I will discuss this in greater detail in a later section. A phonological derivation takes us from the lexicon, through the phonology and eventually brings us to signal. In order to provide a complete account of derivations within GP I will have to follow this trail. This implies that derivations cannot be discussed without also discussing the phonological interfaces. To begin this discussion, let me present a fundamental claim of GP: the Uniformity Principle.

### 1.1 The Uniformity Principle

Simply stated the uniformity principle claims that phonological representations are the same type of object at every linguistically significant level. To clarify this point let us consider a theoretical approach that does not respect the uniformity principle: Classical Generative Phonology (CGP). In CGP lexical representation consisted of two-dimensional feature matrices whose cells could be marked with *m*, +, or −; or they could be left unmarked. Before entering the phonology lexical forms underwent a series of marking conventions that replaced the cells containing *m*'s or nothing, with +'s and −'s. Thus, lexical representation differs *in kind* from phonological representation. The former contains *m* or nothing, + or −; the latter is restricted to +'s and −'s. At the end of a phonological derivation, after the last rule has applied, at least some +'s and −'s are converted to scalar values: 1, 2, 3, etc. These scalar values along with some +'s and −'s constitute the form of *phonetic representation*: a set of instructions to the articulatory apparatus for the production of speech sounds.

As we have seen in the preceding paragraph, CGP does not have uniform representations across linguistic levels. To a greater or lesser extent current theories also violate the uniformity principle. The various flavours of 'underspecification' theory are obvious cases in point. At the level of lexical representation and continuing into some arbitrary and language-specific point within the phonology, representations may be 'underspecified', i.e. not directly interpretable. Eventually incompletely specified matrices are 'filled in' by rules and we arrive again at the level of phonetic representation. At this level all representations are fully specified.

The above considerations lead us to the following formulation of the uniformity condition:

(3) *The Uniformity Condition*

Phonological representations are directly interpretable at every level.

One implication of (3) is that there is no linguistically significant level of phonetic representation. We simply come to the end of a phonological derivation. The kinds of changes that take place in the course of these derivations do not involve any fundamental difference in the type of representation involved. The notion *interpretable* in (3) means mappable to and from signal. Phonological representations are as interpretable at the beginning of a derivation as they are at the end of one.

The view expressed here is completely incompatible with any



notion of 'underspecification'. In fact, nothing corresponding to underspecification is even expressible in GP. This follows from the privative nature of phonological expressions.[5] Phonological expressions are combinations of one or more of a set of elements (including an identity element).[6] These elements are not features; they have no values. They are present in or absent from phonological expressions. Consider the following two expressions:

(4) (a) $I^{\circ}$ (= 'i') (b) $A^{+} \bullet I^{\circ}$ (= 'e')

There is no sense that (4a) is or could be more or less specified than (4b). Both (4a) and (4b) are interpretable as they stand. Nothing is lacking. It makes no sense to say that 'i' is an underspecified 'e'. Both are complete theoretical objects. It does make sense to say that $I^{\circ}$ is less complex (contains fewer elements) than $A^{+} \bullet I^{\circ}$. This property is exploited by the theory. Thus, the privative nature of the representational system of GP precludes any form of underdetermination of phonological expressions. The above discussion should provide a general idea of the force of the uniformity condition.

We see that no fundamental changes occur in the course of a phonological derivation. I will continue the discussion with the issue of phonological constituent structure. The key notion here is the *projection principle* which limits derivational changes relating to constituent structure.

### 1.2 The Projection Principle

Changes in or indeed creation of constituent (syllable) structure within a phonological derivation is a property of many current theories of phonology.[7] In contrast, GP denies this possibility completely. This injunction against any sort of structural changes is formulated in Kaye et al. (1990: 221) under the heading of the *projection principle*.

(5) Governing relations are defined at the level of lexical representation and remain constant throughout a phonological derivation.

The projection principle excludes any form of resyllabification. Onsets remain onsets and nuclei remain nuclei. A 'coda' (nonexistent in GP *qua* constituent but may be a synonym for a postnuclear rhymal position) may not change to an onset, or vice versa. Codas are governed and licensed by following onsets.[8] An onset is licensed by a following nucleus. Shifting between onsets and codas clearly violates the projection principle.

There is some latitude in how we are to interpret (5). I do not

believe that we can claim that *all* governing relations are defined at the level of lexical representation. I have in mind many instances of phenomena associated with the nuclear projection such as stress, tonal phenomena and harmonic effects. Inter-onset government[9] would be another dubious case of a governing relation being set at the level of lexical representation. The simplest approach might be to understand governing relations as applying at the $P_0$ projection, i.e. the projection on which all string positions are present. This covers both constituent and transconstituent government. These are the relations involved in questions of syllable structure and resyllabification. Be that as it may, changes of constituent structure, so prominent in other current phonological theories, are excluded from GP.

To sum up, we have seen that GP recognizes no distinct level of phonetic representation. Phonological representations are uniform in kind throughout derivations. Constituent structure does not change in the course of a derivation. Phonological events take place in accordance with the minimalist hypothesis – there are no language-specific 'rule' ordering statements. All these points are at variance with the claims of other modern phonological theories. It might be well to ponder as to how such fundamentally different approaches could emerge. I believe that the principal reasons for these profound differences lie in two areas:

1. the theory of empty categories
2. the phonology-morphology interface.

I will discuss (1) in the following Section. Topic (2) will be the subject matter of Section 2.

### 1.3 Empty categories and derivations

Much theoretical discussion about phonological derivations involves so-called epenthesis or syncope phenomena. Such phenomena are 'accounted for' by means of arbitrary rules and/or syllable or sonority constraints which are typically language-specific. Discussion of these types of events frequently brings in notions of resyllabification subsequent to the application of such processes. Typical data calling for the invocation of epenthesis or syncope are the following:

(6)

| | | | | |
|---|---|---|---|---|
| French | amɛn | 'brings' | amøne | 'to bring' |
| | apɛl | 'calls' | apøle | 'to call' |
| Yawelmani | 'aːmilhin | 'helped' | 'amlit | 'was helped' |
| | logiwhin | 'pulverize' | logwit | 'was pulverized' |



| | | | | |
|---|---|---|---|---|
| Polish | cukier | 'sugar' | cukru | 'sugar' (gen.sg.) |
| | koper | 'dill' | kopru | 'dill' (gen.sg.) |
| Arabic | kṫb | 'he writes' | kṫbu | 'they write' |
| | šṙb | 'he drinks' | šṙbu | 'they drink' |

In GP these data are manifestations of the phonological ECP.[10] The relevant definitions are found below.

(7) *The Phonological ECP*:

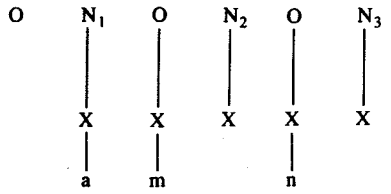
*A p-licensed (empty) category receives no phonetic interpretation*

*P-licensing*: 1. domain-final (empty) categories are p-licensed (parameterized)
2. properly governed (empty) nuclei are p-licensed
3. a nucleus within an inter-onset domain[11]

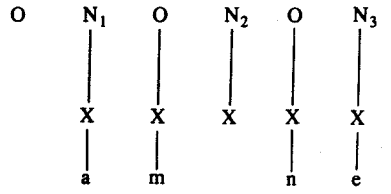
*Proper government*:
$\alpha$ properly governs $\beta$ if:
1. $\alpha$ and $\beta$ are adjacent on the relevant projection,
2. $\alpha$ is *not itself* licensed, and
3. no governing domain separates $\alpha$ from $\beta$

Consider the first line of French data in (6) above. The following structures are involved. Epenthesis or syncope effects involve the

(8) (a) amène




(b) amener




interpretation of $N_2$ in (8a) and (8b) above. In (8a) $N_3$ the potential proper governor of $N_2$ is itself licensed since all domain-final

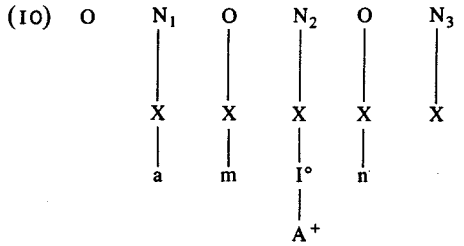
empty nuclei are licensed in French. Since $N_2$ is not P-licensed it must receive phonetic interpretation according to the ECP. If nothing further is said, this is strictly a case of *interpretation* and does not involve any change in representation. This is to say that

(9)



the same object (e.g. (g)), is either interpreted as silence (if it is licensed) or as some phonological expression. This is determined by the Empty Category Principle (ECP). An unlicensed empty nucleus is normally interpreted as [ɨ] which is what the identity element sounds like. An unlicensed empty nucleus is realized as such in Arabic, (European) Portuguese and Korean. What is crucial here, is that in such languages, the representation of (9) does not vary whether or not it is audible. Its interpretation, as stated above, is a matter for the ECP.

In French we do need to add a representational change. The empty nucleus $N_2$ of (8a) is the head of its domain (it bears primary stress). In French, an empty position cannot serve as a domain head. An empty position acting as domain head receives the phonological expression $A^+ \bullet I^\circ$.[12] This is shown in (10).

(10)



Two things need to be said about this derivation. First of all, the phonological expression $A^+ \bullet I^\circ$ cannot be viewed as some form of 'underspecified' or 'default' vowel. An empty position and one filled by the expression $A^+ \bullet I^\circ$ are two distinct theoretical objects. An empty position will display ECP effects, i.e. alternations with zero. The expression $A^+ \bullet I^\circ$ shows no such effects. This point is illustrated in (11).

(11) (a) *Empty position*

| | | | |
|---|---|---|---|
| apɛl | 'calls' | aple | 'to call' |



(b) $A^+ \bullet I^\circ$

| | | | |
|---|---|---|---|
| sɛl | 'saddle' | sɛle[13] | 'to saddle' |

Second, the presence or absence of an audible nucleus between 'm' and 'n' does not involve any change of constituent structure. If we compare (8a) and (8b) we see that their structures are identical. There is no reason to take the derivation any further. The ECP and the French-specific fact about domain-heads suffices to give the correct interpretation to (8a).

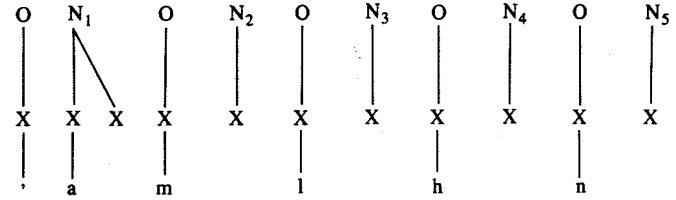
The Yawelmani data in (6) give a further example of how derivations involving rules and rule ordering in standard approaches, require no such treatment in GP. Let us reconsider these data repeated in (12).

(12)

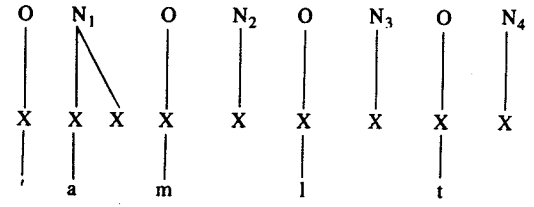
| | | | |
|---|---|---|---|
| 'a:milhin | 'helped' | 'amlit | 'was helped' |
| logiwhin | 'pulverize' | logwit | 'was pulverized' |

Notice that the vowel length in the first example is sensitive to licensed status of the following empty nucleus. It remains long when the following empty nucleus is unlicensed hence audible and shortens when followed by a licensed empty nucleus.

(13) (a) 'a:milhin




(b) 'amlit



The comparison of (13a) and (13b) shows the interplay of vowel shortening and the appearance or non-appearance of the following nucleus. The standard account of the Yawelmani data is to posit two rules: one of vowel shortening in closed syllables and the other of epenthesis. The two rules may be expressed as follows:

(14)

(a) *Epenthesis*
$\emptyset \rightarrow$ i/C_C{C,#}

(b) *Vowel shortening*
v $\rightarrow$ v̆/_C$ (where $ is a syllable boundary)

Applying these two rules to the Yawelmani forms yields the following traditional derivations:

(15)

| | 'a:mlhin | 'a:mlit |
|---|---|---|
| Epenthesis | 'a:milhin | n.a. (= not applicable) |
| Vowel Shortening | n.a. | 'amlit |

If we were to reverse the order of application of these two rules, or allow each to apply to the initial representation the results would change for the worse.

(16)

| | 'a:mlhin | 'a:mlit |
|---|---|---|
| Vowel Shortening | 'amlhin | 'amlit |
| Epenthesis | 'amilhin | n.a. |

The Yawelmani data, viewed in this way, appear to require that the rules be ordered as shown in the derivation of (15). Since GP allows for only the minimalist hypothesis, it will be useful to see how this theory treats these data.

Let us assume that Yawelmani licenses domain final empty nuclei. Let us also assume that unlicensed empty nuclei are spelled 'i', however they may be pronounced. The derivation of (13a) proceeds as follows: $N_5$ is licensed since it appears in domain final position. $N_4$ cannot be properly governed by $N_5$ since this latter nucleus is itself licensed. Therefore $N_4$ is unlicensed and interpreted as 'i'. $N_3$ is in a position to be properly governed by $N_4$. $N_4$ is not itself licensed and no governing domain intervenes between $N_3$ and $N_4$. Thus, $N_3$ is P-licensed through proper government. This brings us to $N_2$, the so-called epenthetic vowel of traditional treatments. $N_2$'s potential proper governor, $N_3$ is P-licensed via proper government. Thus, $N_3$ is not a proper governor for $N_2$ and $N_2$ remains unlicensed appearing as 'i'.

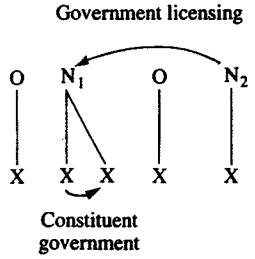


Before turning to the issue of vowel shortening, let us review the derivation of *a:mlit* whose structure is given in (13b) above. $N_4$ is domain-final and hence licensed. $N_3$ lacks a proper governor ($N_4$ is itself licensed) and is, accordingly unlicensed. Being unlicensed $N_3$ can serve as a proper governor for $N_2$. Therefore $N_2$ is P-licensed and receives no interpretation. These derivations show that no rule of epenthesis is required. The interpretation of $N_2$, the nucleus in question, is dealt with by a principle of UG: the ECP, along with the parameter settings appropriate to Yawelmani. Let us now turn to the question of vowel length.

Charette (1990) has shown that governors must be licensed in order to govern. She calls this effect 'government-licensing'. In the French word, [rɛstorã] 'restaurant', the nucleus containing 'o' government-licenses the onset 't' to govern the preceding 's'. Properly governed nuclei are not government-licensers in French. Thus, a word like marg∅rit∅ 'marguerite' 'daisy' must be pronounced [margərit] are not *[margrit]. The empty nucleus located between the 'g' and the 'r' cannot be properly governed since it is required to government-license the 'g' to govern the preceding 'r'. In French, the conflict between proper government and government-licensing is resolved in favour of government-licensing.

In a recent article Yoshida has proposed that branching nuclei are also subject to government-licensing parameters. In such languages the head of a branching nucleus must be government-licensed to govern the weak member of the constituent. This proposal is schematized in (17) below.

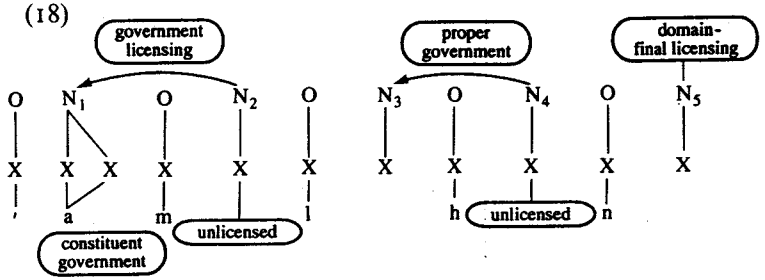
(17)



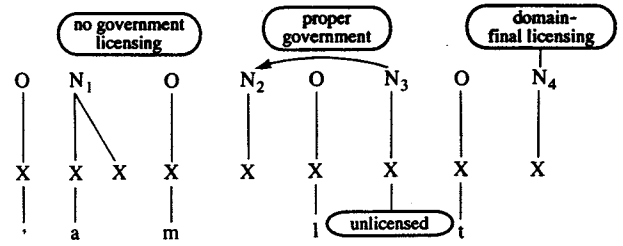

In (17) above, $N_2$ is the government-licenser. Yoshida states that $N_2$ cannot be P-licensed and still government-license $N_1$. The head of the branching nucleus $N_1$ is unable to govern its weak position. It has not received the license to do so. The weak position cannot receive its phonological content from the head of $N_1$. It is inaudible

and $N_1$ gives the impression of being a short vowel. One of Yoshida's arguments for this position is that if branching nuclei require government licensing in some languages, then domain-final long vowels should be impossible in precisely those languages. This appears to be the case. Yawelmani has no domain-final long vowels. Contrast this situation with English where branching nuclei occur freely before licensed empty nuclei as in 'keep' or 'teamster' and domain-final branching nuclei occur equally freely as in 'see' or 'day'. Thus, the apparent length of $N_1$ in (13a) and (13b) is dependent on the status of $N_2$. $N_1$ receives a government license from $N_2$ if $N_2$ is not P-licensed. If $N_2$ is P-licensed, then $N_1$ receives no license to govern its weak position and the vowel is interpreted as short. The situation is schematized in (18).

(18)




(a) Derivation of 'a:milhin




(b) Derivation of 'amlit

The derivations of (18a) and (18b) clearly show the difference in approach between GP and rule-based systems. The phonological ECP takes care of the interpretation of empty nuclei. What we need to know about Yawelmani is that heads of branching nuclei must be government licensed and that unlicensed empty nuclei are spelled



out as 'i'. It does not really matter if one considers these devices as processes or conditions of well-formedness of phonological structure. Events take place where they must and the Yawelmani results follow from the principles and parameters approach illustrated here.

The above discussion was designed to give a taste of phonological derivations in GP. Nevertheless, the story is incomplete without a discussion of the organization of the lexicon, methods of lexical access, and the interface between the lexicon and the phonology. These matters will be discussed in the following Section.

## 2 Phonology, morphology and the lexicon[14]

A number of issues need to be addressed when discussing the phonology-morphology interface. To what extent is morphological structure visible to the phonology? How is morphological structure represented in the phonology (if at all)? Are all lexically related forms derived phonologically from the same source? Are phonological events sensitive to morphological structure? I will lead off the discussion with the question of the visibility of morphological structure in the phonology.

### 2.1 Morphological effects in the phonology

The simplest hypothesis regarding the visibility of morphological information in the phonology, is that there is none. Accepting this hypothesis would entail the prediction that there should be no correlations between phonological representations and morphological structure. This being the case, it is easy to show that this hypothesis is false. To take but two examples, first consider the distribution of the English pseudo-cluster[15] 'mz' as in 'dreams'. Notice that 'dreams' is morphologically complex consisting of a stem plus a suffix. If morphology were totally invisible to the phonology there would be no reason to expect that all such forms are morphologically complex. And yet, this is exactly the case.[16]

In French we find differences between 'son ami' [sɔ̃nami] 'his friend' vs. 'bon ami' [bɔnami] 'good friend'. This same distinction is found in forms with 'non-', 'not, non'.[17] Cf. 'non-attraction' [nɔ̃natraksjɔ̃] 'non-attraction' vs. 'nonobstant' [nɔnɔpstã] 'notwithstanding'. Now many French words that are morphologically simplex contain a sequence of an oral vowel followed by a nasal consonant such as [ɔn] in 'sonate' [sɔnat] 'sonata'. However, forms containing a nasal vowel nasal consonant sequence such as [ɔ̃n] are nearly always morphologically or syntactically complex. These types of correlations, which are quite common, are at odds with

the hypothesis that all morphological structure is invisible to the phonology.

We must conclude then that some morphological structure is visible to the phonology. I will now try to define precisely exactly what that structure is and what form it takes in the phonology. I will conclude that morphological structure has two effects on the phonology: little and none. These two interactions are called *analytic* and *non-analytic*.

## 2.2 Analytic and non-analytic morphology

We have established above that there is some interplay between morphology and phonology. I will argue that this interplay is minimal and is confined to a subset of morphological structures. I will show that some morphology is invisible to the phonology; forms displaying this kind of structure are indistinguishable from morphologically simplex forms. I will begin with morphology that is visible to the phonology.

### 2.2.1 *Analytic morphology*

It has been noted above that a form like 'dreams' which displays the 'mz' pseudo-cluster is invariably morphologically complex. A more extreme example is the form 'sixths' with the pseudo-cluster [ksθs]. Why are these bizarre 'clusters' correlated with morphological complexity? What does this tell us about morphological penetration into the phonology? Suppose we assume that only morphological domains can be represented in the phonology. To take the simplest case, a compound like 'blackboard' would have the following structure:

(19) [[black][board]]

In (19) we see three pairs of brackets. I will use brackets to enclose a phonological domain. The form in (19) has three domains: *black*, *board* and *blackboard*. How are we to interpret these brackets? In fact the brackets are not objects in themselves but rather represent instructions as to how the phonological string is to be processed. To explain what I mean let me define two functions: *concat* which takes two arguments which are strings and returns the string which results from concatening the second argument to the first. For example, concat('abc','def') = 'abcdef'. The second function is $\varphi$. This function has one argument, a phonological string, and returns the application of the phonology to its argument, also a phonological string.[18] The expression $\varphi$(X) means, 'apply phonology to the string X'. $\varphi$(X) returns the phonological string which results from the application of phonology to its argument.



We now have the necessary tools to give an exact definition to (19) above. This is shown in (20).

(20) $\varphi$(concat($\varphi$(black),$\varphi$(board)))

In plain language (20) means, 'apply phonology to "black" and to "board"; concatenate the results to form a string and apply phonology to that string'. The brackets that are found in the representation of (19) are not part of phonological representation. There are no 'boundaries'. The brackets delimit phonological domains which are arguments to functions like *concat* and $\varphi$.

The presence of a stress bearing nucleus (among other things) is symptomatic of domainhood. This is due to the Licensing Principle given below.

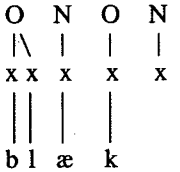
(21) *The Licensing Principle*

All positions in a phonological domain must be licensed save one: the head of the domain.

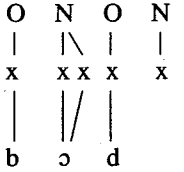
Metrical structure can be viewed as a form of licensing at the level of nuclear projection. The one unlicensed position of a domain will receive the main stress of that domain. In addition to (21), domains impact on the phonology by virtue of the fact that domain-final empty nuclei are licensed in a number of languages including English. Thus, each domain of *blackboard* will end in a licensed (hence inaudible) empty nucleus.

(22)

(a)




(b)




The domains of both (22a) and (22b) end with an empty nucleus. Both of the empty nuclei are licensed and hence the nuclei following the 'k' and the 'd' of *blackboard* are inaudible.

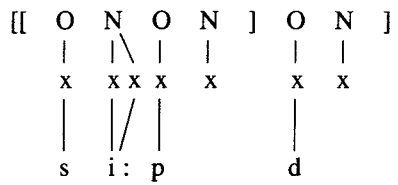
The above illustrates one type of structure involving morphologically complex forms. Given two morphemes, A and B, we can incorporate them into a structure of the form [[A][B]]. This was the case for *blackboard* as it is for most English compounds. This does not, however, exhaust the possibilities. There is a second type of

structure involving two morphemes but which only involves two, and not three, domains. This structure is [[A]B]. It involves the domains A and AB but not B. The interpretation of this structure is given in (22, 23).

(23) $\varphi(\text{concat}(\varphi(A),B))$

What (22, 23) means is, 'do phonology on A and concatenate the result with B; then do phonology on the result of the concatenation'. This kind of structure occurs in the bulk of English regular inflexional morphology. It is also associated with many derivational suffixes such as '-ness' ('darkness') and '-ment' ('enlargement'). Consider the regular past tense of an English verb like 'seep', viz. 'seeped'. Its structure is [[seep]ed]. The details are given in (24).

(24)




Once again, an empty nucleus is found at the end of each of the two domains. They will both be licensed for the reasons discussed above. Notice that the first syllable is open and not closed. It has an onset followed by an empty nucleus as required by *coda licensing*.[19] In such circumstances there is no reason not to expect a branching nucleus in this situation and indeed the length of the stem vowel is constant. The English champion of this type of morphology is probably *sixths* which has the structure [[[sɪks ∅]θ ∅]s ∅]. Each of the domain-final empty nuclei are licensed being domain-final. Although the constituent structure of a form like *sixths* looks impressive, it is rather pedestrian containing only the rhymal-onset sequence *ks*. Analytic morphology has an interesting property in English. The distribution of empty nuclei is very restricted in English (in contrast with languages like (European) Portuguese, Polish, Arabic or French). It is found almost exclusively at the ends of domains and rarely elsewhere.[20] Thus, detection of empty nuclei presents us with a fairly reliable parsing cue. A form like *dreams* gives us two indications that the 'mz' sequence is a spurious one (i.e. the consonants are separated by an empty nucleus): first, the vowel length is maintained which would be impossible if 'mz' were a true transconstituent cluster, and second, there is a lack of



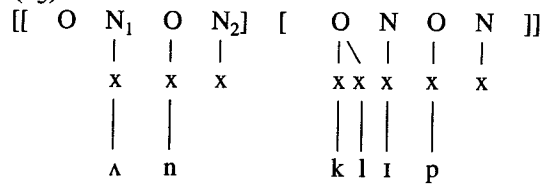
homorganicity between 'm' and 'z'. This is impossible for true clusters. Hence much of analytic morphology is phonologically parsable.

Up to this point we have looked at structure of the form:

1. [[A][B]]
2. [[A]B]

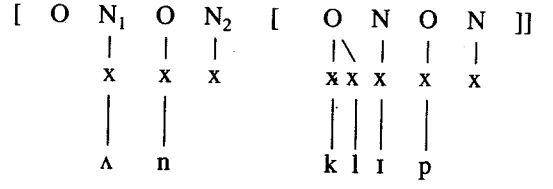
for two morphemes, A and B. The question arises as to whether the third possible structure, viz. [A[B]] is attested as well. In fact this type of structure does not appear to be attested. Analytic prefixes such as English 'un-' show the type of structure in (i). Consider a form like *unclip*.

(25)




The empty nucleus $N_2$ is licensed by virtue of its domain-final position. The first nucleus of *clip* could not be its proper governor because of the intervening governing domain: the branching onset *cl*. This excludes the structure in (26) as a possible source for *unclip*.

(26)




In (26) there are only two domains: *clip* and *unclip*. *un* does not constitute a domain in itself. Since $N_2$ is not domain-final in (26), it can only be licensed by a following proper governor. This is not possible for the reasons stated above. Thus, (26) cannot be the structure of *unclip*. We conclude that for two morpheme combinations, analytic morphology provides only the possibilities shown in (1) and (2) above. The pair, *superman* and *postman*, provide a clear contrast in Southern British English. Note the stress and the assigned structures.

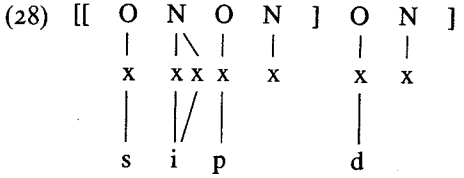
(27) superman [[súper][màn]] postman [[póst]măn]

The *man* in *superman* is a domain and accordingly is stressed. In

contrast the *man* in *postman* is not a domain and thus remains unstressed.

Finally, let us return to the form *seeped*. Its structure is given in (28) below.

(28)




The past tense suffix *-ed* does not form a domain by itself. There is still another reason to affirm this. The suffix consists of two positions: an onset position and a nuclear position. Both positions are licensed. The onset position is licensed by its following nucleus and the nuclear position is (p-)licensed by virtue of its domain final position.[21] If *-ed* were a domain, this would be in violation of the licensing principle which states that a phonological domain must have one unlicensed position, viz. its head.

In this Section we have seen the sort of morphology that impacts on the phonology in the form of domains. I have illustrated two modes of combining two morphemes, A and B in an analytic fashion:

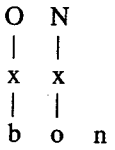
(29) (a) [[A][B][[A][B]]
(b) [[A]B]
(c) *[A[B]]

(29a) and (29b) are commonly found but (29c) is not attested. As a final point let us consider Prunet's analysis of French *son ami* vs. *bon ami*. The facts follow directly if we assume that *son* and *ami* occupy separate domains in the former example but are only found in the same domain in the latter. Prunet (1986) follows an idea of Vergnaud's (1980) concerning French liaison consonants. He assumes that they are floating. Thus, *son* and *bon* will have the structures given in (30) below.

(30)

(a) son


(b) bon

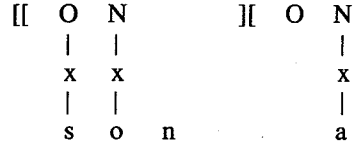



The structures in question are shown in (31).



(31)

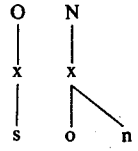
(a) son ami




(b) bon ami

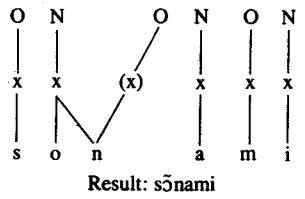

Crucially *son ami* contains internal domains while *bon ami* does not.[22] Taking the derivation of *son ami* we must apply the phonology to the internal domain (30a). The floating *n* has no available onset to which it can associate. Therefore it must associate to the preceding nucleus as shown below.

(32)




When phonology is done on the external domain, an empty onset is available for the *n*. However, the principle of *strict cyclicity*[23] states that the association created in the inner domain cannot be undone in an external domain. The association remains and the *n* also links to the available onset as shown below.

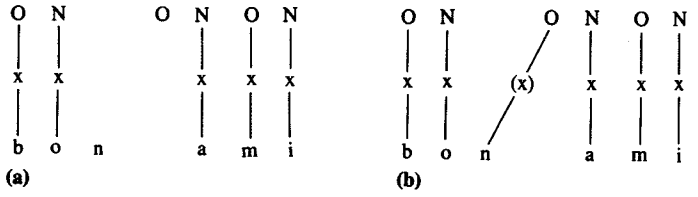
(33)




Result: sɔ̃nami

Let us turn now to the derivation of *bon ami* whose structure is shown in (34a) below. There are no internal domains so there is no point at which an available onset is not accessible to the floating *n*. Therefore, the *n* never attaches to the preceding nucleus as we see below.

(34)




Result: bɔnami

For the moment I leave the question open as to whether *bon ami* goes through any kind of derivation in the phonology. What is important is that there is no internal domain necessitating the association of the floating *n* to the preceding nucleus. Let us leave analytic morphology for the moment and turn our attention to the second type of morphological interaction with the phonology: non-analytic morphology.

### 2.2.2 *Non-analytic Morphology*

In the previous Section we saw that morphology could have an impact on the phonology to the extent that morphological structure was present in the form of domains. These domains have the effect of respecting the integrity of the internal domains. Consider an analytic verbal suffix like *-ing* in English. Its stricture is of the form [[V]ing] where *V* is a verb stem. To pronounce such a form one simply pronounces the stem on its own and appends the suffix. The pronunciation of *V* on its own and *V* in the structure [[V]ing] are pretty much identical. This is what I mean about 'preserving the integrity' of the internal domains. This procedure does not apply in all forms of morphology. Consider two derivations of the word *parent*: *parenthood* and *parental*. The former does respect the integrity of the internal domains. The pronunciation: *párĕnt* is preserved in *párĕnthòod* but not in **párĕntăl*. Instead we get *păréntăl*. The morphology of *-al* is interacting with the phonology in a different way than that of *-hood*.

I will claim that the *-al* type of morphology is invisible to the phonology. That is to say that the phonology reacts as if there were no morphology at all. Thus, a form like *parental* is treated in exactly the same way as a form like *agenda* or *advantage*. Following our earlier model, we can characterize the internal structure of a form like *parental* as follows:

(35) [A B]

Morphology which does not carry domains to the phonology will



be referred to as *non-analytic morphology*. Since the only effect that morphology can have on the phonology is the presence of internal domains and since non-analytic morphology has no internal domains, it follows that non-analytic morphology should be invisible to the phonology. This is indeed the case. To sharpen the analytic–non-analytic distinction, let us compare two negative suffixes in English: *un-* which is analytic, and *in-*, which is not.

These prefixes have rather different properties. For one thing *un-* is completely insensitive to what consonant follows it. Indeed it can be followed by any onset expression of English. In particular, it can be followed by *r* or *l* as in *unreal* or *unlawful*. This property follows from its analytic morphology. Since the prefix final *n* is not adjacent to the following onset, there is no reason to expect any phonotactic restrictions on what follows it.

(36) [[ ʌnØ ][ ri:lØ ]]

As we see in (36) above, the *n* and the following *r* are not adjacent. They are separated by an empty nucleus. This is consistent with *un*'s status as an analytic prefix. In fact, *nl* and *nr* are not possible true sequences (sequences not separated by an empty nucleus). A nasal cannot be the head of a branching onset because of its neutral charm and *nl* and *nr* are not transconstituent sequences because both *l* and *r* are less complex than *n* and therefore cannot govern it – a requirement for a transconstituent sequence. Given the distribution of empty nuclei in English, sequences like *nØl* and *nØr* are phonologically parsable. Thus, the analytic forms *unreal* and *unlawful* are immediately analysable as *un-real* and *un-lawful*.

The situation is quite different with respect to the non-analytic prefix *in-*. There are no internal domains so appending *in-* to a stem must yield a well-formed phonological domain. Using the notational system described above, a non-analytic combination of two morphemes is interpreted as follows:

(37) $\varphi$(concat(A,B))

That is, concatenate the two strings, A and B, and perform phonology on the result. Consider the formation of *irrational*. Concatenation of the strings *in* and *rational* yields *inrational*. But *inrational* is not a well-formed string for the reason stated above. The cluster *nr* is not possible in any language. Accordingly, the *n* is dropped from the string and we have *irrational*, which is a possible domain. The same applies to the formation of *illogical* from *in* and *logical*: *nl* is impossible and once again the *n* is dropped to form the word.

To recapitulate, non-analytic morphology is completely invisible to the phonology. It contains no internal domains nor any other phonological indication that it is a morphologically complex form. Non-analytic forms are not phonologically parsable. They have exactly the same phonological properties as any simplex form. I will assume that any form manifesting non-analytic morphology is listed in the lexicon. The discussion of a form like *irrational* does not reflect a phonological derivation but rather a strategy for the formation of a lexical representation. To drive home this point it will be interesting to consider the difference between regular and irregular English verbal inflectional morphology.

### 2.3 Regular and irregular morphology

We have stated above that English regular verbal inflectional morphology is analytic in nature. A past tense form like *seeped* shows this clearly. Notice that the vowel length is maintained before the pseudo-cluster 'pt' indicating that these two segments are never adjacent. The form is phonologically parsable as [[*seep*]*ed*]. Consider what makes this form parsable: *pt* is a possible transconstituent cluster in English (cf. *inept*, *apt*, *adopt* and so forth). Thus, it is the fact that there is a long vowel before *pt* that reveals its status as a pseudo-cluster. The *p* must be in the onset since the binarity theorem excludes a branching nucleus within a branching rhyme.

Let us now consider an irregular past tense form similar to *seep*, viz. *keep*. The past tense form is *kept*. What is striking about this form, is that its very irregularity involves its vowel length. As we have just seen, the vowel length of regular verbs is crucial for their parsability. The form *kept* could be a simplex English word just like *apt* or *adopt*. Thus, this irregular past tense form is not phonologically parsable. There is no phonological hint of its complex morphological structure. Let us follow this line with some other irregular verbs. Compare the regular verb *grieve* with irregular *weave* and *leave*. The regular past tense *grieved* is parsable for two reasons: first, the vowel length requires that *v* be in an onset position. This onset is followed by a licensed empty nucleus. The only p-licensing condition that could be met in this case is the domain final one. Second, the pseudo-sequence *vd* is not a possible transconstituent sequence. No morphologically simplex forms in English contain this pseudo-sequence. Thus, both the vowel length and the pseudo-sequence *vd* are parsing cues yielding the analysis [[gri:v Ø]d Ø]. Now consider the two irregular verbs, *weave* and *leave*. If we are right about the irregular strategy being to render irregular forms unparsable, then both these cues must be dealt with somehow. We can make the following prediction about the ultimate lexical representations of irregular forms: they will not contain a detectable pseudo-sequence,[24] and a long vowel will not be followed by a pseudo-sequence. In the case of *weave* the vowel length is maintained. Therefore the pseudo-cluster must go. There is a random vowel change and the resulting form is *wove*. The strategy has been followed: *wove* is indistinguishable from a simplex form like *clove* or *stove*. Notice that since non-analytic forms are listed in the lexicon, there is no particular reason to keep any trace of past tense morphology. This is the case for *wove*.



An alternative strategy is possible, to wit create a true cluster and shorten the preceding vowel. This will have the effect of masking the morphological complexity of the form and making it appear like a normal, simplex English word. But *vd* is not a possible true cluster. The closest sequence to *vd* that is, is *ft* (cf. *soft*, *rift*, etc.). This strategy is used to form the past tense of *leave*. Given the strategy just described, the predicted outcome for this verb should be *l*ft*, where '*' represents some randomly selected short vowel. In fact we get *left* confirming our prediction.

One final example which does not involve a branching nucleus will be instructive here. Consider the verb *wing* (as in 'She winged her way home'). The regular past tense is phonologically parsable because the pseudo-cluster *ŋd* involves a non-homorganic sequence of nasal plus stop. True sequences of nasal plus stop are always homorganic. Thus, *winged* could only be analysed as [[wing Ø]d Ø] with the two domain-final empty nuclei being licensed. If we take an irregular verb like *sing*, vowel length is not an issue but the resulting final pseudo-sequence is. Accordingly, we expect no remnant of past tense morphology in the past tense form. In fact only a random vowel change takes place and the lexical representation is *sang*, a possible simplex form along the lines of *bang* or *fang*.

Summing up these observations we come to the following conclusion:

(38) 'Irregular' morphology is always non-analytic.

What this means is that irregular morphology always hides its own morphological complexity.[25] The difference between morphologically regular forms like *seeped* and morphologically irregular forms like *kept* is that the former is phonologically parsable, i.e. identifiable on inspection as morphologically complex, while the latter is not. Its morphology has been rendered completely invisible. Why should this be so? Suppose that there are two ways of dealing with morphologically complex forms: (a) computation and (b)

lookup. In the first case a phonological parse is done yielding, say, *seep* and *-ed*. The meaning of *seeped* is then computed on the basis of these components resulting in something like 'past governs seep'.

In the latter case, *kept*, the user is invited to simply look up the form, the way she would deal with *boy* or *go*. The relevant information concerning the morphological structure will be found in the lexical entry for *kept*. The form is 'precomputed' as it were, and the results are stored in the lexicon. This model has important implications for the nature of phonological derivations. Note that *keep* and *kept* are distinct lexical entries. As such they do not necessarily share a common phonological representation. It has been commonly assumed that *kept* is derived from a form that shares the same stem as *keep*; something like *keep* + *t*. The vowel must be shortened before two final consonants *pt*. The shortening rule will bleed the rule of 'vowel shift' which converts long ɛ: to i:. Notice that ordering vowel shortening before vowel shift is crucial in order to block the derivation of *kipt.

In our model there is no direct phonological relationship between *keep* and *kept*.[26] The latter form is a well-formed English word exactly like *adopt* or *apt*. Since non-analytic morphology involves separate lexical items much of the argumentation used to support extrinsic rule ordering or level ordering as in lexical phonology, simply vanishes. Alternations like vowel shift, velar softening (as in *electric-electricity*), trisyllabic laxing (as in *opaque-opacity*) are simply not reflections of phonological events. This is all to the good since GP is incapable of expressing any of these processes. Phonological events involve fusion or fission of phonological elements. It further requires that there be a non-arbitrary association between an event and the context in which the event occurs. In fact, GP predicts that the so-called rules cited above could not occur in the phonology. This prediction is rich in empirical content – it need not be true. Consider the analytic verbal suffix *-ing*.[27] We are obliged to claim that neither velar softening nor trisyllabic laxing could be triggered by *-ing*. This is correct: *back-backing*, **back-bassing*; *teeter-teetering*, **teeter-tittering*.

With respect to English verbal inflectional morphology we have the following picture: the regular morphology is analytic while the irregular morphology is non-analytic. I have suggested that the strategy of masking the morphological structure of irregular forms constitutes an invitation to look up rather than to compute the ultimate meaning. Since verbal irregularity is an arbitrary lexical property,[28] it follows that which past tense forms are analytic and which are not is also an arbitrary lexical property. Indeed, if we



look at compounds whose second member is *metre* we see the amount of variation that can exist.

(39)

Compounds in *metre*

| *Analytic* | | *Non-analytic* | |
|---|---|---|---|
| millimetre | [[mílli][mètrě]] | thermometer | [thěrmómětěr] |
| kilometre | [[kílo][mètrě]] | kilometre | [kǐlómětrě] |
| altimeter | [[álti][mètěr]] (British) | altimeter | [àltímětěr] (American) |

As we can see from the data in (39) some *metre* compounds are analytic, some are non-analytic. Sometimes this division spans the English-speaking world (*millimetre* vs. *thermometer*); sometimes it varies with the individual (*kilometre*); and sometimes it is subject to dialect variation (*altimeter*). Although the analytic–non-analytic distinction is largely lexically arbitrary, it is correlated with compositionality. Consider the former English compound *cupboard*. Today its meaning is far removed from 'a board for cups'. We find that its former analytic morphology, [[cup][board]] has been lost. Phonologically *cupboard* behaves like an ordinary word with no internal structure (cf. *mustard*, *custard*). The pseudo-cluster *pb* has been eliminated as has the compound type stress *cúpbòard*, in favour of *cúpbŏard*.

In French[29] *mon oncle* [mɔ̃nɔ̃kl] displays its internal domains. Its meaning is compositional: *mon* 'my', *oncle* 'uncle', *mon oncle* 'my uncle'. In Quebec French the historical possessive pronoun *mon* has been absorbed into the word for 'uncle' itself. So 'uncle' is *mononcle* and 'my uncle' is *mon mononcle*; 'your uncle' is *ton mononcle*, and so on. It is not surprising that Quebec French *mononcle* has lost its internal domains. It is no longer phonologically parsable and appears as [mɔnɔ̃kl] and not *[mɔ̃nɔ̃kl]. There is no invitation to parse Quebec French *mononcle* as [[mon][oncle]].

One of the most important results of the model proposed here is that *morphologically related forms which resemble each other phonologically are not necessarily derived from a common source*. The pair *electric–electricity* is not prima facie evidence for the existence of a process of velar softening. The pair *opaque–opacity* does not, in itself, offer evidence for vowel shift or trisyllabic laxing. In providing evidence for a theory of phonological derivations, it is crucial to take into account the assumptions that are made about the phonology-morphology interface. It is equally important to know which portions of the available phonological data are to be derived from a common source and accordingly, provide information on the phonological events involved in their derivation.

Let us take a final look at some derivations that crucially involve the analytic–non-analytic distinction. These derivations are from Polish and revolve around the analysis of Polish *yer* (i.e. the Polish empty nucleus). Polish yers behave in exactly the same way as Yawelmani empty nuclei discussed above. If yers are p-licensed in Polish, they are inaudible. If the yers are not p-licensed they surface as ɛ (spelled 'e' in Polish orthography). Nothing further need be said about the basic facts. Derivations involving yers respect the minimalist hypothesis. No rule ordering nor complex interactions between the morphology and the phonology are required.

In contrast, Rubach (1984a) gives a rule-based account that appears to require both rule ordering and distinctive types or rule application (cyclic vs. postcyclic). Both these devices are at variance with the minimalist hypothesis. Let us compare these two approaches and see how their different initial assumptions give very different views of derivations.[30]

Rubach suggests that yers are underlying lax high vowels. He posits two of them: one which is [− back] and causes palatalization and the other, which is [+ back] and does not. There are also two rules involved in the derivations. A cyclic rule of lowering which converts both yers to ɛ and rule of yer deletion that removes all unlowered yers. This rule applies post-cyclically. The rules are given in (40).

(40)

Lower (cyclic)
[+ syll, + high, − tense] → [− high]/____$C_0$[+ syll, + high, − tense]
Yer Deletion (postcyclic)
[+ syll, + high, − tense] → ∅

The rule of Lower converts a yer to ɛ when the following vowel is another yer. The rule of Yer Deletion then removes all unlowered yers. Sample derivations follow in (41).

(41)

| | | |
|---|---|---|
| | pi☺s☺ | pi☺sa 'dog (nom.sg., gen.sg)' |
| lower | pies☺ | n/a |
| yer deletion | pies | psa |
| | pi☺s☺cz☺k☺ | 'dog (double diminutive)' |
| lower | pieseczek☺ | |
| yer deletion | pieseczek | |

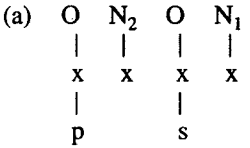
In (41) the symbol '☺' represents a yer. In the nominative singular *pies* Rubach must stipulate that this form takes a nominative singular suffix which contains a yer. The stem yer is lowered before the suffix yer. The suffix yer, not being followed by another yer is not lowered. It must be deleted and *pies* is derived. The genitive singular suffix is *-a*, not a yer. Therefore, the stem yer is not lowered and must be deleted by the rule of yer lowering. This yields *psa*. Rubach does not wish the lowering rule to apply to non-derived forms (note that this move is impossible if we respect the minimality hypothesis). The rule is stipulated as being cyclic and thus does not apply to non-derived forms.

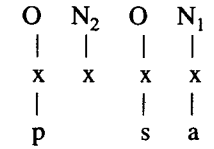


In the derivation of the double diminutive of 'dog', we have a sequence of four consecutive yers. The rule Lower will lower all but the final one, which in turn will be removed by the Yer Deletion rule. This yields *pieseczek*. I have already stated that this analysis violates the minimalist hypothesis, not to mention the projection principle.[31] It is interesting to note that by assuming that the Polish yers are empty nuclei we can derive all the Polish forms without any recourse to rule ordering or stipulations about cyclic/post-cyclic rule application. The minimalist hypothesis remains: events occur when their conditions are satisfied.

Let us consider how these Polish forms are derived in GP. We use the same mechanisms discussed above in the Yawelmani example. The phonological ECP is applied to the structures below. All we need to know is that Polish unlicensed empty nuclei are realized as ɛ and domain-final empty nuclei are licensed in Polish.

(42)

(a)



(b)


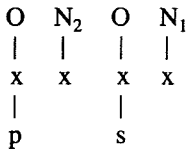

In the nominative form (42a) the domain-final nucleus, $N_1$ is licensed. As such it cannot serve as a proper governor for $N_2$. This latter nucleus is unlicensed and accordingly is realized as ɛ as per the Polish parameter settings.[32] In (42b) the domain-final nucleus is non-empty. In fact it is *-a* the genitive singular suffix. It is unlicensed and can p-license the preceding empty nucleus $N_2$. The diminutive form of 'dog' presents an interesting problem. Note the pairs of forms below.

(43)

(a) pies − psa 'dog: nom.sg./gen.sg.'
(b) pies − piesek 'dog: nom.sg./dim.'

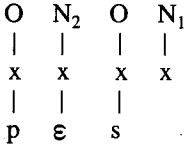
In (43a) the stem yer is licensed by the genitive singular suffix. In (43b) the stem yer is not licensed by the initial nucleus of the diminutive suffix. This vowel is itself a yer as we shall see anon. Nevertheless, it is unlicensed and therefore a potential proper governor for the stem yer. What is going on? In reality (43a,b) simply illustrate the analytic–non-analytic division. The genitive suffix *-a* is non-analytic whereas the diminutive suffix *-∅k∅* is analytic.[33] The derivation of *piesek* is now straightforward. The structure of *piesek* is [[p∅s∅]∅k∅]. Following the model described above, we must perform phonology on the internal domain [p∅s∅].

(44)



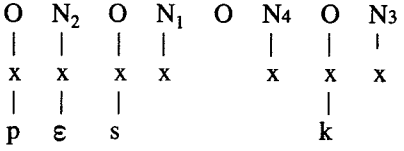

$N_1$ is licensed being domain-final. $N_2$ has no proper governor and remains unlicensed. The unlicensed $N_2$ is realized as ε.

(45)




The structure in (45) is now concatenated with the diminutive suffix and phonology is performed again.

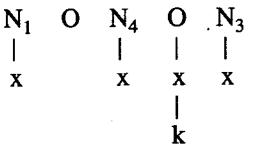
(46)




Note that the domain-final licensed empty nucleus is now immediately followed by an empty onset and then by a following nucleus. The structure in question is shown below.

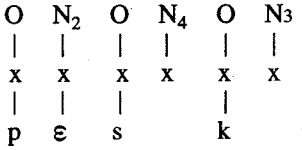


(47)




In a phonological string an empty nuclear position can never be immediately followed by another nuclear position (empty or filled). In such cases the first nuclear position is removed from the structure along with the following onset. This is not a special fact about Polish but rather a universal constraint on phonological strings. It is the one case where the skeletal content of a string may be changed. This constraint is seen to operate in familiar cases such as French definite article behaviour, *l∅ ami* is realized as *lami*. The structure (46) has now become (48).

(48)




Phonology now applies to the result of concatenation. $N_3$ is licensed being domain-final. $N_4$ is not followed by a proper governor ($N_3$ being licensed cannot do the job) and so remains unlicensed and we derive *piesek*. Note that although $N_4$ is now a possible proper governor, $N_2$ is not empty and does not fall under the ECP.

Deriving the double diminutive follows along exactly these lines. The structure is [[[p s∅]∅k∅]∅k∅]. Using the methods discussed above, we successively derive *pies*, *piesek* and finally, *pieseczek*.[34] Thus, the difference in behaviour of the genitive singular suffix and the diminutive suffix is directly attributable to the difference in their analytic–non-analytic status. We have no need of Rubach's cyclic and post-cyclic rules. We adhere strictly to the minimalist hypothesis. Obviously, it is possible to present fragments of data that superficially appear to require something less restrictive than the model proposed here. The point I have been trying to make, with the help of the Polish data, is that it is difficult to evaluate fragmentary data because of the crucial role of the analytic–non-analytic distinction. A fairly rich array of data have already been successfully understood using this model without recourse to such devices as rule ordering or complex interactions with the morphology.

It would be surprising indeed if certain languages require a richer expressive framework than that presented here. If rule ordering and other devices are required for some languages, we would expect them to be required for all languages.

In this Section I have shown how the types of derivations illustrated in Section 1 can be carried over to morphologically complex cases. We have seen that the morphology interacts with the phonology in two ways: its domains are visible to the phonology – analytic morphology, or its domains are invisible to the phonology – non-analytic morphology. With this division we can still maintain the minimalist hypothesis of phonological derivations: events occur when their conditions are satisfied. In the following Section I will compare this view with two competing views.

## 3 Other approaches to phonological derivations

In the preceding Sections I have illustrated the notion of derivation within GP. The key aspect of these derivations is that they respect the minimalist hypothesis. There is no need to say anything more about phonological events than that they occur. There are no ordering statements. Certain events are not labelled as being restricted to a certain level of phonological structure. Events have no notion of the history of a derivation nor of its future outcome. If an event's conditions are satisfied, it takes place; if they are not, it does not. The minimalist hypothesis is bolstered by a theory of the phonology-morphology interface. Non-analytic morphology is invisible to the phonology. It involves separate listings in the psychological lexicon. Forms related by non-analytic morphology do not necessarily share common stems. They are proximate in the psychological lexicon but they do not have a common source. They are subject to lexically arbitrary, random alternations such as *write-wrote*, *ring-rang-rung*, *bring-brought*, etc.

This position is at variance with both the Bromberger and Halle (1989) (B&H) position and that of Declarative or Constraint-Based Phonology. In this Section I will argue that the GP view of phonological derivations is the most empirically adequate of this group. I will begin with a discussion of some of the arguments presented by B&H to support their view that phonological systems must include language-specific rule ordering statements.

### 3.1 Mainstream phonology: the B&H position

B&H's (1989) arguments presuppose a number of theory-internal assumptions. They are presented without justification and may be



assumed to constitute the central dogma of mainstream phonology. Some of these assumptions are listed below.

(49)

(a) 'The phonological surface representation must encode how a word is pronounced.' (p. 53)
(b) '. . . the representations required for the articulation of different words are given in the form of stipulations of discrete sound segments concatenated in the order in which they must be produced' (p. 53f.)
(c) '. . . speech sounds are composite entities constituted by complexes of binary phonetic features.' (p. 54)[35]
(d) '. . . speakers represent words in their memory by means of a code that is directly related to ways of producing linguistic sounds and that words are stored in memory as sequences of items in such a code.' (p. 56)
(e) '. . . the symbols in memory stand in a direct relation to the production of sounds.' (p. 56)
(f) 'Not all of the information required for producing a word phonetically is needed by speakers for storing the word in memory and for retrieving it when the occasion arises, because a significant fraction of that information is predictable through general rules and principles that govern the pronunciation of English.' (p. 56)
(g) '. . . memory storage and search time are at a premium in the case of language' (p. 56)

These assumptions are presented without supporting arguments. If one accepts them, it is reasonable that one will arrive at the same conclusions as B&H concerning the nature of phonological derivations. It is important to note, however, that none of them are *a priori* truths. Any or all of them could be wrong. I happen to believe that all the assumptions of (49) are incorrect. It is not surprising then, that I arrive at different conclusions concerning phonological derivations. The claim that the phonetic representation of a form is a set of instructions as to how it is pronounced may seem to be a truism, not requiring justification. I remain unconvinced. Consider the tune of a song that we know. Is it stored as a series of instructions to the vocal folds requiring them to tense or lax to such and such a degree? This seems to be far from the most obvious hypothesis. Be that as it may, it is not my intention to enter into speculations on the form of word or song storage. I only wish to suggest that B&H's assumptions presented in (49) above are not *a priori* true. Since they are unaccompanied

by any form of argumentation I feel justified in dismissing them. I will concentrate on the more substantive arguments presented in B&H.

B&H offer the interaction of rules for syllabification and rules for stress assignment in English to support their view that rules must be applied in a definite order. Since English stress assignment depends crucially on syllable structure it follows that stress rules must apply after rules of syllabification. There are a number of things that can be said about this example. Strictly speaking, it is not incompatible with the minimalist hypothesis. Any rule assigning stress that is sensitive to syllable structure will not apply to forms which contain no specifications for this structure. That the rule of stress follows the rule of syllabification follows from the fact that the former rule refers to the output of the latter, but not vice versa. This example is instructive on other grounds. B&H argue for a rule of syllabification of the basis of two claims both of which I believe to be false: (i) 'the syllable structure of an English word ... is totally predictable from the sounds that compose the word' (p. 57) and (ii) 'both syllable structure and stress are predictable: therefore they do not appear in the underlying representation' (p. 57).

B&H offer no support for these two claims. They are far from obvious. Consider the English words *beat* and *bit*. We may say that they consist of the sounds, 'b', 'i', and 't'. Since both words contain the same sounds, if B&H are correct they must have the same syllable structure, since the latter is 'totally predictable' from the former. Yet *beat* contains a branching nucleus while *bit* does not. It could be argued that the vowel of *beat* is tense while that of *bit* is lax. Therefore they do not contain the same 'sounds' and so their syllable structure need not be the same. But the tenseness of these vowels is equally predictable; the tense vowel occurs in branching nuclei while the lax one occurs in non-branching nuclei. B&H's claim (ii) states that if something is predictable it does not appear in underlying representation. Therefore *beat* and *bit* do have the same sounds, and so on. It is clear that B&H need to make a choice: is the tenseness a function of syllable structure? or is syllable structure a function of tenseness? B&H offer us no insights on this issue.

Indeed things get worse if we take claim (i) beyond English. Consider French *watt* 'watt' and *oiseau* 'bird'. Their initial portion is pronounced identically, [wa]. If claim (a) is applied to French then their initial portion ought to have the same syllable structure. It does not, cf. *le watt* vs. *l'oiseau* and *les watts* vs. *les oiseaux*. Consider also Italian pairs such as *fato* 'fate' vs. *fatto* 'fact'. Both contain the sounds 'f', 'a', 't', and 'o'. The first syllable of *fato* is open, while that of *fatto*



is closed. Such examples could be easily multiplied. Is it B&H's position that syllable structure is present in underlying representations in some languages but not in others? At best their claim is controversial and cannot be accepted in the absence of compelling arguments.

B&H's second claim is that anything which is predictable is not contained in the underlying (lexical) representation of a form. Once again this claim is not a truism: it could be false. In fact claim (b) can be viewed as the modern version of the phonemic principle which seeks to eliminate predictable/'redundant' aspects of initial phonological representations. Phonological 'contrast' was and is a crucial component of this type of view. This view is certainly the traditional one and one adhered to by a large number of practising phonologists. However, it is a view that could be erroneous and hence requires some form of justification. This is to say that other views are also possible. For example, Vergnaud and I (Kaye and Vergnaud 1990) have expressed the view that phonological representations do not form part of lexical representations as such but are rather the addressing system for lexical access. A phonological representation is the address of a lexical entry.[36] Lexical items that are phonologically similar are physically proximate in the psychological lexicon. What does 'phonologically similar' mean? We suggest that nuclear constituents play a very different role in lexical access from non-nuclear constituents. Non-nuclear constituents play a major role in lexical access while nuclei play a very minor role. The consonantal melody may be viewed as a major factor in locating a lexical form. Constituent (syllable) structure also plays a major role in this process. *Quality* and *equality* share a consonantal melody but are not involved in a lexical relationship. If constituent structure is part of a lexical addressing system then clearly it must be present in lexical representation. This view, which I have sketched in a very cursory fashion, would need considerable justification. But this is equally true of B&H's claim (b). My point is that claim (b) cannot be accepted on the face of it. In the absence of justification it cannot be used as support for the view of phonological derivations that B&H wish us to accept.

In fact, the best possible case to support B&H's view of language-specific rule ordering would be a case of two identical rules found in two languages or two dialects of one language. In dialect A the rules would apply in a given order and in dialect B they would apply in the reverse order. Indeed, B&H provide just such an example: the case of Canadian English. It is important to note that this example is the one piece of empirical evidence supplied by B&H to support their view of derivations. The state of affairs described by them (pp. 58–9) is completely incompatible

with the minimalist hypothesis and the view of derivations that I have presented in the preceding Sections. GP must predict that examples such as that of Canadian English cannot exist. This is part of the empirical content of the theory.

Briefly, the Canadian English example goes like this: a rule turns an intervocalic stop [t] or [d] into a voiced flapped stop.[37] A second rule raises the nuclei [ay] and [aw] to [ʌy] and [ʌw] before a voiceless consonant. B&H show that in most Canadian dialects the derivation proceeds as follows:

(50)

| | r[ay]t | r[ay]d | r[ay]ting | r[ay]ding |
|---|---|---|---|---|
| raising | r[ʌy]t | r[ay]d | r[ʌy]ting | r[ay]ding |
| flapping | r[ʌy]t | r[ay]d | r[ʌyD]ing | r[ayD]ing |

B&H claim that there are Canadian dialects in which the rules apply in the opposite order as shown below.

(51)

| | r[ay]t | r[ay]d | r[ay]ting | r[ay]ding |
|---|---|---|---|---|
| flapping | r[ʌy]t | r[ay]d | r[ayD]ing | r[ayD]ing |
| raising | r[ʌy]t | r[ay]d | r[ayD]ing | r[ayD]ing |

The following quotation (B&H, p. 59) accompanies this example.[38]

> It is worth noting that Principle (7) was not needed to account for the order in which the rules of syllabification and stress assignment are applied in English. That ordering did not need to be explicitly stipulated. It could be achieved by the simple proviso that a rule applies whenever conditions for its application are satisfied. Principle (7) is needed if conditions for the application of more than one rule are satisfied simultaneously. The order of application then – as the Canadian example shows – becomes a language-specific matter.

The theoretical stakes are clearly stated in this quotation. The existence of dialectal 'minimal pairs' such as the Canadian English example described above, are certainly indicative of the correctness of B&H's position on phonological derivations. The absence of these sorts of examples swings the pendulum in favour of 'the simple proviso' which corresponds to the minimalist hypothesis discussed in this article. It is important to note that the Canadian English example is the sole piece of empirical synchronic evidence advanced by B&H to support their position. In fact the data are false. There are no two dialects of Canadian English now and it is highly unlikely that the dialect exemplified in (51) ever existed.[39]



We must conclude, then, that B&H have presented no synchronic evidence to support their Principle (7). They do have another section where they claim to have diachronic evidence to support their position.

The structure of B&H's diachronic evidence for rule ordering is very simple. They make the assumption that sound changes enter the grammar as phonological rules. They show that the order in which these rules are applied in the resulting grammar is significant. Therefore, we must assume the 'psychological reality of ordered rules' (p. 61). They offer no evidence for their claim that all sound changes enter grammar as phonological rules. This is simply an assumption. There is no apparent reason why it must be true.

Their example involves the sound change known as Grimm's Law. They note that it is 'surely one of the most securely established of all "sound laws"' (p. 61). This is not to say that it is one of the most securely established phonological rules of some stage of Proto-Germanic. Grimm's law converts: $p \rightarrow f$, $t \rightarrow \theta$, $k \rightarrow x$ except after an obstruent.[40] So, all cases of earlier 'p' are converted into Proto-Germanic 'f'. But is this a phonological rule? Will a Germanic child hearing 'fo:t' analyse this form as /po:t/ in lexical representation and apply Grimm's Law *qua* phonological rule to derive the Germanic form? B&H do not explain why the Germanic child would not simply set up 'fot' as the lexical representation. There are no Germanic alternations involving $p \sim f$.[41] It could be argued that the Germanic system would have an 'f' but no 'p' and for this reason the child would be led to posit an underlying 'p'. Notice that Arabic has exactly this type of situation: 'f's are realized but there is no Arabic 'p'. To my knowledge no one has proposed deriving Arabic *foq* 'on top of' from /poq/. Perhaps there are arguments that indicate that Grimm's Law was indeed incorporated into the synchronic grammar of Germanic as a phonological rule. B&H offer none. Indeed, they do not even mention the possibility that Grimm's Law is simply a change in lexical representations. Since B&H present no evidence that Grimm's Law was incorporated into Germanic as a phonological rule, none of their ensuing discussion is relevant to the issue of the nature of phonological derivations. Principle (7) remains without empirical support, neither synchronic nor diachronic.

The phonological literature abounds with examples purporting to show the necessity of language-specific rule ordering. By and large the authors of these examples accept without question or evidence B&H's assumptions in (49). In addition, little attention is paid to the analytic–non-analytic morphological division. We have seen above that this division is essential for an understanding of

how morphology interacts with the phonology. Standard scientific practice dictates that we accept that simplest hypothesis that enjoys reasonable empirical success. Those wishing to argue for an expressively richer system must show that the simpler model is in principle incapable of treating a significant set of well-understood analyses. These analyses, of course, must be based on assumptions that are justified and justifiable. B&H do not meet these standards and I am unaware of cases in the phonological literature that do. Principle (7) remains an unjustifiable complication to phonological theory.

In this Subsection I have criticized an approach that allows for greater expressive freedom than the minimalist hypothesis adopted by GP. GP is subject to the criticism that it itself is too unrestricted and that it even approaches mainstream phonology in its (potential) expressive power. I will discuss and evaluate these claims in the following Subsection.

### 3.2 Declarative or Constraint-based Phonology

Phonology is an empirical science. That is to say, phonology is designed to make statements about the material world. Phonological theories may be evaluated on the basis of the empirical content of their claims. To the extent that the claims of a given theory coincide with experimental results, this theory may be deemed to be successful. Such empirical success may be positive: what the theory says may occur does indeed occur, or negative: what the theory says may not occur is not attested.

In like manner a theory may be criticized for being too constrained: it cannot, *in principle*, express events that are known to occur. Likewise, a theory may be criticized for being too unconstrained: unattested events are expressible within the formalism in question. Coleman's contribution to this volume (Chapter 10) contains a number of criticisms of the latter sort directed towards GP. Coleman's conclusion is that 'Government Phonology is therefore as unconstrained as the models it seeks to replace (see p. 344). This is a serious charge and it is surely worth investigating the evidence on which it is based. If GP is too unconstrained then one would expect a list of phonological phenomena permitted by GP that can be shown to be impossible or, at least unlikely, components of a phonological system. For example, a binary feature system containing twenty features allows for $2^{20}$ or 1,048,576 segments. This number can be reduced if certain feature values cannot co-occur. For example, if the features [HIGH] and [LOW] are members of the feature list and no matrix can contain the values [+ HIGH, + LOW], then we can reduce the number of segments by one-quarter to 786,432.



In a GP theory of phonological representations where 10 elements are assumed, H and L do not cooccur in a phonological expression and ATR, H and L cannot be heads, 2,304 phonological expressions can be generated. This number is still too high and recent work in GP has sought to reduce the number of expressions by reducing the number of elements.[42] It is clear that the GP representational scheme is more constrained than the feature based system described above.

These are the kinds of arguments that one would expect from Coleman to support his claims concerning the relative restrictiveness of the theories he considers. No such arguments are forthcoming. Indeed Coleman attempts to reduce GP's unary element-based system to a binary feature system. Unfortunately, Coleman's portrayal of element theory is quite inaccurate. Among other things, he equates 'charm', a property of elements, with an SPE-type feature. Charm and elements are different theoretical types as defined in GP. Coleman is of course free to define any theory he wishes, but his version of GP bears little resemblance to anything that has been proposed. Coleman's discussion contains no reference to the identity element of GP (the 'cold vowel' of early formulations). It is unclear how this would be translated into his feature theory. Similarly, the head-operator distinction is difficult to reproduce in a feature-based framework. There are phenomena involving 'head alignment' where expressions become empty headed when preceded or followed by other empty headed expressions. Once more, Coleman offers no suggestion as to how a feature-based system would express these events.

In fact, these differences between feature theory and element theory could have constituted the basis for a substantive discussion of differing theoretical claims. GP countenances head alignment while feature theory (apparently) does not. Coleman's feature-based system (see p. 357) allows for the expression of [-NASAL] as a linguistically significant class of objects; GP recognizes no such class. Sadly, Coleman does not appear to address such issues.

Much of Coleman's discussion is concerned with the weak generative power of GP. His conclusion is that GP's formal properties 'constitute a rewrite-rule formalism with a weak generative capacity at least equal to the SPE formalism,' (see p. 344). Coleman provides no examples of the nefarious effects of this purported excessive generative power. He makes much of the alleged derivational nature of GP although it is not clear what exactly would change if some of the procedural language in some GP formulations were replaced by declarative ones. Coleman lists a number of 'structural configurations and constraints' but appears unaware of any restraining properties they may have on the expressive power of a phonological

theory. This is surely a crucial issue when one is discussing theoretical restrictedness. Two of the 'structural configurations and constraints' mentioned by Coleman, are the phonological ECP and Coda Licensing. Let me try to show how these constraints make strong empirical claims about the nature of phonological events.

In earlier sections I discussed Polish yers and the ECP in some detail. Rule-based systems such as Rubach's require rules (or their declarative counterparts) as in (40) repeated below.

(52)
Lower (cyclic)
[+syll, +high, −tense] → [−high]/____$C_0$[+syll, +high, −tense]
Yer Deletion (postcyclic)
[+syll, +high, −tense) →∅

The Lower rule converts a yer to [ɛ] when followed by another yer. Yer Deletion removes all unlowered yers. Now consider the Polish alternations *pies* 'dog nom.sg.'; *psa* 'gen.sg.' and *dno* 'bottom nom.sg.'; *den* 'gen.pl.' Rubach's analysis requires that the forms in question be represented as: p☺s☺ − p☺sa; d☺no − d☺n☺, respectively. Since yer is a vowel and since Rubach's theory is not constrained by a principle like Coda Licensing, he is required to stipulate that one of the forms of the nominative singular and the genitive plural is a yer. This means that it is a *contingent* fact that these suffixes behave the way they do. Notice that it just so happens that the nominative singular and genitive plural are all and only the apparently empty suffixes in nominal paradigms. For Rubach this must be an accident. His theory predicts the possibility of some truly empty suffix that does not contain a yer. In such a case we would have p☺s yielding *ps* alongside of p☺s☺ which yields *pies*. This, of course, does not happen. The Phonological ECP and Coda Licensing combine to make the Polish facts *necessary* rather than *contingent*. Coda licensing excludes p☺s as a possible form. It requires p☺s☺. Since yers are really empty categories the ECP will then provide the correct interpretation of these representations. The only contingent aspect of the analysis is how Polish realizes unlicensed empty nuclei.

It seems to me that these are the types of issues that one needs to consider in evaluating theoretical restrictiveness. They are conspicuously lacking in Coleman's discussion. Since I am concerned principally with derivations here, it would be worth comparing Coleman's view of rule-ordering effects with that of GP. I discussed the Canadian English example cited by B&H as evidence for language-specific rule ordering. I noted that GP could not accommodate the data cited by B&H. That is, if the Canadian English facts stand as



correct, there is a serious flaw in the underlying principle of GP. It was this concern that led me to investigate the status of these 'data' and conclude that they are flawed. Since Coleman's contention is that GP is much less constrained than non-derivational models, it would be interesting to see if his approach has any difficulty 'accounting for' the spurious Canadian English data. The relevant 'data' given above in (50) and (51) are repeated below for convenience of reference.

(53)

| | r[ay]t | r[ay]d | r[ay]ting | r[ay]ding |
|---|---|---|---|---|
| raising | r[ʌy]t | r[ay]d | r[ʌy]ting | r[ay]ding |
| flapping | r[ʌy]t | r[ay]d | r[ʌyD]ing | r[ayD]ing |

(54)

| | r[ay]t | r[ay]d | r[ay]ting | r[ay]ding |
|---|---|---|---|---|
| flapping | r[ʌy]t | r[ay]d | r[ayD]ing | r[ayD]ing |
| raising | r[ʌy]t | r[ay]d | r[ayD]ing | r[ayD]ing |

(53) and (54) are given with their rule-based derivations. Traditionally, (53) is called 'Dialect A', and (54), 'Dialect B'. Coleman (see pp. 362–3) unfortunately reverses the usual nomenclature and calls (53) 'Dialect B' and (54), 'Dialect A'. I will simply refer to them by their example numbers for clarity. In speaking of (54) Coleman states, 'it is claimed, there is a rule ("flapping") which transforms intervocalic /t/and /d/ into [ɾ] [our "D"/JK]. Thus, both "writing" and "riding" are pronounced [rayɾiŋ].' Coleman then concludes (see pp. 362–3), 'Firstly, since in dialect A [sic] raising is not manifested, there is no evidence at all that the rule even exists in the grammars of speakers of this dialect.'

A cursory inspection of the first two columns of (53) and (54) shows that Coleman's conclusion is far from apparent. It is difficult to speak with assurance about a dialect which never existed but Joos' original claim was that the behaviour of speakers of the two dialects converged in all cases where /t/ did not undergo 'flapping'. So the mythical speakers of (54) did distinguish 'right' r[ʌy]t from 'ride' r[ay]d. Furthermore the 'manifestation' of raising was completely general when the voiceless consonant following the diphthong was not /t/. Thus, 'type' = [tʌyp], 'typing' = [tʌypiŋ], 'bribe' = br[ay]b and 'bribing' = [braybiŋ]. These forms are all valid for Central Canadian English and would be valid for (54) if that dialect existed. Thus, we must conclude that Coleman's conclusion is false or his phrase 'raising is not manifested' has an unusual interpretation.

Coleman's difficulty with phonological data aside, it is apparent that his theory of derivations is not constrained in any real sense. That is, it makes no empirical claims about the material world. This impression is reinforced as one reads further into Coleman's work. Our old SPE friends 'Tri-syllabic shortening', 'Velar softening', '-ic- shortening', etc. all find a home in Coleman's non-derivational theory. Alternations, the likes of which have not been seen since Lightner's time reappear: '*drink* vs. *drench*, *church* vs. *kirk*, *bridge* vs. *brig*' (see p. 376). All these alternations are grist for Coleman's non-derivational mill. In sum, calling a theory 'truly restrictive' does not make it so. If one searches Coleman for specific empirical claims about what may or may not be a phonological event, one searches in vain.

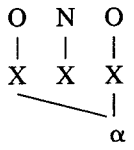
Coleman may have some inkling of the situation when he states, 'Accepting that my proposals in this area may be less than convincing than my criticisms, however, I should point out from the start that the criticisms do not stand or fall by the success or failure of my attempt to propose an alternative' (see p. 336). I fully agree that GP, like any other serious theory, could be significantly improved. I hope that I would be the first to applaud such an improvement by Coleman or anyone else. It is my conviction, however, that Coleman's criticisms are hard to accept since they offer no indications as to how any phonological theory could be improved. Indeed, Declarative Lexical Phonology – as far as one can ascertain from Coleman's contribution to this volume – does not appear to offer any fundamentally new insights into the nature of phonological phenomena. Coleman states that 'It is a matter of speculation and belief as to whether these principles [of GP/JK] combine to define the phonology of even a single language' (see p. 344). There is no need for speculation. GP is not a complete theory. There remains work to be done. There exists no definition of even a single language. This is hardly surprising nor a peculiar property of GP. I am unaware of any definition of even a single language in any theoretical framework. Work in Government Phonology will continue. There are a wide range of interesting and unresolved problems that await treatment. I remain confident that our knowledge of phonological phenomena will increase. I hope this Chapter is a step in this direction.

## Notes

1. For discussions of this theoretical approach see Kaye et al. (1985, 1990), Charette (1991), Kaye (1990a) and the references therein.
2. See, for example, Coleman (this volume).



3. In fact I would prefer the term *event* rather than rule or process. The use of this term here would make the comparison of positions somewhat more awkward to express. I use *process* but attach no theoretical importance to it.
4. I am assuming familiarity with the theory of elements used in GP. The reader is referred to the article by Harris and Lindsey in this volume for further information.
5. See Harris and Lindsey (this volume) for details of GP representations.
6. See Charette and Kaye (in preparation) for a revised version of element theory. The original version is found in Kaye et al. (1985).
7. For a particularly spectacular example of 'resyllabification' see Mohanan (1989).
8. See Kaye (1990a), KLV (1990), Brockhaus (this volume) for discussion.
9. See Cyran (1992), Gibb (1992), Heo (in preparation) for examples of this phenomenon in Irish, Finnish and Korean, respectively.
10. See Charette (1991), Kaye (1990b), Gussmann and Kaye (1992) among others for discussion of the phonological ECP.
11. By 'inter-onset domain' I have the following structure in mind. The nucleus sandwiched between the onsets is p-licensed.




12. This process may well take place in the lexicon as part of the generative process of word formation. I will return to this point in Section 2.
13. *Le Petit Robert* offers **sele** rather than **sɛle** as the pronunciation of the infinitive. Many French speakers use the pronunciation presented here. There is general agreement on **sɛl** as the pronunciation of 'selle'.
14. The following Section presents work done jointly with Jean-Roger Verngaud. An oral version of this theory was presented in Kaye and Vergnaud (1990). I follow the basic tenets of that presentation although I may differ somewhat from it in detail. Nothing momentous hangs on these differences.
15. That is, a sequence of onsets with an intervening licensed empty nucleus.


16. Aside from some proper names such as 'Sims', etc.
17. See Prunet (1986: 148ff.) for details.

18. Strictly speaking $\varphi$ is not a function but a family of functions. A function should return a unique result but given that some phonological processes are optional, the application of $\varphi$ does not always return a unique result. It is therefore not a function in the mathematical sense. Rather it represents a family of functions $\varphi'$, $\varphi''$, $\varphi'''$, etc. where each $\varphi$ represents a unique solution.
19. See Kaye (1990a).
20. In fact, empty nuclei are found between flanking onsets that may contract a governing relation where the first onset could govern the second. Empty nuclei occur in forms like, *bæt Ø̲l* 'battle', *kæt Ø̲n* 'cotton', *æt Ø̲las* 'atlas', etc. See Heo (in preparation) for similar effects in Korean.
21. The domain in question is *seeped* and not *-ed*.
22. See Prunet (1986) for arguments.
23. I mean *strict cyclicity* in its original sense as proposed by Chomsky (1973) and applied to phonology by Kean (1974). It has subsequently been used in a very different sense, for example, in lexical phonology.
24. A detectable pseudo-sequence is one that could not be analysed as a rhymal consonant followed by an onset. *vd* is a detectable pseudo-sequence; *ft* is not a detectable pseudo-sequence (cf. *bluffed* and *soft*). Note that any sequence following a branching nucleus is a detectable pseudo-sequence. The binarity theorem requires such a form to be analysed as $v{:}C_1\ Ø̲C_2$.
25. In fact, (38) is not 100 per cent true. There are analytic past tense forms like *sold*, *told* and *dreamt*. Interestingly, all these forms correspond to words with no obvious internal morphological structure. Alongside of *sold* and *told*, we have *old* and *gold*; *dreamt* is phonologically similar to *unkempt* and has a frequent regular past tense, *dreamed*. See Harris (1990c) for discussion of the *sold-told* type cases.
26. The fact that *keep* and *kept* have a certain phonological resemblance does not mean that they are derived from a common source; *write* and *wrote* also resemble each other and yet one rarely claims that they are derived from the same stem. In the Kaye–Vergnaud model of lexical access, there is a reason for these forms to be similar. We claim that phonological representations are *addresses* for lexical items (or perhaps addresses for pointers to lexical items). We assume that phonologically similar forms are physically proximate in the psychological lexicon. With these assumptions it is possible to develop a model whereby it is advantageous for morphologically related forms like *keep-kept* to be phonologically similar. It should be kept in mind that this similarity is *not* based on derivations from a common stem.
27. We know this suffix is analytic because of the behaviour of the velar nasal, ŋ. Within a domain, English ŋ can never be followed by anything other than a velar stop: *k* or *g* if the following nucleus is not p-licensed. Cf. *finger* (which has no internal domains) vs. *sing̲er*, which does ([[sing Ø̲]er Ø̲]). We know that *-ing* is analytic because of *sing̲in* and not * *singiŋ*



28. Not entirely arbitrary, of course. If we assume that look-up is more efficient, in some sense than computing and if we assume that morphological irregularity does have some overhead, e.g. learning the forms, then it follows that we would expect the morphologically irregular forms to be relatively frequent to exploit their greater computational efficiency.
29. This example is taken from Prunet (1986).
30. This discussion is taken from Gussmann and Kaye (1992). See that article for more detailed discussion of the Polish facts and analysis.
31. This analysis can be criticised on a number of other grounds. See Gussmann and Kaye (1992) for discussion.
32. In fact the nominative singular form of 'dog' is *pies* and not *pes*. The analysis of Polish palatalization appears in Gussmann and Kaye (in preparation).
33. Polish also has a non-analytic diminutive suffix, *-ik*, as in *tomik* 'little volume'. See Gussmann and Kaye (1992) for a discussion of these two suffixes.
34. The realization of *k* as *cz* (= č) does not concern us here. See Gussmann and Kaye (in preparation) for discussion.
35. This may represent a departure from the SPE position (Chomsky and Halle (1968)). 'The distinction between the phonological and phonetic matrices must be kept clearly in mind. In the case of the phonetic matrix, each row corresponds to a phonetic feature, physically defined, from a predetermined initial set. The entry occupying a particular square of the matrix will be *an integer specifying the degree to which the segment in question is characterized by the corresponding property*.' [emphasis mine/JK] Chomsky and Halle (1968: 165). Thus, phonetic features are not considered binary in SPE.
36. Or perhaps, the address of a pointer to that entry.
37. I make no claims for the accuracy of B&H's description. Indeed, it is inaccurate. See Chambers (1973) for details. It will be seen that their description faces more important problems than just accuracy.
38. The principle (7) referred to in the quotation below is the one quoted on page 1 above. It states,

> Phonological rules are ordered with respect to one another. A phonological rule R does not apply necessarily to the underlying representation; rather, R applies to the derived representation that results from the application of each applicable rule preceding R in the order of the rules.

39. Arguments against the existence of the two Canadian English dialects are found in Kaye (1990). The dialect whose data are found in (51) is based on a single report in 1942. Doubt as to its existence is based on the fact that all speakers of this dialect would have died out before the age of 60.
40. B&H express this as an arbitrary fact. In GP terms, Grimm's Law does not take place when the segment in question is a governor of an

obstruent. See Harris and Kaye (1990) for discussion of a similar case in Modern English.

41. Proto-Germanic 'p' did not occur following an obstruent.

42. This is the objective of Charette and Kaye, in preparation.

Chapter 10

# Declarative lexical phonology

*John Coleman*

## 1 Constraining phonological theory

The many generative phonological theories which have arisen in the wake of *The Sound Pattern of English* (SPE) (Chomsky and Halle 1968), several of which are discussed in the other chapters to this volume, share three fundamental assumptions:

First, each attempts to allow only the phonologies of natural human languages to be defined. In other words, there are some languages, which linguists call unnatural or impossible human languages, which the theories aim to exclude. The inclusion of a particular phonology among the set of natural human phonologies is characterized by the use of constraints of one kind or another, a phonological rule being a common way of expressing such constraints.

Second, each posits the existence of lexical representations of words which are distinct from their surface form. The surface forms of words are related to their lexical encoding by certain alterations to phonological representations: insertion, deletion, or substitution of individual features or entire segments.

Third, the phonological rules which define these operations may be cascaded, that is, applied in a particular sequence one after the other, possibly more than once, as exemplified in Table 10.1. Such cascading of rules gives rise to the notion of the *derivation* or *derivational history* of the surface form of an utterance from the lexical forms of the words in the utterance.

These three characteristics of generative phonology are the historical remains of a theory of grammar that has been largely superseded, the 'Standard' or 'Aspects' theory. Its pretensions to psycholinguistic plausibility have been questioned and found wanting.