The restructuring of Early English morphology: Theoretical foundations and some consequences

Kristó László

2005
Introduction

“This music crept by me upon the waters’
And along the Strand, up Queen Victoria Street.
O City city, I can sometimes hear
Beside a public bar in Lower Thames Street,
The pleasant wheeping of a mandoline
And a clatter and a chatter from within
Where fishmen lounge at noon: where the walls
Of Magnus Martyr bold
Inexplicable splendour of Ionian white and gold.

T. S. Eliot

The present thesis aims at presenting a plausible model of how the morphological structure of English was restructured between the periods known as Old English and Middle English, with special regard to regular inflection. The analysis offered here is based on the theoretical framework known as CVCV (or Strict CV) Theory, an offspring of classical Government Phonology. Indeed, the entire analysis relies so heavily on this theory that it would hardly be possible to come up with it without this theory. This statement may require some notes.

Historical linguists have been rather suspicious about “theories”; they (or, we?) prefer to base our investigation on “hard facts” rather than theoretical models, which come and go. Nonetheless, two things must be borne in mind. First, historical linguistics itself is based on theories, the most notable one being the Neogrammarian theory of linguistic change and reconstruction. Though this particular model is itself a theory, it has become so deeply rooted in historical study that we often take its claims for granted. Nevertheless, it is but a theory — even if a spectacularly successful one. We (historians) rely on “facts” established by this framework, often without questioning them. Chapter 1 is largely devoted to this theory; the final part of Chapter 6 gives an analysis of “Middle English Pre-Cluster Shortening”, in which I intend to show that it is a theoretical construct of the Neogrammarian theory rather than a real phonological change. Furthermore, even in case we have attested languages at our
disposal, it should not be forgotten that the overwhelming majority of our texts are written ones, which need interpretation — an enormously difficult task. How we interpret written sources is to a large extent a function of our theoretical assumptions (or even prejudices). The anti-theory bias historians tend to show in many cases is, therefore, unfounded in one very specific sense: if we believe that historical linguistics is a science, we must accept the “hard fact” that no science is possible without theories — whether linguistic, biological, physical, or whatever else.

Second, theories predict things (or at least they should). They also predict something about how and why languages may change. For this reason, general theories must not be denied the opportunity to show that they may be useful in interpreting historical data, or re-interpreting them. I hope that my thesis will support this point of view.

Yet, this is not all to it. Theoretical linguistics in the twentieth century was quite anti-historical in its attitude, too, probably because it overinterpreted Saussure’s claim that synchrony is primary over diachrony. Saussure, however, never said that diachrony was “unscientific” (he was an excellent historical linguist, in fact). Fortunately, recent decades have seen a revival of interest among theoreticians in matters historical: the number of theoretical articles, conference presentations and books featuring historical arguments is on the increase, and this is particularly true for the general theoretical framework I adopt here. I do believe that one cannot do serious linguistics unless adopting a panchronic perspective: change is rooted in (synchronic) variation — and, conversely, much of synchronic variation is rooted in history: all synchronic states are the products of history. It is my sincere hope that this attitude will prevail.

Chapters 2, 3, and 4 provide the rest of the necessary theoretical background. It is especially Chapter 4 which discusses issues of high relevance, notably, the nature of morphological effects on the phonology, the typological classification of affixation into fusional and agglutinating, as well as lexical storage and its consequences for word structure.

Chapters 5 and 6 present a detailed analysis of OE and ME morphology from a morphophonemic point of view. These are, alongside Chapter 4, the central ones in the thesis.

The time has now come to fulfill a pleasant duty — to express my words of gratitude to those who have, directly or indirectly, played a role in forming this thesis (as well as my views). First of all, I wish to thank my professors who introduced me to phonology, morphology, and historical linguistics; they are (in “chronological” order), László Varga, Edit Dési, Mihály Péter, Ádám Nádasdy, Péter Siptár, Péter Szigetvári, Miklós Törkenczy, Géza Wernke, Marianne Bakró-Nagy, Veronika Kniezsa and John Harris. Of these professors,
special thanks are due to Ádám Nádasdy, who has been my thesis supervisor: his constant encouragement and support, as well as his careful reading of this paper, has been invaluable; and to László Varga, our English Linguistics PhD Programme director. I would especially like to thank both of them for their understanding. Second, all my thanks go to my colleagues and friends who have offered help concerning my work, including Katalin Balogné Bérces (for her advice and encouragement as well as reading parts of this thesis), András Cser (for reading an earlier manuscript which was to become the core of Chapter 1 as well as for answering my questions on Latin), Csaba Csides (who has been discussing phonology with me for many years), Péter Rebrus (who helped me a lot during my PhD years with his insights and comments), Monik Charette (who helped me to clarify several essential points and raised intriguing questions during our long London talks), Andrea Nagy (for helping me with Old English data as well as her personal encouragement as one of my best friends) and to Tobias Scheer (who has played a decisive part in forming my views both on phonology and on how linguistic science should be done, which, I assume, is obvious for anyone who has read this thesis; also, personal discussions with him in Budapest and Nice have proved to be invaluable for me). I sincerely hope I haven’t forgotten to mention anyone; if I have, I hope they will forgive me. It goes without saying that none of these people is responsible for any of the remaining errors in this thesis.

I have also learnt a lot from my teaching experience. I would like to say thankyou to all my students, who have improved my understanding in linguistics and teaching, either because they were interested in linguistics or because they weren’t. It would be a hopeless enterprise to enumerate all of those whom I owe a lot.

I would also like to thank my friends who, though not professionally, helped me greatly personally speaking; special thanks are due to Éva Kellermann, Andrea Nagy, and Károly Pintér, who have never ceased to try and make me write this paper.

However, the biggest thankyou goes to my family: my sister and especially my parents, who have, since my childhood, respected my own intellectual spheres and ambitions. Their understanding and unfailing support in all areas of life has been more than vital to me. Köszönöm, Anya, köszönöm, Apa.

Finally, much of the research I have done was made possible by the Hungarian State Eötvös Scholarship granted to me for three months’ scholarly activity at SOAS, University of London, during the autumn term of 2004.
NOTES ON FORMALISM, TERMINOLOGY, & ABBREVIATIONS

1 Early English I use the term “Early English” in a technical sense, comprising both Old and Middle English, as opposed to Modern English which, as universally accepted by historians, starts at around 1500 A.D. “Early English” is, then, a cover term for everything non-modern, but not earlier than Old English, either.

2 Phonetic and orthographic notations and formalism
In general, I use *italics* when quoting data in their traditional orthography. The same goes for editorially marked OE and ME forms. If the fact that the form/symbol is an orthographic one needs special emphasis, I follow the usual practice, e.g., OE <niht> ‘night’.

Phonetic forms are usually typed *bold*, using IPA symbols. I only use bracketing when inevitable. Square brackets are used in general; slants are only used when I wish to emphasise that I talk about abstract (phonemic or morphophonemic) representations.

As usual in historical linguistic discussions, the asterisk is used to denote reconstructed forms, e.g., PGmc *stainaz* ‘stone’. To indicate ill-formedness, a double asterisk is used, e.g., OE **wordu* ‘words’. In those chapters (2—4) where I discuss theoretical rather than historical issues, I use the asterisk according to the usual non-historical practice, i.e., to indicate ill-formedness.

In formal Government Phonology representations, I use \( \phi \) to indicate Licensing, and \( \triangleright \) to indicate Governing relations, so A \( \phi \) B = ‘B licenses A’, and A \( \triangleright \) B = ‘B governs A’. Infrasegmental Government is indicated by thick arrows.

3 Abbreviations for languages

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<tr>
<th>Language</th>
<th>Abbreviation</th>
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<td>AN</td>
<td>Anglo-Norman</td>
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<td>CL</td>
<td>Classical Latin</td>
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<td>E</td>
<td>English</td>
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<td>early Middle English</td>
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1

Sound laws, reconstruction, and cognateness

This chapter discusses some of the basic concepts and techniques of historical linguistics, notably, the concepts of regularity and cognateness as well as their relationship to another central concept: sound change. My aim is to provide an answer to the following questions:

1. What makes linguistic (historical) comparison possible? What can “legally” be compared? That is, what do we talk about when we talk about cognates?
2. What is sound change, and what are sound laws?
3. What is the role of sound laws and the Regularity Hypothesis in providing answers to the above questions?

1.1. [Preliminaries]

The birth of modern linguistic science means the birth of historical linguistics, becoming mature in its methods and its theoretical apparatus in the second half of the nineteenth century. The basic methods devised then are still in use today, and continue to form a fundamental underpinning of linguistics in many ways. Several of our central theoretical constructs, such as the phoneme, have their origins in 19th century historical linguistics (see Anderson 1985:38, 66 for the origins of the term phoneme). It is in no way surprising, then, that several findings of the field are taken for granted by non-historians (and, alas, very often by historians, too). A good example is provided by sound laws, which are treated as empirical “facts” in most (historical as well as theoretical) discussions. Reading the literature, one cannot fail to notice that most authors accept, say, Verner’s Law as a historical event, something that “is there” — just as the African elephant “is there”. Needless to say, this is far from being the truth: sound laws are theoretical constructs, and they may or may not be “true”: their function is to account for certain (empirically observed) facts, rather than being the facts themselves. Of course, as it regularly happens in science, theoretical constructs are used as “facts” (once they are accepted
as explanations) for further analyses; but it is nonetheless necessary to have in mind that they aren’t facts in the same way as African elephants are.

1.2. Relatedness

One of the central problems of historical linguistics is the notion of relatedness, being the subject of linguistic comparison (internal or comparative: see more on this below). The term is used for languages (one can say that English and German are related), as well as for particular items (lexical items, mostly) in related languages (so, for instance, we can say that English team, swim, break, us and German Zaum ‘team of horses’, schwimmen, brechen, uns, respectively, are related). The term can be used in a more loose sense, too: e.g., we can claim that English search, vanity, price and French chercher ‘to look for’, vanité, prix are related, although this is more shaky, because, in a certain (and — for linguists — a fairly “obvious”) sense, these words are not related in the same way as the English and German pairs are. But what is this “obvious” sense?

A fundamental goal of 19th century linguistics was to provide an answer to this question, i.e., to define the notion of relatedness in a scientific way. The culmination of the period was the activity of the group of scholars, centred around Leipzig in the last decades of the century, which came to be known as Neogrammarians (German Junggrammatiker, i.e., ‘young grammarians’). The group included linguists such as Hermann Paul, Karl Brugmann, August Leskien, etc.¹ The Neogrammarians synthesised the achievements of the previous century, and devised a full-fledged theory of historical linguistics. The great works of the period, in which the theory is applied, include Brugmann and Delbrück’s monumental Vergleichende Grammatik (1886-1900). The phonetic foundations of the theory are laid down in Sievers (1876 [1881]), and the theory of language change itself is described in what is generally regarded as a crowning point of Neogrammarian scholarship, Hermann Paul’s Prinzipien der Sprachgeschichte (1880).

In spite of the fact that the central tenet of Neogrammarian theory, viz. the Regularity Hypothesis (see below) received substantial criticism from the earliest times on, it has come to stay, still being used as the fundamental underpinning of all historical linguistics. The reason for this is, as Jankowsky (1990:234) writes:

¹ The term Neogrammarian is usually used with reference not only to these scholars, i.e., the Neogrammarian circle in the strict sense, but to refer to the school of thought they initiated as well as their followers. See Jankowsky (1990).
(...) as a working hypothesis the regularity principle has been in use for over a century now and is relied upon by Neogrammarians and their followers as well as by their opponents. Where it is employed consistently, the results have been not only extremely useful, but also crucially important for the investigations of the historical linguist at large. Its value as a working hypothesis is fully recognised by Johannes Schmidt, who very often took issue with the theoretical position of the Neogrammarians, but of whom Wheeler correctly said that in “his worthy practice, we find no essential point of difference from that of the neogrammarians” (Wheeler 1887:42).

In the twentieth century, perhaps the most serious criticism of the Regularity Hypothesis came from the theory of lexical diffusion, which (at first sight at least) seems to claim just the opposite of what the Neogrammarians claim; see, e.g., Wang (1969, 1977), Chen and Wang (1975), Chen (1977). Yet, the Regularity Hypothesis — as Jankowsky observes — is still alive and well.

The heart and crux of Neogrammarian theory is the notion of sound laws (Lautgesetze), probably the most essential theoretical linguistic construct of the 19th century, comparable to the early 20th century (Praguian) notion of the phoneme. Although the concept of sound laws is well-known, it is perhaps useful to sum up briefly why they are so important and, to start with, what they are at all.

If one observes the vocabulary of natural languages, one will notice that certain groups of languages show similarities not shared by others (where “similarity” means that words with identical or reasonably close meanings have a similar, or even identical, phonetic shape). Consider, for example, the data in (1):

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2 See also Lass (1997:139-143), who considers diffusion and competition to be a “pseudo-problem” for regularity.
Some “similar” items in four Slavonic languages

<table>
<thead>
<tr>
<th>Russian</th>
<th>Czech</th>
<th>Slovene</th>
<th>Bulgarian</th>
<th>GLOSS</th>
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<tbody>
<tr>
<td>brat</td>
<td>bratr</td>
<td>brat</td>
<td>brat</td>
<td>‘brother’</td>
</tr>
<tr>
<td>tr’i</td>
<td>tři</td>
<td>tri</td>
<td>tri</td>
<td>‘3’</td>
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<tr>
<td>jazyk</td>
<td>jazyk</td>
<td>jezik</td>
<td>ezik</td>
<td>‘tongue’</td>
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<tr>
<td>d’en’</td>
<td>den</td>
<td>dan</td>
<td>den</td>
<td>‘day’</td>
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<tr>
<td>ruka</td>
<td>ruka</td>
<td>roka</td>
<td>răka</td>
<td>‘hand’</td>
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<td>ty</td>
<td>ty</td>
<td>ti</td>
<td>ti</td>
<td>‘thou’</td>
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<tr>
<td>na</td>
<td>na</td>
<td>na</td>
<td>na</td>
<td>‘on, at’</td>
</tr>
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There can be three possible reasons for such similarities (cf. Lass 1997:105): (a) common origin, (b) borrowing, (c) chance. Of these, (c) is rare and anyway not particularly interesting, though it can be misleading at first sight, and must clearly be identified. (More on this below.) Borrowing and common origin are both equally general sources of similarities, and the task is to find a principled manner to distinguish the two. As a start, we normally use the observation that items in the core vocabulary, e.g., names of body parts, basic numerals, pronouns, etc., tend to be stable and resistant to borrowing: therefore, similarities in this section of the lexicon are likely to be the result of common origin. The next step is to examine the differences between the items in question. Perfect identity, though it exists, is relatively rare (cf. na ‘on, at’ in (1)), i.e., in most cases there will be some differences.

A great achievement of historical linguistics is the discovery that these differences are not random, but are statable in terms of regular sound correspondences. For example, Russian \( y \) (phonetically \([\text{u}]\)) regularly corresponds to Czech, Slovene, and Bulgarian \([i]\). That is to say, words which contain an \([\text{u}]\) in Russian have an \([i]\) in the other languages under scrutiny. The existence of regular sound correspondences makes historical investigation, and reconstruction in particular, possible.

The aim of reconstruction is to prove relatedness, i.e., common origin (as opposed to chance similarity or borrowing). One can only legitimately claim that two languages are related if and only if regular sound correspondences can be set up: their existence is the most important initial proof of relatedness. (I use the word “initial” because, as I will show shortly, the mere existence of correspondences is not quite sufficient: derivations are essential.) There

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3 The apostrophe in the Russian data indicates the palatalisation of the preceding consonant. Bulgarian \( ā = [\text{a}] \). I have not indicated stress placement for the sake of simplicity, but note that it is lexical in all of these languages except Czech, where it is initial.

4 Of the examples in (1), all are words of common origin, but, of course, one can easily find examples for borrowing within the Slavonic family. So, for example, the 19th-century Czech coinage \( \text{vlak} \) ‘train’ found its way into several Slavonic languages, cf. Slovene/Slovak/Croatian \( \text{vlak} \).

5 Note that in Czech orthography \(<\text{y}> = [i]\). May I note as well that the reverse of this correspondence does not hold, i.e., an [i] in, say, Czech can correspond to a Ru [i], too, cf. the forms for ‘3’.
are two basic reconstructive techniques that are widely used as fundamental tools in historical linguistics: Comparative Reconstruction (CR) and Internal Reconstruction (IR). Let us now turn our attention to these techniques.

1.3. [Comparative Reconstruction and cognateness]

Comparative Reconstruction is the method used when related languages or dialects are involved. It is the existence of regular correspondences that makes CR possible. The method is a rather simple mathematical operation in its first stage, as pointed out by Lass. I will use his example here. Take three related languages X, Y, and Z; let x, y, and z, respectively, be regularly corresponding units (sounds, for our purposes) in these languages. We can create a set C = {x,y,z}, where C stands for “cognateness”. This, of course, is a simple relation, which is symmetrical (xCy ⊃ yCx) as well as transitive ((xCy and yCz) ⊃ xCz). So far C is nothing but a label to give a name to the set; as a result, we have not yet made any historical statement: all we have is a static pattern. We can, however, move on to the second step and assume that C is not a label but an entity, i.e., a physically existing object which is related to x, y, and z historically: it is their ancestor. The relation “ancestor of”, of course, is neither symmetrical nor transitive. We can conceptualize the relation in the form of a tree as in (2):

![Diagram](2)

If C is a physically existing object, it is possible to assign physical properties to it. In case x, y, z are sounds, these will be, of course, phonetic properties. This is the final stage of reconstruction (at least on this level: we can move one level up and reconstruct morphemes, etc.).

Let us take a specific example. Standard Portuguese intervocalic d regularly corresponds to ð in Castilian Spanish: Portuguese cantada, lado, cidade (‘sung-Fem’, ‘side’, ‘city’) correspond to Castilian canta[ð]a, la[ð]o, ciu[ð]a[ð], respectively. We can set up C = {d, ð}, and assign C historical-ontological status, which requires (or makes it possible) to assign a phonetic value to it. Since we know that intervocalic position is a typical lenition site,

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6 1993:161f
we assume that $C = \mathbf{d}$ in Proto-Western-Romance, the ancestor of both languages.\footnote{This stage itself represents a lenited state of intervocalic stops, since the ultimate source of these cognate items is VL $^{*}\text{cantata},^{*}\text{latu},^{*}\text{civitate}$, with an intervocalic voiceless stop.} We can represent this as in (3):

$$
\begin{array}{c}
(3) \\
\text{*d} \\
\text{d} \quad \text{ð}
\end{array}
$$

This is simple so far: we assume a change in one language. The projected proto-character (sound) is thus represented in a presumably unchanged form in one descendant. It may also happen, however, that an assumed proto-character has undergone some change in both languages. For example, word-initial VL $^{*}\text{v}$ ($<$ CL $\mathbf{w}$) yields Portuguese $\text{v}$ but Spanish $\text{b}$, cf. La $\text{vōs}$ ‘you’ $>$ Po $\text{vos}$, Sp $[b]\text{os(otros)}$, La $\text{vīta(m)}$ ‘life’ $>$ Po $\text{vida}$, Sp $[b]\text{ida}$, etc. We can draw a tree similar to the one in (3), but, of course, with a different character in each place:

$$
\begin{array}{c}
(4) \\
\text{*w} \\
\text{v} \quad \text{b}
\end{array}
$$

Up to this point, it may appear that the reconstruction of a proto-character is, in fact, superfluous: it might suffice to simply set up the correspondences, as in (5):

$$
\begin{array}{c}
(5) \quad (a) \ (\text{For (3)}): \quad C_1 = \{\text{Po d, Sp ð}\} \ / \text{if intervocalic in Portuguese} \\
\quad (b) \ (\text{For (4)}): \quad C_2 = \{\text{Po v, Sp b}\} \ / \text{word-initially}
\end{array}
$$

As we have the regular correspondences, we might as well stop here and say that relatedness has been proved. In other words, why do we need to move on to the second (and ultimately, to the third) step of reconstruction described above? Isn’t the fact that we can set up regular correspondences sufficient to prove relatedness? This question is not quite explicitly asked in the literature, so it is worth taking a look. To set up correspondences is itself not sufficient for a number of reasons.
First, relatedness means common origin — in other words, related languages were at one point one and the same language, and the differences are due to divergent developments within the ancient linguistic community. If this is the case (which is universally accepted), what are the reasons why these correspondences exist? How have they come about? The proto-language clearly had a single sound, but then, what was it? By simply setting up correspondences we do not say anything at all about that. Recall that relatedness is a historical statement: and it is impossible to do history unless we reconstruct a proto-character.

Second, borrowing often results in multiple correspondences. For instance, English parent, price, peace may be taken to correspond to French père, pied, paix, respectively. One may conclude that there exists a correspondence set such as the one in (6):

\[(6) \quad C_1 = \{E \text{ p}, F \text{ p}\}\]

But we find another set, too, cf. E father, foot, fish(er) vs. F père, pied, pêch(eur), resp. This is depicted in (7):

\[(7) \quad C_2 = \{E \text{ f}, F \text{ p}\}\]

Which is the “real” one? Obviously, it is (7): we know that words like parent, price, peace are loans from French, hence do not prove relatedness. But by simply setting up a correspondence, how do we tell the difference? Again, a reconstructive (historical) approach is needed. Worse still, borrowing can give rise to correspondences between otherwise unrelated languages, too: for example, Hungarian sors, pestis, vírus etc. (where $<s> = f$) derive from Latin sors, pestis, vīrus, resp., (where the Latin words of course, have a $s$)$^9$, so one might be tempted to set up a correspondence:

\[(8) \quad C = \{\text{Hu } f, \text{ La } s\}\]

Now, of course, we can say that such correspondences are irrelevant because they are obvious borrowings (they are highly cultural items, do not belong to the core vocabulary, etc.), but this is not the point: technically speaking, the ultimate proof of relatedness is the existence of regular correspondences, deriving from a common historical source. We will come back to this question shortly.

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$^9$ Data from Cser (2003:11).
Third, correspondences are rarely biunique and unambiguous. For example, while it is true that Portuguese word-initial \( v \) regularly corresponds to Spanish \( b \), the reverse does not hold, because a Spanish initial \( b \) can correspond to Portuguese \( b \), too, cf. Po beber ‘to drink’, baixo ‘low’, beijar ‘to kiss’, bom ‘good’ = Sp beber, bajo, besar, bueno. We have, then, two correspondences in word-initial position:

\[
\begin{align*}
(9) \quad & (a) \quad C_1 = \{ \text{Po } v, \text{ Sp } b \} \\
& (b) \quad C_2 = \{ \text{Po } b, \text{ Sp } b \}
\end{align*}
\]

But why? We can give no answer without reconstruction, i.e., without a historical derivation, as it were. The reason, of course, is that Latin word-initial \( b \) comes down as \( b \) in both languages; indeed, Spanish exhibits a complete merger of Latin *\( w \) and *\( b \) in a single phoneme (say, /b/), which is realised as a stop [b] initially and after nasals, and as a fricative [β] elsewhere, cf. beber [beβer] ‘to drink’ < La bibere vs. vivir [biβir] < La vīvere\(^{10}\). Without reconstructing a proto-segment, there is no way to account for these phenomena\(^{11}\).

Note also that here, the comparative method requires one to reconstruct something that hasn’t in fact survived: *\( w \). To illustrate the same phenomenon from an unattested proto-language, let us see a classical example in the field of Indo-European linguistics: the “back stop series” of Proto-Indo-European (PIE). For a detailed account, see any good textbook on Indo-European linguistics, such as Szemerényi (1990); I will give a simplified and rather abstract presentation here. I will use the following abbreviations: \( K = \) velar stops, \( K^w = \) labiovelar stops, \( S = \) sibilants, \( K' = \) palatal stops.

The Indo-European languages have been divided into two major groups labelled “Satem” and “Centum”. Between the two groups, the following correspondences hold:

\[
(10) \quad \text{Centum:} \quad \text{Satem:}
\]

\[
\begin{align*}
K & \longleftrightarrow S \\
K^w & \longrightarrow K
\end{align*}
\]

\(^{10}\) Strictly speaking, Sp vivir goes back to a proto-form *vīvīre, i.e., it went over to a different conjugational class, but this is irrelevant for the present discussion.

\(^{11}\) Latin is particularly well attested, so we can rely on documented evidence, too, but this isn’t always the case.
We can make three correspondence sets (11a); the Neogrammarian reconstruction of the three proto-segments is given in (11b): 

\[(11) \quad \begin{align*}
(a) & \quad C_1 = \{K, S\} \\
& \quad C_2 = \{Kw, K\} \\
& \quad C_3 = \{K, K\} \\
(b) & \quad *K' = \{K, S\} \\
& \quad *Kw = \{Kw, K\} \\
& \quad *K = \{K, K\}
\end{align*}\]

The crucial point is that no daughter language has palatal stops deriving from the proposed *K’ series: it is reconstructed only because there are three correspondence sets, hence there ought to be three proto-segment series.

The reconstruction of proto-segments, therefore, is inevitable if our theory aims at explanatory adequacy. Without reconstructions, one can set up as many correspondences as one needs to prove relatedness, which, of course, is rather dangerous, because such a simple theory, based exclusively on determining regular correspondences, is extremely powerful: we can set up as many correspondences as we need to prove that two (or more) languages are related, and practically speaking, there is nothing at all to stop us (except reason) from setting up so many as to be able to prove that virtually everything is related to everything else. This may seem to be an exaggeration, but note that the correspondences are needed precisely to rule out chance similarities and borrowing, i.e., to determine which lexical items are part of the common inheritance.

The power of the theory is significantly more restricted if we reconstruct proto-characters. Yet, this is still not enough: one might, after all, set up correspondences, give them a label (step two of reconstruction), but in essence, this is in no sense more than simply having a set of correspondences: all I have done is that instead of referring to sets by labels such as $C_1$, $C_2$, etc., I now replace these labels with other labels, such as $K'$, $K$, etc. What makes reconstruction an important theoretical tool is that I assign some phonetic quality to these labels (say, $K'$ = palatal stop). This restricts my theory in a significant way, due to two basic principles of history, often termed the Uniformity Principles, stated below in (12):

---

12 I neglect the detailed argumentation here, since it is quite immaterial for this discussion.
13 This is one of the arguments for considering reconstructed forms to be “real” rather than mere abstractions or formulae. See Section 1.5. below for more arguments.
14 Both formulations are from Lass (1997:26).
Let me illustrate this with two examples. First, languages we observe around us, although possessing quite different vowel systems, still exhibit systematic regularities governing the shape of these systems. No known language has, for instance, a vowel system such as the one in (13):

(13) Front: y o æ
    Back: u y \n
In other words, no known language has front rounded and back unrounded vowels only, and we take this to be a matter of principle, rather than a mere accident. The Uniformity Principles state that because such a situation is in principle not possible, it wasn’t possible in the past, either: it follows now that we do not reconstruct such a vowel system for a proto-language even if we could account for our correspondences beautifully by postulating such a system.

Second, it has been observed that the least “marked” (whatever this term is supposed to mean) place of articulation for consonants is coronal. Hence we expect, for example, that if a language has oral stops, it will have a t, and indeed, this is what we normally find. Yet, there are exceptions: Hawaiian, for instance, has the stop system depicted in (14), cf. Lass (1984:147):

(14) p k ?

Therefore, such a system is not impossible, though less likely, and should I want to reconstruct such a system I would probably need to support it with strong and convincing arguments. May I note that the General Uniformity Principle is actually none other than a special subcase of the Principle of Uniform Probabilities — where the probability is virtually zero.\(^{15}\)

\(^{15}\) One of the classical examples of a reconstructed system that is typologically suspicious is found right in the field of Indo-European scholarship: the PIE stops. The most generally accepted
In fact, it may seem that reconstruction ends here, which, of course, is the case in the sense that we have reconstructed a sound (phoneme) system, but the story has not yet come to an end: the proto-phonemes still have to be mapped onto the attested sounds, and this is the point where sound laws come into play.

As the terms sound change and sound law are usually used interchangeably, it may be useful to clarify if the two terms can be kept apart. Ritt, for instance, observes that sound change can be interpreted in two ways. He says,

In this view, then, the concept of sound change (...) refers to the following phenomenon: at one time one group of people pronounce certain words of their language in one particular way, while other people at a different time use similar words to convey similar meanings but pronounce them in a different way. A change can be said to have occurred whenever in a language a certain role is played by one articulatory target at one time, and by a different target at another time. Finally, this view can be broken up to yield the following more specific interpretations of ‘sound change’. In the first, it stands for the mere fact [emphasis original] that the latter target can be regarded as the functional equivalent and thus the temporal successor of the former. In the second interpretation, which is much stronger, ‘sound change’ stands for all the factors that caused [emphasis original] the functional correspondence between the two elements. (1994:7)

It is clear that the Neogrammarian term sound law may only be taken to correspond to Ritt’s second interpretation: a mere observation of a change is not a “law” yet — it becomes one when one describes both the change itself and the conditions under which it takes place. Verner’s Law, for instance, isn’t called a law because it describes some equivalences (the equivalences had been noticed much earlier — e.g., that La t corresponds to OE d in ‘mother’, cf. La māter, OE mōdor, though they had been regarded as irregular before Verner (1875)); it’s a law because Verner describes the precise conditions under which PIE *t shows up as Gmc *ð/d. As I mentioned, sound laws are not “there”: they are invented. In a sense, then, Ritt’s first interpretation may be taken to be a pre-theoretical observation. For example, words which contain an e: in Chaucer’s time turn up with an i: by the time of Shakespeare, as in

reconstruction postulates three series: voiceless, voiced, and voiced aspirated — but no voiceless aspirated ones. Such a system is considered to be typologically somewhat marked, to say the least, though see Comrie 1993 for counterarguments.
feed, meet, me, geese, etc. This is a sound change all right, but not a sound law yet; a sound law must state the conditions, too. This change happens to be unconditioned (but that’s also a condition): so this is the law. (Opposed to this, the change from Chaucerian e to Shakespearean (and MoE) a is context-dependent: it only occurs before a tautosyllabic r, e.g., ME ferm, herte > farm, heart but cf. merry, very, etc.)

I do not know if, theoretically speaking, the two interpretations are separable or need to be separated: the change itself (i.e., under the first interpretation) is, after all, part of the context (i.e., the second interpretation). But I believe that to distinguish the two may prove to be useful on the level of terminology. I will therefore from now on use the term sound change in the first interpretation suggested by Ritt, and I will restrict the term sound law to the second interpretation. In other words, I will define a sound law as follows:

**(15)** A sound law is the set of conditions under which a sound change operates.

The ultimate proof of relatedness between two (or more: I use “two” for the sake of simplicity) languages is that allegedly related lexical items are projectable onto a reconstructed proto-form. In other words, we must be able to reconstruct lexical items (more precisely: morphemes) in the proto-language. The history of the given proto-item in the descendants is none other than the application of sound laws onto it, sound laws that apply in a chronological sequence to yield the attested forms. As the term itself implies, a sound law is understood to be regular, in the following sense:

**(16)** The Regularity Hypothesis

Sound laws operate during a given period of time in a given dialect according to strict phonetic conditioning and, as a result, they admit no exceptions.

Simply, if in a language during a given period of its history d becomes ð in intervocalic position, it will always do so — that is, all lexical items that contain an intervocalic d turn up with a ð in its place by the end of that period. The relevance of the Regularity Hypothesis for relatedness (more precisely: cognateness) is that it (alone) makes it possible to define it in a technical sense. A possible formulation is given in (17):
(17) Item $X_1$ in language $L_1$ and item $X_2$ in language $L_2$ are cognate if and only if both can be derived from a hypothesised proto-form $X'$ in $L'$, the ancestor of both languages, exclusively and exhaustively with reference to sound laws in such a way that $X_1$ must be derived from $X'$ via sound laws that apply regularly in $L_1$ and $X_2$ must be derived from $X'$ via sound laws that apply regularly in $L_2$. Derivations must, furthermore, respect the regular temporal sequence of sound laws.

Such related items have, of course, been traditionally called cognates. From now on, I will stick to using this term in the very technical sense provided in (17), instead of the term related, which will be used with reference to languages sharing a common ancestor. As a given item, depending on its phonetic shape, may undergo several sound changes, related items may differ significantly in their phonetic form, especially over a sufficiently long time span. In other words, to repeat a commonplace in historical linguistics, relatedness does not mean similarity but the possibility of derivation in the sense of (17). Let me illustrate the point with a particularly striking example. Portuguese cheio [ʃejˈo] ‘full-MascSg’ and Romanian plin [pli̯ˈn] ‘id.’ are cognates, because they are both derived from VL plénu [ˈplenu] via regular sound changes, as shown in the table in (18):

---

16 The strict use of the term cognate in this very technical sense has another advantage: one can say that E count (= nobleman) and Fr comte are related (though they are not cognates), since their etymology is common (but their derivation from a PIE form, in the sense of (17), is not possible.

17 The VL form actually had a phonetic long e, but as length was allophonic and it is irrelevant for this derivation, I will disregard it here for the sake of simplicity. It does not affect the discussion in any way.
(18)

<table>
<thead>
<tr>
<th></th>
<th>Portuguese</th>
<th>Romanian</th>
</tr>
</thead>
<tbody>
<tr>
<td>INPUT (VL)</td>
<td>plenu</td>
<td></td>
</tr>
<tr>
<td>u &gt; o</td>
<td>pleno</td>
<td>N/A</td>
</tr>
<tr>
<td>pl &gt; f / #___</td>
<td>feno</td>
<td>N/A</td>
</tr>
<tr>
<td>n &gt; Ø / V ___ V</td>
<td>fero</td>
<td>N/A</td>
</tr>
<tr>
<td>Hiatus breaking</td>
<td>fajo</td>
<td>N/A</td>
</tr>
<tr>
<td>(glide insertion)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Final unstressed</td>
<td>faju</td>
<td>N/A</td>
</tr>
<tr>
<td>o &gt; u</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pre-nasal raising</td>
<td>N/A</td>
<td>plinu</td>
</tr>
<tr>
<td>Final high vowel</td>
<td>N/A</td>
<td>plin</td>
</tr>
<tr>
<td>deletion</td>
<td></td>
<td></td>
</tr>
<tr>
<td>OUTPUT</td>
<td>faju</td>
<td>plin</td>
</tr>
</tbody>
</table>

Notes:
1) Po pl > f, apparently, wasn’t a single change but a series of several ones, probably along the path pl > pj > pf > f, but I only included the final result.
2) The Romanian changes, although they appear after the Portuguese ones in the table, may of course have been earlier than some of the Portuguese ones. Within the Portuguese changes, a different relative chronology is possible to achieve the same result.

The crucial point is that all of these sound changes are regular in the sense of (16), that is, none of them is needed to account for the cognateness of these two items only. This is essential, because if we allowed for unique derivations, there would be no way to identify chance similarities. A good example of similarity due to accident is the almost identical phonetic shape of the word meaning ‘time’ in MoE (= taim) and the Münster dialect of Rhaeto-Romance, where it is taimp (Sampson (1999:231)). One could easily assume, for example, a proto-form in PIE that sounds, say, roughly like the Rhaeto-Romance word, and assume a p-deletion process in the history of English, and to claim that there was a PIE phoneme that comes down as t in both languages, and further, that there was a vowel
phoneme which comes down as /ai/; but such sound changes would be unique to this particular item, hence the etymology simply doesn’t work.\textsuperscript{18}

There is another problem to be addressed, though. First, so far I have been talking about \textit{lexical items} — but what is exactly meant by that? Do we compare words? Or morphemes? The answer is not at all easy, because the concatenation of a given morpheme with different other morphemes may produce different phonetic environments, hence a the morpheme in question may undergo different developments, resulting in allomorphy. In pre-OE, for instance, a front high segment (= \textit{i} or \textit{j}) had a fronting effect on preceding vowels, a process known as I-umlaut\textsuperscript{19}. If a suffix (inflectional or derivational) contained a front high segment, it resulted in the fronting of the back vowel of the root; but if the same root was combined with a suffix that did not possess a high front segment (including zero suffixes), umlaut failed to take place. So, for example, Pre-OE *\textit{mūs} ‘mouse’ > OE \textit{mūs} (no change), but NomAccPl *\textit{mūsi} > *\textit{mōsi} > OE \textit{mōs}, hence the modern difference between \textit{mouse} and \textit{mice}. Here, it is clearly the full word form that must be considered, not the morphemes alone. This, however, does not mean that individual morphemes are not legitimately comparable: it merely means that the conditioning phonetic environment may be outside the morpheme.

Second, it is possible that not all morphemes are comparable between cognate items. This happens typically when a word shifts from one inflectional class to another. For example, take the descendants of Latin \textit{dīcere} ‘to say’ in three Romance languages: Ro \textit{zīcere}, Fr \textit{dire}, Po \textit{dizér} (in the Ro and Po forms, I used the acute accent to indicate stress: it is not used in the conventional orthographies). The Classical Latin form had initial stress; and it is only the French and Romanian descendants that can be regularly derived from it. The Portuguese form assumes a VL form *\textit{dīcēre}, with penultimate stress. In other words, the Portuguese form points to a Latin 2\textsuperscript{nd} conjugation verb, with a thematic element ē (i.e., \textit{dīc-ē-re}), while the others point to a Latin 3\textsuperscript{rd} conjugation verb (the one which is actually attested, i.e., with a thematic element ĕ: \textit{dīc-ē-re}). The root and the infinitive marker -\textit{re} present no problem, but the thematic vowel does. The explanation is that in Iberian Romance, the 3\textsuperscript{rd} conjugation was eliminated, and verbs of that class shifted to the 2\textsuperscript{nd} or the 4\textsuperscript{th} conjugation (with the result that now all infinitives are stressed on their ult)\textsuperscript{20}. As this is not a sound change, but morpho-lexical restructuring, the Po form \textit{dizér} as a whole is not cognate with the other forms.

\textsuperscript{18} We happen to know, of course, that the E word derives from OE \textit{tīma} < PGmc *\textit{tīmon}, whereas the Münster one is a derivative of Latin \textit{tempus}.


\textsuperscript{20} See Posner 1996:130.
although all of its morphemes have cognates in both French and Romanian. This shows that what we can legitimately compare here is morphemes, not full lexical items.

As those who have experience with comparative linguistics will know, there are “misbehaved” items all around the place. Let me take two examples. Consider the Germanic words meaning ‘4’, such as E four, G vier, Go fidwor, etc. They begin with an f whose only regular source (disregarding borrowings, of course) in Germanic is PIE *p via Grimm’s Law. This would lead us to assume that the PIE proto-form began with a *p-. This, however, does not seem to be the case, as all other IE languages exhibit an initial consonant which is traceable back to PIE *kw", cf. La quattuor, Gk tétares, Ru četyre, Welsh pedwar, etc21. Do we set up a sound law turning PIE *kw into Gmc *f? No, because this sound law would be unique to the item in question: *k* regularly yields Gmc *x*, cf. Latin quod ‘what’, sequor ‘I follow’ vs. Go hwa ‘what’, saihwan ‘to see’, etc. One must face the uncomforting reality that, say, Go fidwor and La quattuor are not derivable from a common ancestral form via regular sound changes. In other words, counterintuitive though this may sound, they are not cognates — I like to call such items counterintuitive non-cognates.22 Of course, no one in his right mind would claim that they are as different as, for example, E four and Hu négy ‘4’, since apart from the initial consonant, there is no particular problem. Clearly, a “rescue operation” is needed in order to “save” this unfortunate numeral.

Within the history of a language, too, one often finds unexpected forms. The past participle of the OE strong verb čēosan ‘to choose’ is coren. This form ought to yield MoE (Received Pronunciation) **kan via regular sound changes, but we get chosen *ʧouzan, where neither the initial nor the medial consonant is “regular” (OE k regularly remains unchanged, and r never becomes z, either). Yet, as with the numeral ‘4’, it would be counterintuitive to say that they are not related, even though, technically speaking, they are not cognates23. Again, a “rescue operation” is needed.

The Neogrammarians were much concerned with such irregularities, and they invoked the concept of analogy to “save” them (see, e.g., McMahon 1994:70ff). The term analogy is used here in a technical sense, roughly and oversimplifying, the effect of a form on another. In

---

21 Welsh pedwar looks misleading, but the p here is secondary: in British, *k* > p, while — as in all Celtic — initial *p* is lost, cf. Mr orc ‘pig’ = La porcus, OE fearh, Li paršas (Szemerényi 1989:55-60, 65).

22 The term, of course, implies that such items are not cognates technically speaking, but they are related in an intuitive sense, so their non-cognateness is counterintuitive.

23 The term cognate, of course, may also be used to refer to items in two different stages of the same language. Although this usage is not usual, I can see no particular reason why one should say that relatedness between two stages of a language and relatedness between two languages are in principle two different concepts, especially because the two are justified using the same method.
the case of *four* et al. the source of analogical influence is said to be the adjacent numeral *five* — it has been observed that adjacent numerals sometimes have an effect on each other’s shape. *Chosen* is said to be a case of *analogical levelling*, whereby root allomorphy is eliminated. Specifically, the consonantal skeleton of the root is unified to **tf_ζ**. (The vowel difference, being a characteristic feature of strong verbs, is retained, clearly because it serves a morphological function.) The notion of analogy, then, helps to rescue cognateness: if unexpected developments can be explained with reference to analogical influence, we can still maintain that the offending items are *related*, even though, technically speaking, they aren’t *cognates*.

1.4. **[Internal Reconstruction]**

This discussion of cognate forms within a language brings us to the second essential reconstructive technique: Internal Reconstruction (IR). The best known early application of the method was Ferdinand de Saussure’s influential *Mémoire* (1878), one of the most important books ever written on a linguistic topic. Saussure used the method to reconstruct the phonological system of Pre-Indo-European, i.e., the stage preceding PIE reconstructed via CR. The essence of IR is that it starts out from (non-suppletive) *alternants within one language at a given time*; assuming that the alternation (i.e. non-identity) reflects earlier identity, i.e., it arose at some point in the history of the language due to some sound change(s), it attempts to reconstruct the original single form which the alternants are derived from by regular sound laws.

Take as an example the word-final devoicing of stops in German. Let **T** be any voiceless stop and **D** a homorganic voiced one. There are many stems which sometimes surface with a final **T** (if it is also word-final), sometimes with a final **D** (if it is not word-final but followed by suffixal material). So, for example:

\[
\begin{align*}
(19) \quad & \text{Ra[t] ‘wheel-NOMSG’} \quad \sim \quad \text{Ra[d]es ‘wheel-GENSG’} \\
& \text{Wei[p] ‘woman-NOMSG’} \quad \sim \quad \text{Wei[b]es ‘woman-GENSG’} \\
& \text{Ta[k] ‘day-NOMSG’} \quad \sim \quad \text{Ta[g]e ‘day-NOMPL’}
\end{align*}
\]

---

24 In Slavonic, for instance, the numeral ‘9’ shows a similar deviation: it begins with a **d**, although it ought to have an initial **n**, cf. Ru *d’ev’at’*, Sln *devet*, etc. Here, the source of analogical influence is ‘10’, cf. Ru *d’es’at’*, Sln *deset*, which regularly has a **d** < PIE **d**, cf. La *decem*, Gk *dēka*, E *ten*.

25 Or minimised, as here — the root vowel remains different.


We can now set up a correspondence set \( C = \{T, D\} \) as the first step. As the second step, we interpret \( C \) as the ancestor of both \( T \) and \( D \), as in (20):

\[
(20) \quad *C \rightarrow T \quad D
\]

As the final touch, we assign phonetic properties to \(*C\). We can safely assume, on both theoretical and language-internal grounds, that \(*C = D\), i.e., historically, such occurrences of voiceless stops derive from voiced ones via the regular sound change of Word-final Devoicing.

Anyone familiar with phonological analysis will have noticed that this is the same as what one does in a process-based paradigm (such as the SPE model\(^{28}\)) when analysing synchronic alternations. Indeed, Anttila says, “Internal reconstruction (…) is exactly the same as morphophonemic analysis” (emphasis mine).\(^{29}\) But then, where is the difference? After all, synchronic analysis is not internal reconstruction. The answer is that the difference lies not in what one does but in how one interprets the results. In synchronic analysis, we set up \( C \) as a set and assign a theoretical status to it, and we may as well stop there, but we can go on and claim that the alternants are actually derived from it (if we believe in phonological processes); in other words, we can regard \( C \) as an underlier. Internal Reconstruction is none other than assigning historical status to \( C \); that’s where IR is, for the historian, more than simply a synchronic analysis: that’s why it’s something historical. In other words, set up an alternation, label it, and whether you do IR or synchronic analysis depends on the content you give to your set: in synchronic analysis, it is “alternates with”; in IR, it is “cognate with”\(^{30}\).

As Anttila says, IR is but morphophonemic analysis as far as the method goes. In fact, I take the opportunity to correct Anttila here: IR is not necessarily based on morphophonemic alternations, although this is indeed the majority case: any purely phonologically governed alternation is liable to such an interpretation. (See below for an example involving English R-Liaison.) Second, I must disagree with Anttila in equating IR with synchronic analysis. IR is

\(^{28}\) SPE = Chomsky and Halle (1968).

\(^{29}\) 1989:264.

\(^{30}\) “Cognate with” is understood here, of course, in a non-comparative sense (roughly, “having the same ancestral form”).
not the same as synchronic analysis: it is a historical interpretation of the same data as used for synchronic analysis. Or, to put it differently, IR = synchronic data + historical interpretation.

Nonetheless, IR has serious flaws. I illustrate this with two examples. First, take English Spirantization, illustrated by pairs like defend ~ defensive, omit ~ omission, etc. At first sight, we might be tempted to use IR to reconstruct an earlier single stem form underlying the present-day alternation. But we know that these words are Latinate borrowings, in which the alternation is already present: in other words, English borrowed the alternation hand in hand with the words. It would be wrong to assume a Spirantization Rule as a sound change in the history of English: a synchronic rule, then, is not necessarily a historical change.

Second, although an alternation may point to a historical change, it may do so in the wrong way. Consider those non-rhotic accents of English which have obligatory full R-Liaison, i.e., both Linking-R and Intrusive-R, such as London English. This means that a set of words ends either in a non-high vowel or a non-high vowel + r, as in car kær ~ karə, depending on what follows the word. In a synchronic analysis, we can assume a rule of R-Insertion to handle the alternation. But we know that historically, there are two distinct processes: (i) R-Dropping, (ii) R-Insertion. If we did not have any historical information at our disposal, we could not choose which process to assume; it is due to the testimony of other accents (as well as orthography, grammatical descriptions, etc.) that we know what happened. For example, take the words spa and car; both have R-ful and R-less alternants, in exactly the same environments; there is no difference between the two words. Historically, though, one of them is R-ful, the other R-less; but based on the present-day language alone, we can’t tell which is which. The appearance of Intrusive-R results in what we can regard as a kind of merger: the historically distinct categories -Vr# and -V# merge, yielding a situation where they have become context-dependent variants. This reflects a basic problem one must face when doing IR: unconditioned merger, which renders previous contrasts unrecoverable for the method. To sum up, IR requires comparative backup, and therefore, it is insufficient to solve this particular problem.

This much has often been said. Yet, we must be careful here, because CR is not almighty, either. Consider another type of accent, in which there is no R-Liaison whatsoever, such as Southern US English (SUSE). Here, it would not even occur to anyone to reconstruct anything, because we have no alternation: car is always pronounced kær. Let us now imagine the situation that all we have access to is SUSE and London English. In an analysis of the
latter, we are faced with the problem described above; but would SUSE provide the necessary comparative backup? It would not. We are still faced with the same problem, because the difference between two accents can still be accounted for in two ways: either by assuming that SUSE is conservative and London E innovates (via R-Insertion) or that SUSE is innovative (R-Dropping). We need even further comparative support, either from rhotic accents or from ones which have linking but no Intrusive-R (if there are any such accents left; maybe conservative RP speakers have it). The possibilities of rhoticity and R-Liaison are summed up in (21):

\[
\begin{align*}
&\text{> London E:} & \kappa \sim \kappa r, \sigma \sim \sigma r \\
&\text{> Southern US E:} & \kappa r, \sigma \\
&\text{> GenAm E:} & \kappa r, \sigma \\
&\text{> ? Cons RP:} & \kappa r \sim \kappa r, \sigma
\end{align*}
\]

To sum up this lengthy discussion: the fact that IR is not flawless is not in itself an argument against it or in favour of CR, because CR is not flawless either. The point is that when we have access to both IR and CR, and the two disagree, CR takes precedence, but this is a logical consequence of the fact that CR works with data from several dialects. It is in this sense only that CR is superior.

The relevance of IR for cognateness, of course, is pretty much the same as in the case of CR. Technically speaking, two morphs are cognates if one can reconstruct a proto-form, which, in the case of IR, will come from the same language, but from an earlier stage. The example of *choose vs. chos(en)* is a problematic case in point. IR can be useful in sorting out loans, too. For instance, even though the morphemes *foot* and *ped(al)* have a similar meaning, it is impossible to reconstruct a common underlier, i.e., ancestral form (and here, even analogy will not help). To put it differently, one could assume that the noun and the adjective share a common historical root, which underwent differing developments in combination with different morphemes (much like Pre-OE *mūs-* — but, of course, this is not the case. Finally, historically suppletive pairs (such as *go ~ went*) are ruled out, too, not only intuitively but technically as well. In these particular cases, of course, the reader might ask why we should rely on IR at all, since the history of these words is well known, but there can be instances
where all we have access to is a language without (close) relatives and an undocumented past. In such instances, IR is virtually the only tool at our disposal.

1.5. [Postlude: The abstractness debate]

We have seen that in order to prove cognateness, one must inevitably go beyond setting up correspondences and reconstruct proto-segments; furthermore, reconstructed proto-segments are to serve as “starting points” from which the related forms are derived — exclusively and exhaustively via sound laws (and not simply sound changes, at least in the Neogrammorian approach). Now, reconstructed proto-segments are not directly observable (that’s why we call them reconstructions, after all), which has a serious consequence: we have no direct access to the actual pronunciation of proto-forms. This fact has led to a lasting debate about the status of reconstructions: should we regard them as “phonetically real” or as mere “formulae”?

In this section, I point out that the way we have defined cognateness actually requires that we assign (some) phonetic content to proto-segments. Afterwards, I add further arguments to support a realist view. As this topic, however relevant it is for historical investigation, does not, strictly speaking, constitute an integral and organic part of the main topic of this chapter, I include it in the form of a “postlude”: it is possible to see what this chapter is essentially about without this section, though I do believe it illuminates the nature of reconstructions and the method itself.

To start with, I must emphasise that the whole debate is, to my mind, none other than a mere confrontation of highly idealised positions rather than a matter of practical importance. The reason for this is that no one is a “perfect realist” or a “perfect idealist” in practice. No realist would suggest, for example, that we can reconstruct the full phonetic identity of, say, PIE *t. Conversely, no idealist would claim that we know nothing about its phonetic properties. Yet, I do not think the debate is directed against strawmen: science, after all, does idealise, and it often happens that one is forced to take sides on issues that are of no really practical importance. In what follows I will attempt to argue for a (reasonably) realist position.

One of the classical formulations of the “idealist” or “formulist” position is that of Meillet, who writes, “the reconstructions are merely symbols with which we express the correspondences in an abbreviated form”? Indeed, this stance has been taken by many linguists. For an idealist, then, there are either no “proto-segments”: reconstructed forms are just set labels; or, a bit less abstractly maybe, “proto-segments” are not labels, but quite

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31 1964:42; my translation.
abstract (past) entities, whose phonetic content is immaterial; what counts is that the entity underlies the correspondence set. (I do not see an essential difference between these two idealist views; the second is but a softer version of the first.)

Defendants of the “realist” position have pointed out several weaknesses of the idealist argument, and I will not enumerate all of them here.\footnote{Lass 1993 is an elegant overview.} Note, however, that cognateness has been defined as derivability — via regular sound laws — from a proto-form. Sound laws state conditions on sound changes, conditions which are purely phonetically conditioned and admit no exception. Now, if present-day forms, which are undoubtedly phonetically “real”, derive via phonetic changes from earlier forms, it follows in a more than trivial sense that earlier forms, including unattested proto-forms, must have been phonetically real, hence they contained segments which made up a phonological system. The shape of the phonological system one reconstructs is constrained by the uniformity principles. If, however, proto-segments are believed to be mere formulae, one is simply allowed to reconstruct any system: systems of formulae are not phonological systems. The Uniformity Principles lose their constraining force; and once the Uniformity Principles are gone, and we can reconstruct whatever we need, we’re back to the point where we may as well not reconstruct anything but simply set up a correspondence set. This, however, has been shown to be insufficient to prove relatedness. The formulist position results in a vicious circle.

There is one more argument I wish to put forward, concerning the scientific appropriateness of the idealist position; I will elaborate on Lass 1993. Recall that the main reasoning behind this view is that we cannot know the exact phonetic quality of the proto-segment anyway, which means we can only speculate about it; and science should end where speculation begins. Such arguments are, though they sound good, quite misguided (and misleading).

Imagine a situation when we end up with five correspondence sets for the vowel system of the proto-language *L. Furthermore, the daughter languages show remarkable unity in the phonetic quality of the vowels; say, we can safely reconstruct two high vowels, *i and *u, two mid ones, *e and *o, and a low one, *a. Now, the idealist would say that we do not really know the quality of these vowels: the high and mid vowels may have been tense or lax; as for the low vowel, was it front, central or back, rounded or unrounded? We do not know. The logical conclusion, the idealist says, is that the whole thing is hopeless, and we had better think of *a, *e, *o, *i, and *u as mere labels. (We might use perfectly different symbols, too, to represent them, e.g., *∅, *øy, etc., avoiding any association with phonetic qualities.) But
the idealist is totally wrong here. For we might not tell exactly if, say, \( *a \) (or \( *\smallcirc \)) was back or rounded or whatever; but we know that it was low. Similarly, we know that \( *i \) was front high unrounded; and so on. Therefore, using \( *a \) is not quite the same as using \( *\smallcirc \); in fact, the latter symbol gives no reason why it should have low vocalic reflexes in the descendants. Of course, it is much more ideal if we can tell that it was “low back rounded”; but the fact that we are unable to do so does not mean that we should not say anything. And even the most extreme idealist would not in practice reconstruct \( *\smallcirc \), because he knows that to say that it was low (or that it was a vowel in the first place) is something. The dividing line is not between knowing that it was “low” and knowing that it was “low back rounded”: the dividing line is between knowing something about it or not knowing anything. Schematically:

\[
(22) \quad \text{(Nothing)} \leftrightarrow (V < \text{low} V < \text{low back} V < \text{low back rounded} V)
\]

Therefore, once we say \textit{anything at all} about the phonetic quality of our proto-segment, we are in principle realists.34 I emphasise this very strongly, because once we have specified some phonetic content, there is no principled basis how far we must go into details to abandon idealism and turn into realists. This is a science-theoretic requirement: either 1 or 0.35

But there is more to it: Why should we give up and say \textit{nothing at all} about something just because we can’t tell \textit{all} about it? As Lass says quite appropriately, our reconstruction “may not get us as far as we’d like, but it does get us somewhere, and that isn’t a bad place to be”.36 After all, science is about going as far as we can; going beyond that may be speculation (and science should indeed stop there), but not going as far is a grave mistake, too. No branch of science is almighty: we have our limits. Yet, no archeologist in his right mind would ever suggest that we should not reconstruct what we can of a ruined church or house just because we cannot reconstruct the exact structure of the roof. For science, it is imperative to define its limits; but it is also imperative to reach out as far as the limits are. The idealist position, in my view, is unscientific (on two counts). And this I find a very strong counterargument.

34 “Anything at all” should be taken literally: if I say “this proto-whatever was a vowel”, that is already a phonetic statement.
35 This is why concepts such as “heavy” or “long” are unscientific and are not defined in physics: how heavy must something be in order to be heavy? We can’t give a principled answer.
The phonology-morphology interface 1: Introduction and the SPE model

It has long been observed that morphological structure may have phonological effects. Specifically, certain morphologically complex forms exhibit phonological behaviour that is uncharacteristic of monomorphemic forms. Related to this is the observation that alternations are of different types with respect to the morphology and the phonology: some are clearly morphological, some purely phonological, but many alternations are somewhere “in between”. The main aim of this chapter is to give a brief overview of these interface phenomena and take a look at the view of classical generative phonology on them. This view will be contrasted with a radically different one in Chapter 3, taken by Government Phonology. We will also discuss at some length what is essential in this respect: the definition of the domain of phonology.

2.1. [Affix types]

There are two basic facts relating to our topic that any theory of language must account for. First, morphologically complex forms often behave differently from simplex ones. Second, morphologically complex forms show a dual behaviour themselves: some of them exhibit features that characterise simplex forms, while others fail to behave in that manner.

The first situation is illustrated by the operation of vowel harmony in Hungarian, for example. As it is well known, Hungarian has vowel harmony (VH), i.e., an arrangement of vowels within a phonological word according to backness and, to a limited extent, according to roundedness. Specifically, the vowels in a word must agree in backness (I will disregard roundedness harmony here, partly because it is restricted to certain vowels, partly because backness harmony will suffice as an illustration). As a result of VH, the overwhelming majority of Hungarian suffixes have two variants, one with a back vowel and another with a front vowel. For example, the inessive suffix may be realised as -ban bon or -ben ben, as illustrated by forms like kútban ‘in well’, házban ‘in house’, boltban ‘in shop’ vs. földben ‘in
earth’, kertben ‘in garden’, tűzben ‘in fire’, vízben ‘in water’. Within morphologically simple stems, however, VH is inactive in Modern Hungarian, as testified by a number of loans that fail to conform to it, e.g., sofőr ‘chauffeur’, attitűd ‘attitude’, malőr ‘mishap’, etc.

A similar situation can be observed in English with regard to the distribution of long (or tense) vowels in antepenults, a regularity known as Trisyllabic Shortness (Laxness). In monomorphemic forms, long vowels happily occur in antepenults, cf. ōmega, nīghtingale, etc. Morphologically complex forms, however, behave differently, as shown by the shortness of the root vowels in vān(ity), grāt(itude), vis(ible) vs. the long vowels in vāin, grāte(ful), (re)vīse. One might conclude that the vowel of the antepenult must be short in suffixed forms, but not in monomorphemic ones. In other words, Trisyllabic Shortness applies only if the vowel in question is in an antepenult due to the application of a morphological process, i.e., suffixation; but a long vowel is possible in underived forms, that is, if the vowel’s being in the antepenult is lexically given. Effects of this sort are usually referred to as derived environment effects (see Scheer 2001).

Yet, the situation is even more complex, because not all suffixes trigger shortening. In lāziness, cātering, prōvable, for instance, we find long vowels in antepenults, although these forms are clearly morphologically complex. In a certain sense, then, the suffixes -ness, -ing, -able, as well as many others, behave as if “they were not there”. This is not only true with respect to a set of segmental phonological processes, but for stress placement as well: whereas the “-ity-type” suffixes influence stress placement, and may cause stress shift (e.g., históric — historicity), the “-ness-type” never does (cf. interesting — interestingness). All this is familiar to everyone who has had some experience with phonological analyses of Modern English. In this chapter and the next, we will consider the matter in some detail.

In his recent work Understanding morphology (2002), Haspelmath refers to these two affix types as integrated vs. neutral. He sums up the main differences in a table, reproduced here as (23) (his (10.3), 2002:199):

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37 The phonetic values of Hu vowels appearing here are as follows: 〈a〉 = [ɔ], 〈e〉 = [ɛ], 〈o〉 = [o], 〈á〉 = [aː], 〈ú〉 = [uː], 〈i〉 = [iː], 〈ő〉 = [ɔ], 〈ő〉 = [o], 〈ü〉 = [yː]. Of the consonants, 〈s〉 = [ʃ]; the rest is as expected.
(23) Integrated vs. neutral affixes

<table>
<thead>
<tr>
<th>Integrated affixes</th>
<th>Neutral affixes</th>
</tr>
</thead>
<tbody>
<tr>
<td>— are in the domain of stress assignment</td>
<td>— are not in the domain of stress assignment</td>
</tr>
<tr>
<td>— trigger and undergo morphophonemic alternations</td>
<td>— do not trigger or undergo morphophonemic alternations</td>
</tr>
<tr>
<td>— words with integrated affixes show the phonotactics of monomorphemic words</td>
<td>— words with neutral affixes may show phonotactic peculiarities</td>
</tr>
<tr>
<td>— tend to occur closer to the root</td>
<td>— tend to occur further away from the root</td>
</tr>
</tbody>
</table>

Let us now see some examples for these differences.

1. **Stress assignment.** As pointed out above, an integrated affix may cause stress shift compared to the unaffixed form. The English suffix -*ity*, for example, attracts the stress on the last syllable of the stem. In fact, this is just what we expect if we assume that this suffix is literally integrated into the body of the word, hence stress assignment works in precisely the same manner as for a monomorphemic word. Specifically, the relevant stress rule here assigns stress as follows: (i) disregard the last syllable of the noun (it is extraprosodic), (ii) stress the penult if it is heavy, (iii) if the penult is light, stress the antepenult. This is how the noun *América* receives a penultimate stress, for instance (as opposed to, say, *aréna* and *veranda*); any noun in -*ity* will automatically follow the same pattern, since -*ity* is disyllabic, and its first syllable is light: it is not at all surprising that stress falls on the last syllable of the stem. As opposed to this, nouns with a neutral suffix (*interestingness, párenthood, etc.*) receive stress in such a way that the suffix is disregarded, and only the stem is considered.

2. **Morphophonemic alternations.** A suffix like -*ity* triggers Trisyllabic Shortening (cf. *vanity*); a neutral suffix does not (cf. *laziness*). Conversely, integrated affixes may also undergo morphophonemic alternations, cf. *credible* — *credibility* *abul* ~ *abul*.

3. **Phonotactics.** The English irregular past tense suffix -*t* produces forms which show the phonotactics of morphologically simplex forms, e.g., *kept, left*: both exhibit a short vowel, which is expected before consonant clusters of this type (long vowels are not found before *pt*.

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38 It isn’t quite true, however, that stress assignment in forms with an integrated suffix in English *always* works in the same way as in monomorphemic forms. A notable exception is the suffix -*ic*, which is pre-stressed just like -*ity* (cf. *históric, enigmátic, etc.*) — even if the last syllable of the stem is light, something one would not expect in monomorphemic forms.
and *ft inside a morpheme\(^{39}\), and *left has a cluster *ft, again possible in monomorphemic forms (cf. *soft, *lift). As opposed to this, the concatenation of the regular (and neutral) past suffix *ed may produce sequences that are not found inside a morpheme, cf. *peeped *pipt, moved *mu:vd.

4. **Distance from the root.** If affixes of different types are found in a word, it is usually the case that the affix closer to the root is an integrated one, and the one further away is a neutral one; in other words, neutral affixes tend not to come between the root and an integrated suffix. For example, taking the noun *child*, one can form an adjective from it using the neutral suffix *-ish* to yield *childish*; from this adjective, a further noun can be formed, viz. *childishness*; the suffix *-ity*, although synonymous with *-ness*, is not possible here (*childishity*), since *-ity* is integrated.\(^{40}\)

2.2. **[Alternations]**

In order to see what is at stake when discussing the domain and the functions of the phonological component of grammar, a brief overview of the nature of alternations is probably useful. In fact, the study of alternations has been one of the central issues of linguistics for a long time. Firstly, alternations are crucial to historical linguistics inasmuch as historians have always considered it to be a chief aim of science to account for how they have arisen (cf. Internal Reconstruction); second, on a synchronic plane as well, attempts at the classification of alternations appear at an early date\(^{41}\). Let us now see what types of alternations can be distinguished. I will follow Haspelmath 2002 again, who (in my view) gives a treatment which can be considered as pre-theoretical as possible.

Haspelmath 2002:185 divides alternations into two basic types: *automatic* vs. *morphophonological* alternations\(^{42}\). He provides several criteria along which the two can be distinguished, the most important of which are shown in (24) below; the last one (italicised) is

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\(^{39}\) Though *ft* is problematic, at least in RP, cf. *draft* *draft*; it is also true, however, that the only long vowel that appears here seems to be *a*.

\(^{40}\) This “proximity to the root” business is, nevertheless, a tendency more than an absolute rule, and even systematic exceptions are to be found. Abstract nouns in *-ity*, for example, are regularly formed in English from adjectives in *-able*, itself a neutral suffix, cf. *readability, analysability*, etc.

\(^{41}\) The earliest analyses which make serious theoretical claims are Kruszewski 1881 and Baudouin de Courtenay 1895 [1972], itself a modification of Kruszewski’s theory; see Anderson 1985:56-82 for a historical overview. Though the terminology introduced by these scholars (known as the Kazan school) is no longer in use, their observations are surprisingly modern.

\(^{42}\) The division coincides, by and large, with the lexical/post-lexical distinction of Lexical Phonology, cf. Durand 1990:188ff, McMahon 1994:66f, Roca 1994:252, etc., but it must be borne in mind that this distinction in the generative tradition refers to rules (assumed to be responsible for alternations) rather than the alternations themselves.
my own addition, not featuring in his list, but it is nonetheless important enough not to be excluded.

(24) **Two types of sound alternations**

<table>
<thead>
<tr>
<th>Automatic</th>
<th>Morphophonological</th>
</tr>
</thead>
<tbody>
<tr>
<td>— phonetically coherent</td>
<td>— not necessarily phonetically coherent</td>
</tr>
<tr>
<td>— alternants are phonetically close</td>
<td>— alternants may be phonetically distant</td>
</tr>
<tr>
<td>— only phonologically conditioned</td>
<td>— at least in part morphologically or lexically conditioned</td>
</tr>
<tr>
<td>— not contradicted by simple morphemes</td>
<td>— may be restricted to derived environments</td>
</tr>
<tr>
<td>— extend to loanwords</td>
<td>— need not extend to loanwords</td>
</tr>
<tr>
<td>— can create new segments</td>
<td>— do not lead to new segments</td>
</tr>
<tr>
<td>— not necessarily restricted to the word level</td>
<td>— generally restricted to word level</td>
</tr>
<tr>
<td>— admits no exceptions</td>
<td>— may admit exceptions</td>
</tr>
</tbody>
</table>

The classification is familiar to all who have studied phonology, and it is probably unnecessary to go into details here; suffice it to name some examples. Automatic alternations include the alternation between “clear L” (= [l]) and “dark L” (= [ɫ]) in many varieties of English, or the alternation between r and zero in non-rhotic English accents; morphophonological alternations include the long ~ short (or: tense ~ lax, depending on analysis) alternations of vowels of the grāve ~ grăvity type or the alternation of final f with v (e.g., *wife ~ wives*). (The reader is invited to check the validity of the above classification with respect to these examples.)

Two notes are in order here. First, the characteristic features of automatic alternations seem to be derivable from the single property of *strict phonological conditioning* (where the conditioning, of course, includes both the sounds that participate in the alternation as well as the context). Second, all theories of phonology agree that such alternations are in the domain of the phonological component, i.e., no one seems to suggest that they should be treated as morphological or lexical. The point where linguistic theories diverge is whether, and to what extent, morphophonological alternations are assignable to the phonology. In other words, can they be regarded as products of phonological processes?
The answer is the more difficult because morphophonological alternations do not constitute a homogenous class themselves. Indeed, it is probably fair to say that (and, historically, this seems to be the case) such alternations are arrangeable along a cline which ranges from near-automatic (overwhelmingly phonologically conditioned) to fully lexical (i.e., suppletive). As pointed out above, automatic alternations are generally treated as the results of genuine phonological processes; as for the other end of the scale, no linguist would ever suggest that pairs such as *go ~ went* be accounted for by the phonology\(^{43}\). But what about intermediate cases?

A few examples may shed light on why the question is so essential and what can depend on our answer to it. We have already seen examples for alternations (such as English L-Darkening) which are generally regarded as truly phonological. To illustrate the other extreme, i.e., true allomorphy, consider the genitive singular of Slovene masculine nouns. This category is generally (and productively) realised as a suffix -a, e.g., fant ‘boy’ → fanta, pêtek ‘Friday’ → pêtka, komunizem ‘communism’ → komunizma, etc. (The acute accent indicates stress.) A handful of masculines, however, add -u instead of -a, e.g., grad ‘castle’ → gradu, most ‘bridge’ → mostu, etc. This is clearly an instance of pure allomorphy: the selection of the suffix depends on the lexical identity of the stem, and the two allomorphs are not relatable phonologically\(^{44}\). Such alternations are obviously not likely candidates to be treated by the phonological component, and, to the best of my knowledge, no theory has ever suggested anything of the sort\(^{45}\).

Consider now in detail the alternation introduced by L-Darkening in many varieties of English (I take RP here as an illustration.) As it stands, L-Darkening is an automatic process whose domain is the utterance, i.e., it operates across words as well as within them. Now, if a word which ends in /l/ is followed by nothing (i.e., it is in absolute word-final position) or a word beginning with a consonant (exc. yod), its final /l/ is realised as [l]; otherwise, it is

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\(^{43}\) Historically, in fact, *go* and *went* are different lexemes, not connectable in any sense. The situation is somewhat different in case of pairs such as *tooth ~ dental*, where the two roots *are* from the same etymological source (PIE *dVnt- ‘tooth’), but are nonetheless not cognates in English (but E *tooth* and Lâ *dent- ‘tooth’). Lightner 1978 attempts to relate members of such pairs via phonological rules, but this is rare: most linguists would disagree.

\(^{44}\) It is true that -u only occurs with monosyllabic stems, but the majority of monosyllables select -a, so this is hardly a sufficient condition; it is better to say that -u is restricted to monosyllabic stems, so there’s a phonological constraint on its use (if it is not a mere accident, which is also possible, given the small number of such items in contemporary Slovene), but it by no means predicts the selection of this suffix. May I also note that the majority of such nouns can take -a as well. See Toporišič 1991:217, Herrity 2000:47.

\(^{45}\) Indeed, according to the model I propose in Chapter 4, this is not even allomorphy: the two suffixes are separate morphemes.
This yields surface alternants such as *te[l] us ~ te[H] them*, etc. As for the morphology, it plays no role whatsoever, the alternation is fully automatic and only phonologically conditioned; probably no one would analyse *te[l] ~ te[H]* as an instance of allomorphy. This alternation is just at opposite end of the scale compared to the Slovene alternation depicted in the previous section.

As a third example, consider the English regular plural in -(e)s. Most analyses assume that the underlying form of the suffix is /z/, from which the surface variants (viz. [z, s, ɪz/əz]) are derived by fully predictable, automatic phonological processes. Now, inasmuch as we talk about the realisations of a particular morpheme, this alternation could be considered a case of allomorphy. Yet, it is fundamentally different from the Slovene example, where the choice is not determined by phonological factors; here, the pronunciation of the suffix depends only on the last segment of the stem. Indeed, as Spencer 1991:6 notes, several linguists do not even consider cases like this to be instances of real allomorphy. In fact, the same alternation characterises all English suffixes as well as clitics with an underlying /z/, as shown in (25) below (data from Pinker 2000:35f):

(25)

<table>
<thead>
<tr>
<th></th>
<th>[s]</th>
<th>[z]</th>
<th>[ɪz/əz]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Plural:</td>
<td>hawks</td>
<td>dogs</td>
<td>horses</td>
</tr>
<tr>
<td>Possessive:</td>
<td>Pat’s</td>
<td>Fred’s</td>
<td>George’s</td>
</tr>
<tr>
<td>Sg3PresInd:</td>
<td>hits</td>
<td>sheds</td>
<td>chooses</td>
</tr>
<tr>
<td>Contracted has:</td>
<td>Pat’s eaten</td>
<td>Fred’s eaten</td>
<td>George’s eaten</td>
</tr>
<tr>
<td>Contracted is:</td>
<td>Pat’s eating</td>
<td>Fred’s eating</td>
<td>George’s eating</td>
</tr>
<tr>
<td>Contracted does:</td>
<td>What’s he want?</td>
<td>Where’s he live?</td>
<td>—</td>
</tr>
<tr>
<td>Affective:</td>
<td>Pops, Patsy</td>
<td>Wills, bonkers</td>
<td>—</td>
</tr>
<tr>
<td>Adverbial:</td>
<td>thereabouts</td>
<td>towards, nowadays</td>
<td>—</td>
</tr>
<tr>
<td>Link in compound:</td>
<td>huntsman</td>
<td>landsman</td>
<td>—</td>
</tr>
</tbody>
</table>

The data in (25) suggest that the choice between the variants [z, s, ɪz/əz] is not a matter of allomorphy, as it is not restricted to a particular morpheme or morphemes; instead, it appears

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46 The choice between [ɪz] and [əz] is mostly dialect-dependent: in RP, for instance, only the former is possible, whereas only the latter is found in Australian English.

47 This is much rarer, and this may be the reason why Pinker does not give an example for the third column.

48 Again, it is difficult to come up with an example for the third column. *Patsy* contains two suffixes, of which -y, of course, is irrelevant for us.
that the phenomenon is independent of morphological or lexical identity. It must be added, though, that it is not as fully automatic as L-Darkening, and it results in phonemic alternations, which is obviously the reason why the possibility of its being treated as an allomorphic alternation has been considered.

Finally, let us briefly give a characterisation of the morphophonological behaviour of English irregular Preterites. These are (historically) of two types, traditionally referred to as strong vs. weak. The strong Preterites are descendants of a common Indo-European pattern, referred to as Ablaut (or Gradation, or Apophony: I stick to Ablaut, this term being probably the most widespread of the three). The exact origins of Ablaut alternations are immaterial here; suffice it to say that strong verbs form their Preterite (as well as their PretParticiple) by way of internal vowel changes, i.e., Ablaut, without the addition of any suffix that would serve as a Preterite marker. MoE examples include sing, come, bite, drive, fall, etc. Weak verbs, on the other hand, represent a distinctively Germanic innovation, whereby the Preterite (and the PretParticiple) is formed by the addition of a suffix, realised in the Germanic languages as ð, d or t. (Only the last two are found in English.) Examples in MoE include all regular preterites as well as ones such as kept, meant, told, brought, met, fed.

As for strong preterites such as drive ~ drove, it is easy to see that a phonological treatment of such alternations runs into serious difficulties. One might, for instance, propose a morphophonological rule replacing a with o (where the conditioning is entirely outside the phonology) but then, this won’t work for bite, whose past tense form is bit, rather than *bote. A more appropriate solution, it seems, is to say that this type of alternation is purely morphological, although see later for a phonologised analysis in the SPE framework.

The case is somewhat different with weak preterites, especially when the preterite suffix is present on the surface, such as dreamt, kept, etc. Here, while there is an alternation in the root morpheme, it is not the sole visible exponent of the category “Preterite”, and one can in fact find a way to refer the alternation to the domain of phonology, provided one adopts a definition of phonology which allows for such a treatment. As a phonology-based analysis

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49 See, for example, Lehmann 1993:61f, 117-135.
50 This suffix is usually referred to as the “dental suffix” in the literature, obviously because it contains a dental (or at least coronal) consonant. I show some reluctance in using this term, because, strictly speaking, it confuses phonological and morphological categories, i.e., it appears from it as though “dental” were a morphological category. Nonetheless, the term has come to stay. As for the origins of this Germanic past tense formation, there are various theories; see Prokosch 1939:194-199.
51 Due to sound changes in the history of English, some of these (e.g., met, fed) look as if they were strong verbs, but historically, they are distinct, cf. OE mêt-te ‘met’, fêd-de ‘fed’. They will be discussed in Chapter 6.
will be presented shortly, I do not go into details here. As for a non-phonological one, see Chapter 4.

Let us now summarise the abovesaid. Alternations can be classified as follows:

1. **Automatic alternations.** These are purely phonologically conditioned, and there seems to be a general consensus among scholars that they are to be accounted for by the phonology.

2. **Allomorphic alternations** (such as the Slovene example). These are morphologically conditioned only, and may involve phonetically unrelated alternants. Again, probably all linguists would agree that such alternations are the products of morphology, and they must, therefore, be excluded from the domain of the phonology; in my interpretation (see Chapter 4), they are probably not even morphological, but lexical.

3. **The “in-between” cases;** let us call these, as usual, **morphophonological alternations.** Alternations of this type involve phonological as well as morphological conditioning, and it is this class which presents problems for the linguist. As it happens, some theories refer them to the phonology; others exclude them from the phonological domain.

Having briefly sketched the problem, let us now turn our attention to two different treatments of it. The first one will be the position of classical generative phonology; then I will turn to Kaye’s (1995) analysis, presented in the framework known as Government Phonology.

### 2.3. [The SPE model: relevant aspects]

This section deals with the classical generative model of morphophonology and the domain of phonology. It is beyond the scope of this paper to give an outline of the entire model, and it would be probably unnecessary, too, given that it is probably the best known phonological paradigm in modern linguistics. I will deal with two aspects only, both of which will turn out to be relevant to the remainder of this thesis. One of the topics I discuss is how SPE defines the domain of phonology; put differently, what functions are referred to the phonology in this framework. This aspect of the theory is relevant in two ways. First, it contrasts sharply with the Kayean (and, in general, Government Phonology) interpretation, which refers much of what is performed by the phonological component in SPE to the lexicon. Second, as it will become clear in Chapter 4, what one can regard as a fusional morphological concatenation (as opposed to an agglutinating one) depends greatly on how one defines the domain of phonology (as opposed to the morphology and the lexicon). The other topic I will devote some attention to is the classical generative treatment of how morphological information is

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52 Which happens to be the case in the Slovene example, but is not necessary, cf. the English alternation index ~ indices, where the stem alternants are [ˈɪndəks] and [ˈɪndɪst(t)iː]-, where there is at least a partial overlap in phonetic shape (see Spencer 1991:6).
represented in phonology, i.e., what effects morphological structure can have on the application of phonological rules.

Classical generative phonology as outlined in SPE is an item-and-process type paradigm, which incorporates a principle that Lass 1984:63 calls the *Unique Underlier Condition*, stated in (26) below (Lass’s (4.8.)):

\[(26)\] **The Unique Underlier Condition (UUC)**

Every non-suppletive alternation is to be accounted for by assigning to each morpheme a **SINGLE, PHONOLOGICALLY SPECIFIED UNDERLYING REPRESENTATION**, with the allomorphy derived by general (preferably phonologically specified) rules (emphasis original).

The UUC is not specific to SPE, but it clearly does incorporate it as an organising principle of grammar. The following notes are in order.

First, note that Lass speaks about **allomorphs** being derived by (phonological) rules from a common underlier. In fact, in the SPE interpretation, this includes purely phonological rules as well — including rules which had been treated as allophonic in the structuralist tradition — simply because the theory does not recognise an independent phonemic level of representation. In other words, **there are two systematic levels in SPE: a morphophonemic level (phonological/underlying representation) and a phonetic level (phonetic/surface representation)**\(^{53}\).

Second, the tricky notion of **suppletivism** is part of the formulation. Lass, admitting that it can be difficult to determine what counts as non-suppletive, adds that “at least the extreme cases are clear”, and gives the following examples (his (4.9)):

\(^{53}\) Though it must be added that the term *morphophonemic* is not used in SPE itself, because of its structuralist connotations (see SPE:11). Also, it must be added that the phonological representation of an item in SPE is not necessarily the same as its lexical representation: the former is derived from the latter via readjustment rules.
Then he goes on to say “that ‘phonological motivation’ is not always obvious, and that ‘depth’ of analysis plays a crucial role” (ibid.).

The nature of representations in SPE is an issue we must address next. The model is a linear one: all representations, whether underlying or surface (or intermediate), consist of a linear string of feature matrices which specify phonetic segments as well as boundary symbols. We’ll come back to the latter shortly; let us say a few words about the former. Each segment is a feature matrix, in which each feature is binary: e.g., \([\pm \text{voice}]\) (this is one of the points where the model is in sharp contrast with Government Phonology: see later)\(^{54}\). Phonological rules, in a re-write format, modify feature values, or delete, insert, or reorder matrices\(^{55}\).

As the internal organisation of segments is not particularly relevant for this thesis, I will neglect this aspect of representations here. What I find important, however, is that segments are not organised into higher-level units. In other words, no rule can make reference to a group of segments in a systematic fashion. For all intents and purposes, this means that phonological units larger than a segment — most importantly, the syllable as well as its constituents such as the coda — are not recognised in this framework\(^{56}\). For example, the R-Dropping rule of non-rhotic accents of English, operative in coda position, can only only be formulated with reference to the much criticised disjunctive context “before a consonant or in absolute word-final position”\(^{57}\).

A further relevant aspect of the SPE model is that non-surface representations can be *abstract*, i.e., they can contain segments which do not appear on the surface\(^{58}\), and indeed, underlying representations are often far removed from surface ones. For example, the word

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\(^{54}\) Strictly speaking, this is only half the truth: a segment can have a value \([u \text{ Feature}]\), too, \(u\) standing for *unmarked*, to be filled in later by + or - according to markedness conventions (see Roca 1994:46).

\(^{55}\) See, e.g., Roca 1994:1ff.

\(^{56}\) The strict CV version of Government Phonology appears to be of the same type of model in this respect, but this is only apparent: see Chapter 3.

\(^{57}\) See, e.g., Kahn 1976.

\(^{58}\) Not only in the sense that the given segment does not appear in any of the surface forms of the given morpheme, but also that it is possible to posit underlying segments which never surface *at all* in the language.
coin [kɔɪn] is assumed to have an underlying form /kə:n/ (SPE:192). The possibility of such a high level of abstractness was subject to intense attacks from the earliest times on, and it is certainly not permitted in Government phonology.

For us, the most important aspect of the SPE model is the representation of morphological information in phonology. The issue is given a thorough treatment in Chapter 3 of SPE (SPE:66ff). SPE recognises three morphosyntactic boundaries, symbolised by +, =, and #. All of them are characterised by the feature specification [-segment], which sets them apart from phonetic entities (i.e., “real” segments). They are, therefore, nonsegmental, which marks a clear break with the American structuralist tradition. The distinction between the individual boundaries is made with reference to two features. It would be difficult to sum up the essence of the distinction in a more concise and elegant way than it is found in SPE (SPE 66f):

Among the features of the boundary system, “formative boundary” (henceforth “FB”) requires explicit mention. Only a single boundary is marked [+FB]. This boundary, which we will designate with the symbol +, appears between the final segment of one formative and the initial segment of the following formative. (...) All other boundaries are marked [-FB]. One of the non-FB boundaries is the unit # that appears automatically before and after a word and in sentence-initial and sentence-final position. We will also have occasion to refer to another boundary, which we will denote by the symbol =. In our terms, the unit = must be distinguished from # by some feature, let us say the feature “WB” (word boundary). Thus the symbol + stands for the feature complex [-segment, +FB, -WB], # stands for the feature complex [-segment, -FB, +WB], and = for the feature complex [-segment, -FB, -WB].

Chomsky and Halle go on to make a substantial claim about the status of the formative boundary, i.e., that “the presence of + can be marked in a rule, but (...) the absence of + cannot be marked in a rule” (ibid.), and claim that the other boundaries do not behave in that way. They write (ibid.),

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59 For typographical reasons I use IPA transcriptions rather than the ones employed in SPE, but this is immaterial here.
60 See Kiparsky 1973a, 1973b, Hooper 1976 for early generative discussions.
61 Where the junctures are phonemes just like any other (they have allophonic realisations, for instance): see, e.g., Anderson 1985:299ff, Scheer 2004b:4.
A string containing # is not subject to a rule unless this rule explicitly mentions # in the proper position. Notice that this convention amounts to a fairly strong empirical assumption about the nature of rules. It implies that although we can frame phonological processes which are blocked by the presence of the boundary #, we cannot frame processes which are blocked by the presence of formative boundary. If a process applies to a sequence without formative boundaries, it also applies to otherwise identical sequences containing these units (emphasis mine).

As the = boundary appears exclusively between certain prefixes and roots in the SPE analysis of English, I do not discuss it in detail — for the simple reason that the nature of prefixation is irrelevant for the present work. I limit the discussion to suffixes, therefore.

It is useful to recall the distinction between integrated and neutral affixes. The integrated affixes (suffixes from now on for the sake of simplicity) described there correspond, by and large, to the + boundary suffixes of SPE, e.g., van+ity, kep+t, etc. The neutral ones, on the other hand, correspond to those suffixes that are joined to the stem with a word boundary, e.g., lazi#ness, peep#ed, etc. To illustrate the difference in the behaviour of the two types of morphological structure, let us see some derivations. I will simplify the derivations to some extent for clarity of exposure, but in no significant sense will this affect the essence of how the SPE model works. First, however, let me give a formulation of Pre-Cluster Shortening (PCS), taken from SPE:241 (their rule 20(III)):

\[
(28) \text{PCS: } V \rightarrow [-\text{tense}]/\_\_\_ [+\text{cons}][+\text{cons}]_{-\text{voc}}
\]

That is, a vowel is lax before a cluster of at least two consonants\(^{62}\). Notice that no reference is made to boundaries, which predicts that (i) monomorphemic forms are subject to it (= you cannot make reference to lack of +), (ii) the rule is not applied if the two consonants are separated by a word boundary (#). Let us now see three derivations:

\(^{62}\) This formulation of PCS, Chomsky and Halle say (SPE:241, Footnote 2) respects the fact that “laxing does not take place in consonant clusters ending with a liquid”.

(29) *keep, kept, and peeped: derivations*

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<tr>
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<th>keep + t</th>
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<tr>
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<td>keep</td>
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<td>Voicing assimilation</td>
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<td>SR</td>
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Let us now sum up how morphological information can affect the phonology in SPE.

1. Rules which make no reference to + will automatically apply to all strings between two word boundaries (i.e., #......#), no matter if the string contains internal + boundaries or not.
2. The very same rules will, on the other hand, be blocked by a word boundary, i.e., their domain of application is closed by a # (cf. *peep#ed*).
3. It is possible to have a rule, however, which *does* make reference to +, in which case it does not apply inside a formative.
4. If a process applies across a word boundary, or it is triggered by its presence, the word boundary must be mentioned in the rule.

The abovesaid leads to a very important conclusion: suffixes which are joined to the stem with a formative boundary (i.e., “integrated” ones) may trigger a rule which does not apply to morphologically simplex forms, but the opposite does not hold: there is simply no rule which applies to monomorphemic forms only, but not to an integrated concatenation. In a sense, then, morphologically simplex forms exhibit the same behaviour as morphologically complex ones (without an internal #, of course), but only in one direction. It is important to have this in mind, since (as we will see shortly) Government Phonology makes a stronger claim, viz. that monomorphemic forms and certain morphologically complex forms (corresponding, to some extent, to the SPE-type “+ concatenations”) always behave in *exactly* the same way.

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63 A good example is laxing before the suffixes +ic, +id, +ish, cf. SPE:241.

64 The deletion of *g* before nasals in word-final position (e.g., *resign, paradigm*) is a good instance for the triggering effect of a word boundary (in fact, it is spirantisation first, deletion second, but this is a matter of detail here — see SPE:234, 241). Glottalling of *t* before, e.g., *n*, can be a good example for a process that operates across a # as well, cf. *Pu[i?]ney, ho[i?]ness*.
Let us now turn to the final point in our discussion of the SPE model: what is the domain of phonology? That is, which alternations are to be handled by phonological rules, and which are treated by non-phonological mechanisms? Recall the Unique Underlier Condition, stated above in (26), repeated here as (30) for convenience:

\[(30) \text{The Unique Underlier Condition (UUC)}\]

Every non-suppletive alternation is to be accounted for by assigning to each morpheme a single, phonologically specified underlying representation, with the allomorphy derived by general (preferably phonologically specified) rules (emphasis original).

The question as to what belongs to the domain of the phonological component boils down to a decision as to what non-suppletive means. In traditional discussions, the term suppletive has been applied to instances such as go ~ went, where the two roots represent historically different lexemes. Synchronically, though, the question is by far not as simple as that. For one thing, very ancient items can have alternants which have — due to historical reasons — become so different in their phonetic shape that it needs proper historical investigation to show that they are derivable from a common historical source; witness E am and is, from PIE *es-, which must be regarded as suppletive in present-day English (synchronically, that is). The other (more difficult) problem is that there are alternants which are fairly close phonetically, such as mouse ~ mice or sing ~ sang, but are clearly not “regular”. Take as an example the English verbs ride and hide. The former’s preterite is rode, but the latter’s is hid, though there’s absolutely no relevant phonetic difference between the two verbs (apart from the initial consonant, but that can hardly be a reason for their different behaviour) to enable one to predict this difference. In other words, the divergent past formation is a matter of lexical information: it simply must be learnt. A similar problem is presented by a pair such as bring and sing (preterites brought and sang, respectively).

To take a less radical example, let us revisit verbs of the keep ~ kept type. Are these pairs suppletive or not? The answer lies in the extent to which we regard them as “regular”. Now, in a certain sense, of course, such items are irregular inasmuch as they do not follow a productive pattern — and I think no one would consider them to be regular in this sense. The question of regularity, however, concerns the relationship of such items with the phonology as well. Notably, can the alternants be derived by phonological processes from a common underlier?
The SPE position, outlined above, is that they can (see (29)). Note, however, that in order to make this claim, i.e., that a phonological derivation of this sort is possible, one must assume the following: (i) underlying (as well as intermediate) representations may be rather abstract (= quite removed from the surface forms), (ii) the derivation consists of the ordered application of language-specific rules, (iii) rules may be triggered or blocked by lexical labels. All these assumptions are present in the SPE model. Indeed, SPE appears to adhere to a rather radical interpretation of what phonology is: wherever there’s some phonological regularity, one assigns it to the phonology, even in extreme cases where the phonological conditioning is minimal or even virtually non-existent. A case in point is provided by strong (and some weak irregular) verbs in Modern English. In SPE, alternants such as break ~ broke, buy ~ bough(t), find ~ found, tell ~ tol(d), etc. are derived from a common lexical representation via readjustment rules and phonological rules. For example, the verb BIND is lexically represented as /bi:Nd/, from which a (pre-cyclic) readjustment rule creates the phonological (underlying) representations for the present and the preterite. The rule in question is formulated in (31):

\[ V \rightarrow [\text{back}] [\text{round}] \]

in a number of irregular verbs, nouns, and adjectives in certain contexts.

This rule applies to the preterite form, producing /bu:Nd/. The phonological representation for the present and the preterite are, therefore, /bi:Nd/ and /bu:Nd/, respectively; the phonological rules apply to these forms, yielding the surface (phonetic) forms [baind] and [baund] (SPE:209).

The example above serves to illuminate two important points. First, SPE clearly does not treat pairs of the bind ~ bound type as suppletive: as the alternants are phonetically similar, and the similarity follows a more general pattern\(^65\), SPE assigns a common lexical representation to them. Second, it must be emphasised that the pair is nevertheless different from the keep ~ kept type. Whereas the root allomorphy in the latter case is derived via phonological rules proper, this is not the case with bind ~ bound, because here, a readjustment rule is used as well. The status of readjustment rules is by far not unambiguous in SPE, and it is worth quoting Chomsky and Halle’s own formulation from Chapter 5 (entitled “Summary

\(^65\) I.e., there are other items that follow a similar or even identical pattern, as shown by the formulation of the rule itself.
of rules”) of SPE. First, they say, “In this chapter we restate the major rules of the phonology” (SPE:236), then they go on to say that

The rules that we have given fall into two general classes: the rules of the readjustment component and the phonological rules. The former apply before any of the phonological rules. They express properties of lexical formatives in restricted syntactic contexts, and they modify syntactically generated surface structures in a variety of ways (ibid.; emphasis mine).

The status of readjustment rules is ambiguous for the simple reason that in a sense they are part of the “rules of the phonology”, yet in another sense, they are not “phonological rules”. As their exact status is very important in deciding what constitutes the domain of the phonology, it is inevitable to clarify some points.

The conception of grammar as envisaged in SPE (and classical generative grammar in general) is that it consists of three modules: the syntax, the phonology, and the semantics. There is no module corresponding to the traditional notion of “morphology”: its roles are assigned either to the syntax or the phonology. The syntax provides lexical representations, which are modified by readjustment rules, so that these representations could serve as inputs to phonological derivations. The point is that readjustment rules are not syntactic rules: they apply to syntactic surface representations. If, however, readjustment rules are not syntactic rules, what are they? As there’s no morphology, they can only belong to the phonology. Furthermore, their form and content is identical to phonological rules proper. Take, for example, the rule of Backness Adjustment in (31). It looks just like any other phonological rule both in its form and its content: if we look at it, there’s no way to tell if it is meant by the particular analysis of English as a readjustment rule or a phonological rule proper. It is only because we are told that it is a readjustment rule that we know where it belongs. As readjustment rules use phonological formalism and phonological vocabulary, and they apply to syntactic surface forms, we must regard them as part of the phonological module. This is in fact implied by the formulation given above by Chomsky and Halle (“In this chapter we restate the major rules of the phonology”). That is, the “rules of the phonology” are not the same as the “phonological rules”, the latter being a proper subset of the former.

66 And, of course, the lexicon, but it is included in the syntax (SPE:7).
67 See, for example, the nine readjustment rules listed in Chapter 5 of SPE (pp. 238f).
All this is, of course, extremely important, because ultimately, alternants such as *bind* ~ *bound* are related to each other by the phonology, even if not in the same strict sense as *keep* ~ *kep(t)* or *te[th] them* ~ *te[l] us*. Put differently, the SPE model assigns a very wide role to the phonology, letting it account for almost all alternations except cases of extreme suppletivism (where no phonetic similarity *at all* is detectable). The rationale behind this view is that the grammar must capture as many generalisations as possible; and, because there is no other possible candidate, phonology must do the job.

To illustrate how wild the theory can go let me mention Lightner’s proposal (1978:18), according to which pairs such as *heart* ~ *card(iac)*, *three* ~ *tri-, fear* ~ *per(il)*, etc. could be treated phonologically by adopting Grimm’s Law as a synchronic phonological rule of Modern English. Absurd though such a solution may seem to contemporary linguists, the theory does permit it, and indeed, historical information is often replicated in SPE-type derivations (including SPE itself, where many rules “repeat” historical changes such as the Great Vowel Shift or *x*-dropping). Whether the reflection of a historical change can be regarded as a synchronic rule is, of course, a different question than abstractness itself, but the example illustrates, I hope, that the SPE model of phonology is a very powerful one indeed.

2.4. [Summary]

To sum up, we have seen that the SPE model is characterised by the following features that are relevant for subsequent discussion:

1. The rules of phonology apply to linear strings of feature matrices in an ordered fashion.

2. Morphological information is represented in the phonology by way of boundary markers, themselves being feature matrices, but their identity is non-phonological: they are not segments, and they are inserted by the syntax into the string.

3. The position of SPE with regard to alternations is an extreme one: virtually all alternations are accounted for by the rules of the phonology (except clear cases of total suppletion).

4. The previous point shows that the model aims at “regularising” as much as possible.

Let us now turn our attention to Government Phonology, its basic tenets and its view on what belongs to the phonology and how morphological information is represented in the phonology. This is the topic of the next chapter.
3

Essentials of CVCV Theory

This chapter introduces the basic tenets of Government Phonology. We will then discuss some of the claims of the version of Government Phonology known as Strict CV or CVCV. Much of what the theory has to say about phonology bears no direct relevance to our discussion; such bits and pieces of the model will not be discussed, or will only be mentioned briefly. It must be emphasised that I do not, because I cannot, set up the goal of justifying the CVCV model in its entirety: such an enterprise is beyond the scope of this thesis, and it is unnecessary as well, given that by now, a number of articles and books are available which do precisely that. The reader is referred to the items referred to in this chapter. What I offer here is an overview, as well as some illustrative examples.

3.1. [Government Phonology]

Government Phonology is a theory — or, rather, a group of related theories — developed by scholars associated primarily with the University of London during the 1980’s and the early 1990’s (Kaye, Lowenstamm and Vergnaud 1985, 1990, Kaye 1990, Charette 1990, 1991, Harris 1990, 1992, 1994). The theory starts to develop variants early enough, and it hardly makes sense nowadays to talk about Government Phonology (GP) as a single, unified model, although it is true that there is a significant set of underlying “guiding” principles which all variants share. It is for this reason that I prefer to call GP a “group of related theories”, and when I talk about GP in general I refer to the common core assumptions of the various models.

Perhaps the most significant innovation within GP is the introduction of Strict CV theory by Lowenstamm 1996, subsequently developed in various ways by a number of authors (Lowenstamm 1999, Ségéral and Scheer 2001, Scheer 2000, 2004a, Szigetvári 1999, Dienes and Szigetvári 1999, Balogné 2004, Csides 2002, 2004, etc.). I will first discuss the generally accepted tenets of GP, then I consider Strict CV in detail, primarily because this is the model I will use in later chapters.
GP differs in a significant sense from earlier generative phonological theories inasmuch as it assumes the “Principles and Parameters” approach developed in syntactic theory during the late 1970’s and 1980’s. The essence of this approach is that Internal language (I-Language) consists of (i) universal principles governing linguistic structure, (ii) parameter settings, i.e., choices between options made available by Universal Grammar. I-Language, then, can be defined as follows (taken from Uriagereka 1998:36):

(32) A definition of I-Language
Given a set of universal principles encoded as the initial state \( S_0 \) of an idealized language faculty, and a set of parameters of variation for \( S_0 \), an I-Language \( L \) is a complete specification \( S_f \) of parametric options.

Put differently, the grammars of individual languages differ in parameter settings: the principles are universal. What relevance does this have for the phonology and, more specifically, for the topics we are concerned with?

If phonological systems are governed by the same principles and vary along parameters only, the powerful machinery that the SPE-type phonological component is will be simply ruled out. We will see in detail why this should be so in the next chapter; at this point, let me illustrate it with one very clear example. Consider the SPE analysis of the alternants *bind* ~ *bound*, derived from a common lexical representation. The same analysis is not possible in a Principles and Parameters framework: such highly language (and item!) specific rules are hardly possible candidates for universal status\(^6\).

The basic tenets, or ground rules, of GP as laid out in Kaye, Lowenstamm & Vergnaud 1990 (henceforth KLV) are as follows (KLV:194):

(i) **Privativeness**: “Phonological oppositions that are privative at the level of lexical representation remain privative at all levels.” [Emphasis original.]

(ii) **Universality**: “The set of available phonological processes behaves like a function mapping initial representations onto final representations”.

(iii) **Non-arbitrariness**: “There is a direct relation between a phonological process and the context in which it occurs.”

Let us see in some detail what these tenets mean and what their consequences are.

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\(^6\) GP does not claim, of course, that an alternation between, say, \( \text{ai} \) and \( \text{au} \) can *never* be phonological: it is perfectly possible to imagine a language in which this alternation is accounted for by the phonology, but this requires that the alternation be phonologically transparent, i.e., a *phonological cause* must be present; see the Non-arbitrariness principle, to be introduced shortly.
First, privateness. An opposition which is privative à la Trubetzkoy behaves in such a way that the marked value in the opposition is present in the representation, while its unmarked counterpart is simply not there. There is no possibility of turning such oppositions into equipollent ones: values are expressed by unary primes called elements in GP, which are either present or not. Take voice, for example. There is a prime, let’s call it VOICE\textsuperscript{69}, which is present in voiced segments; voiceless segments lack it. This contrasts sharply with the binary features employed in SPE (and several other theories). A consequence of this stance is that you can’t underspecify, harmony processes are univalent, and, as KLV say “you can’t spread something that isn’t there”. In other words, it is impossible to refer to the distinction between voicelessness and the lack of any specification for voicing\textsuperscript{70}.

Second, universality. This is perhaps the most mysteriously formulated tenet. What it really means, as far as I can see, is that lexical (initial) representations are not different in kind from phonetic (final) ones: both are directly interpretable, hence lexical representations are in no way “abstract”. An important consequence is that “the same physical object will receive uniform interpretation across phonological systems” (KLV:194), i.e., a representation, whether initial or final, is not only directly interpretable, but it will receive the same phonetic realisation in any language. A further important point is that the word mapping is used, rather than derivation: in GP, there is no derivation in the SPE sense (that is, there are no extrinsically ordered rules). This, however, does not mean that GP forbids derivations, but it employs them in a restricted manner\textsuperscript{71}.

It must be emphasised that the above tenets are “ground rules” which are adhered to by GP-ists, rather than principles in the “Principles and parameters” sense. I now proceed to describe the most important principles and parameters assumed by the theory, i.e., how the above ground rules are realised in the model.

GP places a heavy burden on representations. As such a theory, it employs non-linear autosegmental representations. In the classical version of GP, a unitary skeletal tier is used, consisting of timing slots (represented by X’s), hosting both consonantal and vocalic

\textsuperscript{69} What exactly the prime corresponding to “voice” is in GP need not concern us here; see, for instance, Harris 1994:135.

\textsuperscript{70} This does not mean that GP does not recognise Trubetzkoyan equipollent oppositions: it does, but it encodes them in the lexical representation. For example, vowel height oppositions are expressed by elemental makeup and headedness (see below). The Privativeness principle simply says what it says: privative oppositions are not treated as equipollent ones at any level.

\textsuperscript{71} See Kaye’s model using domains and serial application of the phonology in Chapter 4.
segments; in Strict CV, the skeleton is made up of CV units, where C positions host segments with consonantal properties and V positions host vocalic ones.

The internal structure of segments is characterised by internal organisation, i.e., segments are not composed of unordered primes but are linked to their respective positions via a root node in a geometrically arranged fashion, residing on several tiers. As mentioned in the previous sections, the primes employed in the theory are not features but elements. These differ from SPE-type features in two ways: first, they are unary, as I have already pointed out. Second, they are independently realised. Indeed, it is possible to have a segment containing one element only. What elements are employed shows significant variation in the works of GP scholars, and the present thesis is not concerned with this topic in a direct way, so I will not present a full theory of elements, and I give a simplified account for purposes of illustration. I limit the discussion to vowels only, chiefly because they are much less controversial than consonants.

The chief elements for vowels used in virtually all versions of the theory are A, I, U and @. The first three represent, by and large, the three corners of a triangular vowel space, in which A stands for sonority (aperture), I represents palatality (i.e., the high front corner of the space), while U stands for labiality (and, when not combined with I, backness as well). Accordingly, the independent phonetic realisations of these elements are ə, i, u, respectively. The fourth element, @, is used in different ways in the various versions of the theory. What is common to all versions is that its independent realisation is a neutral (central unrounded) vowel, i.e., the schwa (or ə). There are two basic views on its role. The first one is that it is just like any other element: it is either present in the segment or not (this is probably the majority view). The second one, associated primarily with John Harris and Geoff Lindsey, is that it is present in all vowels, being an “identity element”, which behaves like a “blank canvas” upon which the other segments are “painted”; but, unless it is assigned head status within the segment (see the next paragraph for headedness), it remains inaudible (Harris 1994:108f, 180f, Harris & Lindsey 1995:59ff). This interpretation of the identity element is crucial for them to account for the realisation of lexically empty nuclei.

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72 This is reminiscent of Clements & Keyser’s (1983) CV phonology, though not identical: Clements & Keyser define V positions as being associated with syllabic peaks, C’s being associated with non-peak positions, and they recognise a syllable constituent.
73 In this respect, GP resembles Dependency Phonology (see Anderson and Ewen 1987).
74 There is no element in most versions of the theory for “backness”, capturing the insight that in an unmarked case, backness involves roundedness, too, though see Scheer 1999:208ff who argues that “velarity” and “roundedness” should be represented separately.
Elements can, of course, combine within a segment. For example, the combination of A and I yields front non-high unrounded vowels. The complex expression\(^{75}\) can be headed, in which case one element acts as the head and the other is the operator. (If several elements combine, there are several operators; of course, there can only be one head.) In a headed expression, it is the head which determines the basic property of the segment, modified by the operator. For example, A.I\(^{76}\) defines e, while I.A is interpreted as a or æ\(^{77}\). An unheaded combination (= AI) is a mid vowel, i.e., e. The same goes for back vowels, so, for example A.U = o, etc. The neutral element, when it heads an expression, is responsible for non-ATR articulation; for instance, while I = i, I.@ = i\(^{78}\).

In many languages, one finds a three-vowel system consisting of a low vowel (a ~ ø) plus u and i. In such languages, the theory claims, the three elements reside on the same autosegmental tier (that is, there is but one tier\(^{79}\), hence they cannot combine: within a segment, there is only one available place on a single tier. In languages with mid vowels, two tiers are available, hence A can combine with either I or U, but not with both. Such languages (such as English) do not possess front rounded vowels. In order for a language to have them, the three elements must be assigned different tiers (e.g., Hungarian, Old English, etc.); here, for example, IU = y, AU.I = ø, etc\(^{80}\).

\(^{75}\) For those readers who are not familiar with GP, may I note that the term *melodic expression* is standardly used in the theory to refer to segments.

\(^{76}\) I use the standard practice of indicating the head by writing it last and placing a dot before it.

\(^{77}\) There is no way to distinguish the two, and it is a matter of language-specific variation whether the expression is interpreted as a or æ. This seems to contradict the principle of Universality, according to which the same physical object will receive a uniform interpretation in all languages. Nevertheless, the phonetic difference between these two vowels is slight, and probably there is no language in which the two would contrast. Therefore, such a small degree of variation in phonetic realisation is presumably permissible.

\(^{78}\) In fact, it is possible to state the difference between e and ø (or o and ø) in a parallel fashion, i.e., to assume that ø is not IA (unheaded) but IA.@. This treatment is found, e.g., in Harris & Lindsey 1995:62ff, who, of course, assume that @ is present in all expressions. I do not make this assumption, however, which is why I represent mid-low vowels as unheaded combinations. Under this interpretation, then, @-headedness means centralisation and is only possible in vowels that occupy a non-peripheral position in the vowel space; ø (= IPA Cardinal Vowel № 3) is clearly not centralised. At any rate, this issue is not relevant for this thesis.

\(^{79}\) If one adheres to John Harris’ interpretation of the function of @, it has its own tier, of course, being present in all expressions.

\(^{80}\) It is interesting to note that there appears to be no phonetic difference between UI, U.I and I.U. Yet, the theory predicts that an I-headed y can behave differently from an U-headed one *phonologically*, and this prediction is indeed borne out. In Icelandic, for example, front rounded vowels do not palatalise velars but front unrounded ones do; in Canadian French, on the other hand, both high front segments (= i and y) affricate a preceding dental plosive. In Canadian French, therefore, y behaves in the same way as i, unlike in Icelandic. One can explain this difference by assuming that y is U-headed in Icelandic but I-headed in Canadian French.
3.2. [Prosody: CVCV]

The next question to be addressed is prosodic organisation. It is this aspect in which Government Phonology represents the most radical break with earlier tradition, most notably, in denying the status of the syllable as a phonological constituent and the extensive use of empty positions. The Classical GP (CGP) view on constituency and principles of prosodic organisation will not be discussed here, since I will not use this version of the theory in the present paper, and it would unnecessarily complicate the discussion. In what follows, I outline the version known as Strict CV or CVCV.

It must be emphasised right at the outset that CVCV\(^81\) is not a unified model: it has several variants, and several basic concepts are interpreted differently by different authors\(^82\). Nonetheless, it is possible to give an outline of what represents, by and large, a kind of “common ground”. Where divergencies are found, I will follow Scheer’s version described exhaustively in Scheer 2004a.

CVCV theory is a radical offspring of GP. Already in CGP, syllabic constituency is significantly curtailed, as the only constituents the theory recognises are the Onset, the Nucleus, and the Rhyme; neither the Coda nor the Syllable itself is granted theoretical status. In his 1996 paper, Jean Lowenstamm makes the radical claim that even this simplified tree structure is superfluous, stating that (Lowenstamm 1996:419)

\[(33)\text{ Syllable structure universally, i.e., regardless of whether the language is templatic or not, reduces to CV.}\]

Let us now see the details.

The skeleton in CGP (as in several other theories) consists of X slots (timing positions); in such theories, whether a slot is likely to host a consonant or a vowel depends on which constituent dominates it, i.e., X’s dominated by the Nucleus are ideal hosts for vocalic expressions, other slots will ideally host segments with consonantal properties. Now, if all syllabic constituents are eliminated, such a solution is not possible. As a result, CVCV replaces X positions with C and V positions in such a way that the skeletal tier consists of

\(^{81}\) I will use the term CVCV from now on.

\(^{82}\) Dienes and Szigetvári 1999, as well as Szigetvári 1999, for example, use a version in which the skeleton is made up of VC rather than CV units. Nonetheless, VC Theory shares the majority of the organising principles of phonology with other versions.
strictly alternating CV pairs. Crucially, this holds for the representation of all words in any language, including those which exhibit clusters of consonants or vowels, including long segments. To illustrate what this means, let us give a simplified preliminary representation of the English words *America, trend,* and *see,* as well as the Italian word *latte* ‘milk’:

\[
\begin{align*}
&\text{C V C V C V C V} \\
&\text{C V C V C V C V} \\
&\text{C V C V} \\
&\text{C V C V C V}
\end{align*}
\]

\[
\begin{align*}
&\text{t r e n d} \\
&\text{a m e r i k a s i} \\
&\text{l a t e}
\end{align*}
\]

The proposal in (33) has several important consequences (cf. also Lowenstamm:420ff):

1. The fact that CV (surface CV, that is) is the universally unmarked syllable type receives an explanation. In other words, the unmarked case is that a language requires all C and V positions to be filled by melodic material. It is well known that a number of languages are of this type. Furthermore, languages which do tolerate consonant clusters and long segments, word-initial vowels, or word-final consonants, will allow CV syllables, too. The difference boils down to whether, and in what contexts, the language tolerates empty positions. A theory which uses (non-universal) arboreal syllable structures fails to shed light on these facts: it does not explain why CV is universally well-formed, and, worse still, makes the (wrong) prediction that there could be languages without CV syllables.\(^{83}\)

2. Syllable structure becomes universal on the skeletal level. For a “Principles and Parameters” approach, this is more than welcome. The skeletal organisation of phonological representations is deprived of any language-specific variation, and differences between individual languages relate to the distribution of empty positions.

3. One of the crucial differences between syntactic and phonological structures falls out automatically as a consequence of doing away with arboreal structures. As has been noticed (but not explained) for long, syntactic structures are recursive, but phonological ones are not: you can embed an NP into an NP, but you cannot embed a Syllable into a Syllable or an Onset into an Onset, etc. With CVCV, this is logical: recursivity assumes arboreal structures.\(^{84}\)

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83 Or, in order to avoid making this prediction, extra principles such as Preference for Onsets, etc., must be stipulated.

84 This has been pointed out to me by Tobias Scheer (personal communication).
4. Syllabification for all words in a language becomes identical. Much criticised and problematic notions such as ambisyllabicity and resyllabification lose their relevance. An important consequence of this is that a basic requirement of (all versions of) GP is fulfilled: that of Prosodic Structure Preservation, stated in (35) below (taken from Harris 1992:366):

(35) Prosodic Structure Preservation

Conditions on prosodic structure holding of lexical representations also hold of derived representations.\(^{85}\)

Let us now turn to the principles which govern the distribution of empty positions.

A theory which allows empty positions must set up clear principles governing their occurrence. An unlimited proliferation of empty categories dangerously increases the power of the theory — one can express almost anything (and its reverse), whether found in natural language or not. It goes without saying that this is more than undesirable: such a scientific theory is not a scientific theory.

The distribution of empty V positions is (partly\(^{86}\)) restricted by the following principle:

(36) Empty Category Principle (KLV:219)

A position may be uninterpreted phonetically if it is properly governed.

Proper Government is an asymmetrical relation holding between two positions \(V_1\) and \(V_2\), where the following conditions must be satisfied:

1. The governing relation is right-headed: \(V_2\) is the proper governor, \(V_1\) is the governee.
2. \(V_1\) and \(V_2\) are adjacent on the level of V positions, i.e., no third V may intervene.
3. \(V_2\) may only properly govern \(V_1\) if it is not properly governed itself (i.e., if it is phonetically filled).

---

\(^{85}\) This principle is not specific to GP, but, as Harris himself says, “it is assumed with varying degrees of explicitness” in some other approaches, too, mentioning Selkirk 1982 and Itô 1986. He goes on to say that “the effect of the principle is to prevent phonological processes from creating syllabifications which violate lexically established well-formedness conditions on prosodic structure” (1992:366).

\(^{86}\) There are two other mechanisms I will make use of: Licensing of Final Empty Nuclei and Infrasegmental Government.
Before illustrating PG with examples, let us return to the problem of word-final\(^87\) empty V positions, or, to use a different term, Final Empty Nuclei (FENs). Obviously, PG cannot account for their remaining silent, as there is no available governor. Instead, it has been assumed that FEN are parametrically licensed to remain silent (this parameter will be revisited and revised in Chapter 4):

\[(37) \text{Word-final Empty Nucleus parameter: ON/OFF}\]

That is, languages may set the parameter “ON”, in which case words can end in a surface consonant (i.e., an empty V), or “OFF”, in which case all words must end in a (pronounced) vowel.

Let us now see an example. As we will see, PG is an ideal tool to account for vowel ~ zero alternations as well, observed in a range of languages and following a remarkably similar pattern. Consider the English words \textit{rhythm} `\textdiakrytma` and \textit{rhythmic} `\textdiakrytimik`; I use lowercase \(v\)’s and \(c\)’s to indicate lexically empty positions\(^88\).

\[(38)\]

\[(a) \quad \begin{array}{cccc}
\text{PG} & \text{c} & \text{v} & \text{C} & \text{V} \\
\text{PG} & \text{C} & \text{v} & \text{r} & \text{i} \\
\text{PG} & \text{C} & \text{v} & \text{\textdelta} & \text{\textomega} \\
\text{PG} & \text{V} & \text{C} & \text{v} \\
\text{PG} & \text{\textomega} & \text{C} & \text{v} \\
\end{array}\]

\[(b) \quad \begin{array}{cccc}
\text{PG} & \text{c} & \text{v} & \text{C} & \text{V} \\
\text{PG} & \text{C} & \text{v} & \text{r} & \text{i} \\
\text{PG} & \text{C} & \text{v} & \text{\textdelta} & \text{\textomega} \\
\text{PG} & \text{V} & \text{C} & \text{V} \\
\text{PG} & \text{\textomega} & \text{C} & \text{V} \\
\text{PG} & \text{\textomega} & \text{C} & \text{v} \\
\end{array}\]

Let us see these words in detail. This will also give us the opportunity to discuss the phonetic realisation of empty vocalic positions.

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\(^87\) In fact, \textit{domain-final} is the appropriate term rather than \textit{word-final}, but I will use the latter for the time being until the concept of domains is introduced.

\(^88\) Note: in the case of \textit{rhythm}, there is another possibility: the \(\text{m}\) can spread into the preceding V position, acquiring syllabic function. The use of lowercase letters to indicate empty positions originates from Szigetvári 1999.
Both words end in a lexically empty \( v \), which in English is parametrically licensed. In (38a), the preceding vocalic position (encircled) is empty, but the final \( v \), being itself licensed to remain silent, cannot properly govern it, hence the encircled \( v \) receives phonetic interpretation: it surfaces as a schwa (for why a schwa, see shortly). In (38b), the encircled \( v \) position is followed by a lexically filled, ungoverned \( V \), which acts as a proper governor: as the encircled \( v \) is properly governed, it must remain silent.

Note from (38) that an empty \( cv \) unit is posited word-initially. According to most proponents of CVCV, words begin with an empty \( cv \) unit in English, and in many other languages including Hungarian. One reason for positing an empty \( cv \) pair is to account for the ungrammaticality of clusters such \( dl, tn, kt, lt \), etc. in word-initial position. E.g., *dlin is not well-formed; crucially, this fact cannot be explained without reference to an initial empty \( cv \), since the \( l \) could in principle serve as a proper governor for the \( v \) between \( d \) and \( l \), cf. Maudlin 'mædlin. (Cf. Hu katlan ‘crater, cirque’, but no tl-initial words.) The ungrammaticality of *dlin &Co is due to the fact that the \( v \) of the initial \( cv \) pair requires PG, and the \( v \) trapped between the first two C’s cannot act as a proper governor, itself being properly governed by the following full Nucleus.\(^{89}\)

Let me now address the problem of the phonetic realisation of empty \( v \)’s. Various proposals have been put forward as to how \( v \)’s receive phonetic content. Harris, for example, proposes that the “identity element”, i.e., \( @ \), lodged in all vocalic positions (Nuclei), whether empty or full, is the element that surfaces if the position is not properly governed. The difference in this framework between an empty nucleus and a full one is that the former contains a non-head identity element only, whereas the latter either contains further elements or else a \( .@ \), i.e., the identity element which acts as a head (while being the only element in the expression). He illustrates this by pointing out that there are pairs in English like dine vs. Dinah, the latter ending in a schwa. This schwa, he claims, cannot be the realisation of an empty nucleus, since FENs are parametrically licensed in English. His solution is that the last nucleus of Dinah contains \( .@ \), which makes it a lexically full nucleus. He argues that in the majority of languages, vowels alternating with zero (= empty nuclei), as well as “default” vowels, are typically central ones (English, European Portuguese, French, etc.), except if Vowel Harmony is operative in the language, whereby the stressed nucleus may spread one or some of its

\(^{89}\) Another reason for postulating an initial empty \( cv \) is to explain why consonants in \( cv \) initial languages do not generally lenite word-initially. The details of the full argumentation are complex and largely irrelevant here; see, e.g., Scheer 2001. Szigetvári 1999:91ff presents an alternative analysis based on the idea that the skeleton is made up of VC, rather than CV units.
elements into the empty position, producing *predictable* variation in the phonetic manifestation of empty nuclei (see Harris 1994:180ff).

Aside from the fact that Harris’ proposal that two simplex expressions can contrast only in one containing a single non-head element while the other containing the same single element as a head may be controversial, there is another, empirical, problem. In several languages, alternating/default vowels are different. In Czech, for instance, it is \( \varepsilon \), in Russian, \( \varepsilon \) or \( \text{o} \), in Brazilian Portuguese \( \text{i} \), in Serbo-Croat, \( \text{a} \). How such a range of different vowels, scattered all around the vowel space, could realise the same element remains a mystery.

Another possibility is that empty nuclei are really empty, and they receive phonetic realisation by language-specific epenthesis, a solution originally assumed by CGP. This, however, is still problematic: as Scheer 1997:81 points out, there are languages where “different vowels alternate with zero in identical contexts” [emphasis mine — L.K.], such as Russian, where the alternating vowel is either \( \varepsilon \) or \( \text{o} \), but this is determined lexically. A “blind” epenthesis rule cannot account for this.

Another proposal, which I find more acceptable, is that offered by Scheer (ibid.), which distinguishes two types of consonant clusters. First, there are sequences of consonants which are never broken up by an “epenthetic” vowel, as in English *mend, fault, Patrick, clue*, etc. These, he says, are separated by a nucleus devoid of any melodic content: such nuclei are literally empty. As opposed to this, vowels which alternate with zero are floating segments, lexically unattached to their V position; if the position is not properly governed, the floating melody attaches to it. In case the position does receive PG, it remains phonetically empty, and the floating segment, being unattached to any position, is subject to Stray Erasure\(^90\). See Scheer (ibid.) for details. I adopt this proposal in the present paper. Let us now revisit the case of *rhythm ~ rhythmic*, where the dotted line indicates non-lexical association.

(39)

\[ (a) \quad \begin{array}{cccc}
\overset{\text{PG}}{c} & \overset{\text{PG}}{v} & \overset{\text{PG}}{C} & \overset{\text{PG}}{V} & \overset{\text{PG}}{C} & \overset{\text{PG}}{v} \\
\text{r} & \text{i} & \text{ð} & \text{ø} & \text{j} & \text{m}
\end{array} \]

\(^{90}\) Stray Erasure is a principle used in several models, stating that melody that fails to attach to a skeletal position during the derivation is deleted by general convention. (See Kenstowicz 1994:285f.)
Finally, I present an outline of the chief organising principles of phonological structure in CVCV. In syllable-based theories, the well-formedness vs. ill-formedness of strings is accounted for by postulating maximal constituents (such as “the coda in language L may contain a maximum of \( n \) consonants, or that the nucleus may only contain one (short) segment, etc.), as well as co-occurrence restrictions of various kinds (e.g., in a two-member onset, the two consonants may not share their place of articulation). The chief organising principle of syllable structure and syllabification, however, is the allegedly universal Sonority Sequencing Principle, stated below in (40):

(40) *The Sonority Sequencing Principle* (from Roca 1994:153, his (41))

The sonority profile of the syllable must slope outwards from the peak.

This principle is used to explain why sequences such as \( rt, md, lk, wz \), etc., are ruled out as complex Onsets, for example. Together with the principle of Onset Maximisation (stating that intervocalic consonant clusters are always syllabified so as to gather as many consonants into the Onset of the second syllable as possible) and certain language-specific restrictions, syllable-based theories claim to model the (un)grammaticality of particular strings.

Needless to say, such a “syllable-tree building” procedure is not available in CVCV, where there is no arboreal organisation. For this reason, it is tempting to think that this theory is back where SPE was: it seems to have abolished constituency. Nevertheless, this is only apparent. We have seen a very important principle, that of PG, which restricts the occurrence of empty nuclear positions. PG is a lateral relation, holding between vocalic positions. Indeed, it is but one manifestation of Government, a lateral asymmetrical relation holding between positions on the skeletal level, which, together with another relation, referred to as Licensing, fulfils the function of holding material together and accounting for well-formedness. Both
relations are strictly right-to-left in most versions of CVCV. CVCV, therefore (as we will see shortly), does not abolish constituency: it merely replaces arboreal structures with lateral relations. Let us first see the possibilities as to what positions can govern/license, and what positions their targets can be.

As we have seen, Nuclei can govern Nuclei: this internuclear government is traditionally called PG. If, however, a Nucleus does not govern a preceding Nucleus (because the preceding Nucleus, or V position, is not empty) it will govern its Onset (i.e., the C position before it). That is, in a sequence \( C_1V_1C_2V_2 \), \( V_2 \) governs \( V_1 \), but in \( C_1V_1C_2V_2 \), \( V_2 \) governs \( C_2 \). It is important to note, however, that the governing potential of Nuclei is seriously limited: only phonetically expressed (non-empty) nuclei may govern. This is something we saw in connection with PG (an empty Nucleus cannot properly govern a preceding empty Nucleus); as PG is but a subcase of Government, it is no wonder that empty V positions cannot govern a preceding C, either. (There is one exception: FENs, more on which below.) Consonants can under no circumstances govern Nuclei.

Let us now turn to Licensing, a notion well known in one interpretation or another before the advent of Government Phonology. Essentially, "Phonological Licensing is a process whereby a constituent or a segment (possibly via its constituent) receives support from another segment. The melodic interpretation of segments or constituents that fail to be licensed is narrowed or null" (Scheer 2004a:134). In CVCV, Licensing is assumed to be operative between (i) a Nucleus and its Onset, (ii) a Nucleus and another Nucleus. Consonants, as with Government, are always passive in relation to Nuclei: a C cannot license a V. The licensing potential of Nuclei (just like their governing potential) is subject to an important condition: only phonetically expressed nuclei may license. (Except, as the reader may already suspect, FENs. We’ll come back to this problem shortly.)

A full Nucleus always licenses its Onset, and it may also license a preceding Nucleus. As this aspect of Licensing is important for us, I describe the situation in detail. Recall that long vowels are represented in CVCV as a vocalic segment occupying two V positions separated by an empty \( c \). In fact, a claim of CVCV is that the segmental content of a long

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91 Though see Csides 2004 for a CVCV model accommodating bidirectional government.
92 In fact, lateral relations are extensively used not only in CGP, but even in traditional syllable-and-sonority based models, even if they do not use the term “lateral relation”. I am thinking of the use of a relative sonority difference between adjacent segments to determine well-formedness. A statement such as “in an Onset cluster, the first member must be less sonorous than the second” expresses an implicit lateral relation (see also Scheer 2004a:238).
93 Please remember that PG is local, i.e., when we talk about a preceding Nucleus, we always mean an immediately preceding one, i.e., there can only be a single C between the two V’s. Another note: the terms Nucleus and V position, of course, mean the same in this framework.
94 See, e.g., Itô 1986.
vowel is lexically lodged in the first V position, while the second V position is lexically empty\(^{95}\); the vowel surfaces as long because the melodic content spreads into the second position, as in (41) below; note that the dotted line, as usual, indicates spreading (non-lexical association)\(^{96}\):

(41) The representation of long vowels in CVCV, e.g., a\(^{97}\)

\[
\begin{array}{c}
V_1 \quad c \quad V_2 \\
\hline
a
\end{array}
\]

In several languages, including English, long vowels are subject to a distributional constraint: they are banned in closed syllables\(^{98}\). This phenomenon has been called *Closed Syllable Shortening*: vowels must be short before a Coda. It is (partly) this phenomenon that is explained with reference to Internuclear Licensing in CVCV.

The point is that in languages which forbid long vowels in closed syllables, the second V position, which is lexically empty, must be licensed to host melodic material. As Licensing (just like Government) is a right-headed relation, it can only come from the Nucleus to the right of V\(_2\). We know that only full Nuclei may License. In CVCV, a traditional “Coda” consonant equals one which is not followed by a full Nucleus (i.e., phonetically, it’s followed by a consonant or nothing). Schematically (using hypothetical examples):

(42) (a) a:ka = OK

\[
\begin{array}{c}
V_1 \quad c \quad V_2 \\
\hline
V_3
\end{array}
\]

\[
\begin{array}{c}
a \quad k \quad a
\end{array}
\]

\(^{95}\) Also of CGP; for an opposing view, see Szigetvári 1999:63.

\(^{96}\) In fact, Scheer (2004a:267ff) argues that it is a matter of language-specific choice whether it is the first or the second position which is lexically associated with the given melody. Languages where melody is lexically attached to V\(_2\) will allow superheavy rhymes: V\(_2\), being lexically filled, (i) needs no licence, (ii) can license V\(_1\). English happens to be of the type described here.

\(^{97}\) As for the empty c position’s ability to remain silent, I follow Szigetvári (1999:62), who argues that “empty C positions need no special care like empty V positions” as C positions are inherently silent.

\(^{98}\) In English, this isn’t quite true, cf. *pöst, kīnd*, etc; it is better to say that they are restricted to a certain type of closed syllable, viz. one closed by a coronal consonant (with some exceptions such as *chāmber*).
The empty $v_3$ cannot license $V_2$; as a result, the $a$ cannot spread into it. This is in line with the basic insight behind Licensing: it supports melodic material.

The careful reader will have noticed, however, that the above analysis seems to fail with regard to two environments: (i) before an obstruent + sonorant cluster (symbolised as TR), i.e., before a cluster traditionally syllabified as an Onset, and (ii) if the “Coda” consonant is word-final ($= \text{a:k#}$). What has CVCV got to offer to explain these “exceptions”?

The former situation is explained in Scheer’s version of the theory with reference to the fact that Onset clusters (TR) exhibit a special type of relation called *Infrasegmental Government (IG)* (Scheer 2004a:37ff), though the name is slightly misleading, because it is a relation that is quite different from the instances of Government described so far. The details are complex and largely irrelevant for us, so I will merely introduce the notion in (43) below:\(^{99}\)

(43) *Infrasegmental Government*

i) In a sequence $C_1v_1C_2V_2$, $C_2$ may govern $C_1$ iff

(a) $C_1$ is an obstruent and $C_2$ is a non-nasal sonorant,
(b) $C_2$ gets license to govern $C_1$ from $V_2$, i.e., its Nucleus, for which $V_2$ must be phonetically expressed\(^ {100}\).

ii) The effect of IG is that $v_1$ is *circumscribed*, and its Empty Category Principle is satisfied: it remains silent.

In other words, in a TvR sequence where IG holds the ECP is satisfied on the melodic level, making the empty Nucleus “invisible” for relations on the skeletal level. An important

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\(^{99}\) See Scheer 2004a:64f; for details, see ibid., 24-67.

\(^{100}\) This is understandable: only full Nuclei may license. Needless to say, word-final empty Nuclei behave specially; but word-medial empty Nuclei, as always, are not licensors.
consequence is that a V following the R may both license and govern a vocalic position to the left of T. This does not only explain why long vowels are possible in a _TRV context (but not, e.g., in _RTV or _KTV\(^1\)), but also why in languages of the English type #TRV is well-formed but *#RTV is not, as illustrated by the following data:

In (44a), we can see that a long vowel is possible before an Onset cluster, because the sonorant can I-Govern the obstruent, creating a situation where the empty Nucleus between them is “circumscribed”; for the skeletal level, the TvR sequence acts as a single C; this is symbolised by encircling it here. As a result, V\(_3\) can license V\(_2\). In (44b), the full V can properly govern the empty Nucleus before the cluster for the same reason: though the v between the T and the R is empty, it doesn’t need PG since its ECP is satisfied by IG. In (44c), the ungrammaticality of initial RT sequences in English (and several other languages) is

\(^1\) I use KT to abbreviate a cluster of two different obstruents, such as ft, pt, kt, etc. (exc. sC clusters, on which see below).
accounted for: as there’s no IG in a RvT sequence, the v inside the cluster does need PG, so it can’t properly govern the v of the word-initial cv pair\textsuperscript{102}.

The second problem is one that we have faced several times: the problem of word-final empty nuclei. We have seen that they behave specially: they may be allowed to license and govern, unlike their word-internal peers. Recall that according to GP, word-final empty Nuclei are parametrically licensed to remain silent\textsuperscript{103}. (If a language licenses final empty Nuclei, it means that words in the language may end in a single surface consonant.) It is a matter of further parametric variation if a final empty Nucleus may govern: if yes, the language has words ending in RT (or KT) sequences; English and Hungarian are of this type, witness E Kent, act, melt, Hu park, bent, akt (glosses: ‘park’, ‘inside’, ‘nude’). It is important, however, that even if FENs are licensed to govern, their governing potential is curtailed: they can only govern a Nucleus which is devoid of any melodic content, including a floating melody\textsuperscript{104}. The third parametric choice is whether a final empty Nucleus may license. This manifests itself in two ways. First, a final empty Nucleus may be allowed to license a preceding V position, in which case the language can have long vowels before final single consonants: English is such a language, cf. keep, food, etc. (Turkish is an example for the opposite, i.e., when long vowels are only free to occur in surface open syllables.) Second, they license preceding consonants. This is primarily manifested in the behaviour of word-final consonants with regard to lenition\textsuperscript{105}.

In the Scheerian model I adopt, the above licensing abilities are not provided to a final empty Nucleus by the phonology: it is the morphology which decides if the morphological object “the end of the word” is projected into the phonology. If yes, it is manifested phonologically as the possibility of having empty Nuclei in that position, even though no phonological mechanism could give them the license to remain phonetically unexpressed; also, they may be given the licence to contract lateral relations, something that is strictly forbidden in the case of word-internal empty Nuclei\textsuperscript{106}. In a language (such as Italian) where

\textsuperscript{102} Scheer (e.g., 2004a:485ff) argues that not all languages mark the beginning with the word with an initial empty cv: instead, this is a parametric choice. Languages such as Russian, Polish, etc., where #RT as well as #TR is well-formed, are the ones in question. The reader is referred to Footnote 97 for how the c of the initial cv pair receives license to remain empty.

\textsuperscript{103} This stance will be revised in Chapter 4.

\textsuperscript{104} Recall that Scheer makes a distinction between alternating vowels (cf. rhythm [a]m ~ rhythmic) and Nuclei that are always silent, i.e., totally empty. FENs may only govern the latter type, hence the appearance of the schwa in rhythm [a]m.

\textsuperscript{105} This aspect of Licensing is not relevant to this thesis, however.

\textsuperscript{106} As internal empty nuclei are not adjacent to any morphological object (boundary), this is understandable: their behaviour is governed by the phonology only. As we will see, the licensing of FEN is a phonological parameter in CGP.
all words must end in a surface vowel, the “end of the word” shows no special phonological behaviour: the morphological information is simply not handed down into the phonology\textsuperscript{107}.

The situation is parallel in the case of “the beginning of the word”: morphology decides if this morphological object is projected into the phonology or not. If yes, an empty \( cv \) pair is inserted. In such languages, word-initial position is different in a phonological sense from word-internal position (e.g., \#RT sequences are illicit); if a language does not mark the beginning of the word by an empty \( cv \) pair, \#RT clusters may appear at the beginning of the word (45) (compare (44c), see also Scheer 2004a:483ff):

\[
(45) \quad \vcenter{\hbox{egin{tabular}{c}
\begin{tikzpicture}
\node[anchor=south] at (0,0) {PG};
\node[below of=PG] (Cv) {C v C V};
\node[below of=Cv] (rka) {r k a};
\node[above of=Cv] (n) {\phantom{C V}};
\draw[->] (PG) -- (Cv);
\draw[->] (Cv) -- (rka);
\end{tikzpicture}
\end{tabular}}}
\]

As there is no empty \( v \) before the sonorant, PG will take care of the empty \( v \) inside the cluster.

The final problem is that of word-initial \( sC \) sequences, as in E stop, spit, sky. The problem is that given the abovesaid, such clusters should not exist: a plosive may not I-Govern, hence the empty Nucleus between the \( s \) and the plosive needs PG from the full Nucleus to its right; but then, the \( v \) of the initial \( cv \) pair fails to be properly governed (recall: that’s why RT/KT sequences are out in English). Yet, such clusters happily exist, and not only in English. The problem has been noticed for long. For traditional syllable-based theories employing the Sonority Sequencing Principle, such clusters are difficult because \( s \) is more sonorous than a plosive. For CGP, they are equally problematic though for different reasons (see Kaye 1992). CVCV cannot give an explanation either, but as Scheer observes, such offending clusters always involve a coronal fricative (most frequently \( s \), but \( \mathbf{f}, \mathbf{z}, \) etc. are also found): other fricatives (labial or velar, for instance) are generally well-behaved. Therefore, Scheer says, it is probably fair to say that the “bad guy” here is \( s \) (\&Co.)\textsuperscript{108}; this may be connected to the fact that coronals tend to be special in other ways, too\textsuperscript{109}. The solution

\textsuperscript{107} I will return to this point in some detail when discussing the CVCV view on the phonology-morphology interface in Chapter 4.

\textsuperscript{108} Scheer 2004a:107.

\textsuperscript{109} It is well known, for instance, that several languages only allow word-final C’s if they are coronal; also, the monstrous word-final clusters found in the Germanic languages (e.g., E sixths) are always coronal ones.
(unknown as yet) must therefore rely on melodic, rather than skeletal relations. Recall that IG is such a relation, a kind of “Melodic Government”, contracted between a sonorant and an obstruent, i.e., melodic expressions rather than between skeletal slots\textsuperscript{110}. In sC clusters, IG isn’t possible, but it is not unrealistic to assume that the empty $v$ inside the cluster is silenced by some similar, so far undiscovered, mechanism\textsuperscript{111}. At any rate, the same problem remains for everbody, so the inability of CVCV to handle the problem is by no means an argument against this particular theory vis-à-vis other theories.

3.3. [CVCV: Two examples]

In the final section of this chapter, I present two analyses in CVCV, in order to illustrate some of the advantages of the model. As I pointed out at the beginning of the chapter, however, I do not wish to argue for all points introduced in the theory.

First, the theory manages to give an explanatorily adequate account of stress placement in mora-counting languages (such as Classical Latin). I consider this to be one of the most convincing arguments for CVCV. It has been noticed long ago that in languages of this type, Coda consonants are “moric”: they contribute to the weight of the syllable, but Onsets never do, no matter how complex they are. Mora-based theories explain the phenomenon by saying that it is because the Coda adds a mora to the syllable. This argument, however, is overtly circular: I observe that Codas count, so I call them moric. Why are they moric? Because they count. Then, when asked the question, why Codas count but not Onsets, I answer, because they are moric. In other words, an observation is turned into an explanation. Consonants sometimes do count, sometimes they do not, and by labelling some of them as “moric” is no explanation.

CVCV handles the situation in a much more elegant way: it is not only observed that Codas count: it is explained. First, the traditional formulation of the Classical Latin stress rule is as follows: (i) stress the penult if it is heavy (contains a long Nucleus or has a Coda), (ii) otherwise stress the antepenult. This accounts for the stressing of words such as habēre ‘have’, arista ‘ear of corn’ vs. ēdere ‘eat’. Let us see how this works in CVCV (see Scheer & Szigetvári 2002, Scheer 2004a:613):

\textsuperscript{110} In fact, it is not the sonorant itself that dispenses government, but its head element, but this is immaterial here; see Scheer 2004a:64.

\textsuperscript{111} In CGP, Government (understood in a rather difference sense than in CVCV) is assumed to hold between the skeletal position of the plosive and that of the $s$, but in that theory, the two are not separated by an empty Nucleus. The problem for CGP is that they must assume an empty Nucleus before the $s$, and they cannot explain why it can remain silent. Kaye (1992) calls the phenomenon “Magic Licensing”, alluding to the fact that we simply do not know. See his article for details.
As it is clear from (46), CVCV unifies stress placement: in all cases, the third-last Nucleus receives stress; furthermore, the observation that “Codas count but not Onsets” receives an explanation: consonants never count, stress is a vocalic property. “Codas count” is an optical illusion: recall that in CVCV, a consonant traditionally assigned Coda status is one which is followed by a governed (empty) Nucleus. It is this Nucleus that counts. No theory which does not assume a strict CV skeleton can achieve the same result. For additional examples as well as some problems (most of which remain for all analyses) see Scheer 2004a:597-623.

I note but one “problem” mentioned by Scheer (2004a:606): the stressing of words like fórmula ‘frame, rule’: here, the third-last Nucleus is the empty one between the r and the m. Hence, stress here falls on the fourth-last Nucleus. As Scheer notes, this is a problem “for all competing theories”. I do not quite see why this is a problem: the empty Nucleus between the r and the m never surfaces, i.e., it’s literally empty. As a result, it is in no way surprising that it cannot receive stress (How could stress materialise on an unpronounced Nucleus?); neither can the preceding r, since consonants never bear stress in Latin. Hence the stress automatically skips this CV pair.
As a second example — anticipating the discussion of Old English — consider the strong\textsuperscript{113} declensional paradigm of the adjective \textit{glæd} ‘glad’ in the West Saxon dialect of Old English (cf. Campbell 1959:264, Wright & Wright 1925:217f)\textsuperscript{114}:

(47)

<table>
<thead>
<tr>
<th>Case</th>
<th>Feminine\textsuperscript{115}</th>
<th>Masculine</th>
<th>Neuter</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sg</td>
<td>gladu</td>
<td>glæd</td>
<td>glæd</td>
</tr>
<tr>
<td></td>
<td>glad</td>
<td>glæd</td>
<td>glæd</td>
</tr>
<tr>
<td>Acc</td>
<td>glæde</td>
<td>glædne</td>
<td>glæd</td>
</tr>
<tr>
<td></td>
<td>glæde</td>
<td>glædne</td>
<td>glæd</td>
</tr>
<tr>
<td>Gen</td>
<td>glædred</td>
<td>glades</td>
<td>glades</td>
</tr>
<tr>
<td></td>
<td>glædred</td>
<td>glades</td>
<td>glades</td>
</tr>
<tr>
<td>Dat</td>
<td>glædred</td>
<td>gladum</td>
<td>gladum</td>
</tr>
<tr>
<td></td>
<td>glædred</td>
<td>gladum</td>
<td>gladum</td>
</tr>
<tr>
<td>Ins</td>
<td>—</td>
<td>glade</td>
<td>glade</td>
</tr>
<tr>
<td></td>
<td>—</td>
<td>glade</td>
<td>glade</td>
</tr>
<tr>
<td>Pl</td>
<td>glade, glada</td>
<td>glade</td>
<td>gladu</td>
</tr>
<tr>
<td></td>
<td>glædra</td>
<td>gladra</td>
<td>gladra</td>
</tr>
<tr>
<td>Gen</td>
<td>glædra</td>
<td>gladra</td>
<td>gladra</td>
</tr>
<tr>
<td>Dat</td>
<td>gladum</td>
<td>gladum</td>
<td>gladum</td>
</tr>
<tr>
<td></td>
<td>gladum</td>
<td>gladum</td>
<td>gladum</td>
</tr>
</tbody>
</table>

As one can see, the root vowel alternates between \textit{æ} and \textit{a}. The alternation is not unique to this item: all monosyllabic adjectives exhibiting a root \textit{æ} in their base (unsuffixed) form behave in the same way. (Nouns, too, show an alternation, but there, the conditioning is different.) Let us see if a phonological generalisation is possible as to the distribution of the alternating vowels. We can say the following: \textit{a} appears if the suffix is vowel-initial, \textit{æ} occurs elsewhere. As far as the phonological environment goes, a linear formulation à la SPE can be given as in (48):

(48) (a) The distribution of \textit{a}:

\[ \_ C + V \]

(b) The distribution of \textit{æ}:

\[ \_ C + \{ C \} \]

\textsuperscript{113} As in other classical Germanic languages, adjectives have two different declensions in OE, traditionally referred to as \textit{weak} vs. \textit{strong}. The terminology is confusing, the two terms also used for verbs (as well as nouns), albeit with adjectives the distinction is not a matter of lexical properties: apart from some exceptions, all adjectives can decline both strong and weak, the choice depending on syntactic factors. The details are irrelevant here.

\textsuperscript{114} The data are given in traditional orthography; all letters here are to be interpreted as if they were IPA symbols, exc. \textless a\textgreater, which is = IPA \textit{a}.

\textsuperscript{115} I depart from the classical order \textit{Masc}—\textit{Fem}—\textit{Neut} not for PC reasons, but because Masculine nouns share declensional properties with both Neuters and Feminines, hence they are better placed in the middle.
Of course, + stands for a morpheme boundary (formative boundary, in SPE terms). May I note that adjectives of this type always have a single C root-finally. The formulation in (48) is observationally adequate, but, unfortunately, includes (in (48b)) the disjunctive {C,#} context.

In classical OE grammars (see, e.g., Wright & Wright 1925:39) the pattern is explained with reference to analogy (in the Neogrammian spirit). In pre-OE, the two vowels involved were in complementary distribution: a occurred iff (i) the following syllable contained a back vowel, (ii) before a nasal consonant; elsewhere, ø was found. Later changes destroyed the neat conditioning; then, the explanation goes, a was “analogically extended” to forms which had a front vowel in the suffix. The problem with this explanation is (as it happens so often with analogical explanations) is that it doesn’t predict which forms are subject to analogy, hence it is not falsifiable. The difficulty with this particular case is that we get no explanation as to why a does not extend to other forms. The GenPl form, moreover, has an ø, though the suffixal vowel is back. The analogical explanation, therefore, is not satisfactory.

In an orthodox syllable theory, one might propose that a is restricted to open syllables, whereas ø is found in closed ones. This seems to explain why a is found in __CV contexts, but ø elsewhere, without using a disjunctive environment. This analysis, however, runs into a serious difficulty: notably, it must assume that the clusters tr, dr (as in the PGen forms lætra, glædra, etc.) are heterosyllabic, i.e., denoting a syllable boundary with a dot, the syllabifications are læt.ra, glæd.ra, etc. The chief problem with this assumption is that these clusters qualify as well-formed Onsets in OE, witness trum ‘strong’, drincan ‘drink’ and numerous other words. This analysis, then, only works if one makes the claim that these clusters are syllabified heterosyllabically in these particular cases (which violates Onset Maximisation), or that the plosive is ambisyllabic. Unfortunately, classical syllabic analyses cannot offer any independent evidence for this, which makes the argument overtly circular.

It is easy to demonstrate that CVCV can accomodate our OE data in a fairly straightforward way. Note that according to this theory, consonants which are final or pre-consonantal on the surface are in fact followed by an empty nucleus, either governed (via PG) or licensed parametrically (FEN). The distribution of ø and a in West Saxon can then be formulated as follows:

116 Other adjectives of this class include bær ‘bare’, hräed ‘quick’, smæl ‘small, tender’, læt ‘slow’, etc.
(49)  (a) The distribution of $\alpha$: before a pronounced Nucleus (i.e., __CV)
(b) The distribution of $\alpha$: before an empty Nucleus (i.e., __Cv)

In (50), I give a CVCV representation for both situations where $\alpha$ is found and for one in which $\alpha$ occurs:

(50)  

This concludes our discussion of the basics of CVCV. Let us turn to how morphological information is encoded in the phonology in this framework.
The aim of this chapter is twofold. First, I discuss the GP (more specifically, CVCV) view on the way morphological information is represented in phonology, i.e., the phonology-morphology interface problem is revisited. Specifically, the Kayean notion of analytic vs. non-analytic domains will be introduced. Together with the phonological representation of word edges (mentioned briefly in the previous chapter), this distinction can account for the differing behaviour of affixation processes with regard to their phonological effects, an issue discussed at some length in Chapter 2. I will point out, however, that the Kayean distinction needs to be re(de)fined. Second, I present the traditional typological classification of affixation as agglutinating vs. fusional, and I consider the (tempting) conclusion that the Kayean analytic/non-analytic distinction is but a reincarnation of this old classification. I will conclude that the two classifications are distinct (even if possibly not totally independent), giving a working definition of fusion (vs. agglutination). It will turn out to be the case, nonetheless, that this definition needs to be supplemented with a definition of phonology. As a result, what one can regard as a fusional concatenation depends greatly on what one assigns to the domain of the phonology. This chapter discusses a great many theoretical notions and offers possible solutions to some problems; as a result, it is somewhat lengthy. Nonetheless, most of it will be of special relevance for the rest of the thesis.

4.1. [Kaye’s model]
The first attempt within GP to formalise morphological effects on the phonology was Kaye’s 1993 paper (later published as Kaye 1995; as usual, I will use this later version for reference). Kaye proposes that morphology can have two effects on the phonology: little or none. Specifically, morphological concatenations are of two kinds: analytic and non-analytic (synthetic), respectively (1995:302). According to Kaye, non-analytic concatenations differ in no way from morphologically simplex forms in a phonological sense: the phonology treats
them as monomorphemic. In such morphologically complex forms, the morphological complexity is not “passed on” to the phonology: the phonology, as it were, cannot “see” that non-analytic forms are complex, and, accordingly, treats them as simplex. Analytic concatenations, on the other hand, come down to the phonology as complex: their morphological complexity is phonologically visible. Accordingly, the Kayean classification, supplemented with the distinction between (a) morphologically simplex and (b) morphologically complex non-analytic forms, can be represented in a diagram form as in (51):

![Diagram](image)

As clear from (51), non-analytic and synthetic are not synonyms: the latter is proper subset of the former. The distinction, however, is not relevant for the phonology, as already mentioned\(^\text{117}\). I indicated the phonologically relevant distinctions in the diagram by encircling them. Let us now see some examples and how exactly Kaye formalises the distinction.

Kaye illustrates the difference with examples from English verbal morphology. Consider the regular forms *dreams* `drɪːmz` and *peeped* `piːpt` first. Such forms, Kaye says, are analytic, which is betrayed by two facts. First, *dreams* ends in a “pseudo-cluster” `mz`, never found in monomorphemic forms. As Kaye’s claim is that synthetic forms display the same phonological behaviour as monomorphemic ones, the presence of this cluster excludes the possibility of *dreams* being synthetic. The same does not hold for *peeped*, whose final cluster is permitted in monomorphemic forms as well (cf. *apt, adopt*, etc.). Nevertheless, *peeped* must still be considered analytic due to the presence of a long vowel in a (surface) closed syllable, more specifically, before a `pt` sequence: long vowels do not occur in this context monomorphemically. The same is true for *dreams*, though here, of course, it is impossible to\(^\text{117}\) Though the division of non-analytic forms into monomorphemic and synthetic ones is not relevant for the phonology, it is relevant for the morphology. I will, therefore, consistently use the term synthetic whenever morphologically complex non-analytic forms are at issue, especially if their morphological complexity needs to be emphasised. May I note that Kaye (1995) himself does not use the term synthetic, but the term has been widely used in the literature in the above sense. I do not know who the use of the term in this technical sense originates from.
tell (empirically speaking) if a long vowel could occur in this particular environment in monomorphemic forms, since the cluster mz, as mentioned, is always heteromorphemic in English.

As opposed to these examples, consider irregular Preterites such as kept and left: both contain a short vowel before a cluster which appears monomorphemically as well, cf. apt, adopt and soft, lift, left_adj, etc. The non-analyticness of these forms has an important result: their morphological complexity is hidden: it is not betrayed by their phonetic shape. We will make this more explicit in the following sections.

Kaye formalises the distinction by assuming that a synthetic form constitutes a single phonological domain. Given a two morphemes A and B, their synthetic concatenation yields the single domain [AB]. Each single domain is shipped off into the phonology as an unanalysable unit. Analytic concatenations, on the other hand, are not single domains. Consider a form like peeped, for instance. The fact that the boundary between the root and the suffix is detectable phonologically reflects the fact that peep constitutes a domain on its own, i.e., a “domain within a domain”: its structure is [[peep]ed], that is, [[A]B]. The entire word is a domain and so is the stem. Note, however, that the suffix is not a domain on its own — there is no pair of brackets including only -ed: put differently, peeped contains two domains, viz. peep and peeped. This is because phonological domainhood is defined in Classical GP (CGP) with reference to stress: a domain must include a stressed Nucleus. Concatenations where both morphemes contain a stressed Nucleus, i.e., are domains, have the structure [[A][B]]. This structure is assumed for compounds (e.g., [[black][board]]) as well as analytic prefix-stem combinations (e.g., [[un][natural]]). Such forms involve three domains, e.g., black, board and blackboard. Note that the analyticness of these concatenations is shown not only by the presence of a stressed Nucleus in both parts, but also by the existence of monomorphemically illicit clusters straddling the morpheme boundary (i.e., kb and the fake

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118 The reader will have noticed that this distinction between types of affixation is in principle the same as what we called, following Haspelmath, integrated (as opposed to neutral).

119 This is a consequence of the GP interpretation of the Phonological Licensing Principle which requires that all positions within a domain must be licensed except one: the head of the domain (see Kaye 1995:303). The head is the stressed Nucleus.
geminate **nn**, respectively). To sum up, morphological concatenations are of the following types:

(52)  

(a) Non-analytic domain:  

(b) Analytic domains:  

(i)  

(ii)  

Kaye devotes special attention to emphasising that the brackets merely represent domainhood. It is worth quoting Kaye himself here (1995:302):

(... 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: \( \text{concat} \) which takes two arguments which are strings and returns the string which results from concatenating the second argument to the first. For example, \( \text{concat}(\text{'abc'}, \text{'def'}) = \text{'abcdef'} \). The second function is \( \phi \).

This function has one argument, a phonological string, and returns the application of the phonology to its argument, also a phonological string. The expression \( \phi(X) \) means, ‘apply phonology to the string \( X \)’. \( \phi(X) \) returns the phonological string which results from the application of phonology to its argument.

Let me elaborate on this, especially because Kaye’s formulation is rather technical. Take, for example, a form such as **left** — either meaning ‘the opposite of right’ or ‘leave-Pret’ (121). As a single domain, the phonological derivation runs as follows (53):

\[
(53) \quad \phi(\text{left}) \quad \rightarrow \text{Output: 'left}
\]

That’s the end of the story: the form is handed down to the phonology as a single domain, i.e., whether morphologically complex or not, there is but one step: phonology is applied to the

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120 Kaye claims that structures of the type [A[B]] are not attested. In other words, there are no analytic prefixes which are not domains in themselves. Whether this is true or not is, of course, an empirical question. Proclitics, however, can be analysed in this way, cf. [the [man]]. This concatenation, nevertheless, is not a morphological but a syntactic product. I will neglect such cases here as I am not concerned with syntactic procedures.

121 The difference is immaterial: since **left**, the past of **leave**, is a non-analytic domain, behaves exactly like the monomorphemic **left**.
string. Compare this to *leaving* — an analytic form, the derivation of which is shown below in (54)\(^{122}\):

\[
(54) \quad (a) \ \phi(\text{l}i\text{v}) \quad \rightarrow \text{Output: 'l}i\text{v}
\]

\[
(b) \ \text{concat}((\text{l}i\text{v}\text{ng}))
\]

\[
(c) \ \phi((\text{l}i\text{v}\text{ng})) \quad \rightarrow \text{Output: 'l}i\text{v}n\text{g}
\]

That is, do phonology on the internal domain (the base here); then concatenate it with the suffix into a complex domain, then do phonology on that domain. Some notes are in order here.

First, as already mentioned, the brackets are not objects according to Kaye:

“[they] are not part of phonological representation. There are no ‘boundaries’. The brackets delimit phonological domains which are arguments to functions like \text{concat} and \phi” (1995:303).

The consequence of this is that no phonological process can refer to the brackets, in sharp contrast with Lexical Phonology, for example, where brackets are part of the representation and may trigger rules. Scheer, however, argues that the Kayean framework does in fact treat “the end of the domain” as a phonological object\(^{123}\). We will see shortly why.

Second, the derivation is fundamentally different from classical generative ones. There are no ordered rules. The \phi function applies \textit{all} phonology to its argument. That is, phonological processes do not apply sequentially, neither selectively: either \textit{all} of them are applied to a string, or none, and all of them are applied whenever the triggering conditions are met (= no ordering).

Third, a very important issue must be clarified: non-analytic forms like *left* (*leave-*Pret’), *kept*, *dreamt*, *vain*, etc., are not related phonologically to *leave*, *keep*, *dream*, *vain*, etc., respectively. They are separate lexical entries. How they are related to each other is a question that is referred to the lexicon (or the morphology), but in no sense can they be derived from a common underlying representation\(^{124}\).

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\(^{122}\) Note that the suffix -\textit{ing} appears lexically as -\textit{ŋŋ}: GP (at least Scheer), much like SPE, derives surface \textit{n} from lexical \textit{ŋŋ} sequences, based on distributional arguments.

\(^{123}\) Tobias Scheer (personal communication)

\(^{124}\) See Kaye 1995:310f for suggestions how this might work in the lexicon.
The next issue to be addressed is how Kaye represents the distinction in his representations. Crucially, Kaye uses a CGP framework, which is characterised by the following important traits (some of which were mentioned in the previous chapter):\textsuperscript{125}

In CGP, prosodic structure boils down to a strict repetition of Onsets and Rhymes, both maximally binary branching. Under the Rhyme, there’s only one constituent, viz. the Nucleus, itself maximally binary branching. If the Nucleus branches, the Rhyme may not do so. There is no Coda constituent: if the Nucleus is non-branching, i.e., it dominates but one skeletal position, the Rhyme may dominate a skeletal position to the right of the Nucleus, but this position is directly dominated by the Rhyme; this position is called Rhymal Adjunct in GP\textsuperscript{126}. Schematically, a maximal Onset-Rhyme pair can be of one of the two structures depicted in (55) below:

\begin{equation}
\begin{array}{ll}
(a) & R \\
O & N \\
x & x & x & x \\

(b) & R \\
O & N \\
x & x & x & x \\
\end{array}
\end{equation}

A “Coda” is only possible before an Onset, hence word-final consonants are Onsets in CGP (just like in CVCV), followed by a parametrically licensed empty Nuclear position. Hence, the syllabification of the English words \textit{sand} and \textit{trendy} is as follows:

\begin{equation}
\begin{array}{ll}
(a) & R \quad R \\
O & N \quad O \quad N \\
x & x & x & x & x \\
\quad s \quad æ \quad n \quad d \\
(b) & R \quad R \\
O & N \quad O \quad N \\
x & x & x & x & x \\
\quad t \quad r \quad æ \quad n \quad d \quad i \\
\end{array}
\end{equation}

\textsuperscript{125} See KLV 1990 or Harris 1994, for example, for details on CGP.

\textsuperscript{126} See, for example, KLV:198ff. Recall from the previous chapter that the skeleton in GP is a string of undifferentiated X slots.
As the reader can see, not all consonant clusters are broken up by empty Nuclei. Indeed, empty Nuclei are only inserted into the string in the following cases:

1. Domain-finallly in languages which license FENs;
2. Where there is a vowel–zero alternation site;
3. Word-initially before a sC cluster;
4. Where there is a “bogus cluster”, i.e., one which cannot be syllabified either as an On-set or a Coda-Onset sequence. An Onset cluster is of the TR type; Coda-Onset clusters involve (i) obstruent-obstruent sequences where the second member is a plosive, (ii) sonorant-obstruent sequences; if the sonorant is a nasal, it must be homorganic with the obstruent. Hence clusters such as \( tn \), \( mt \), \( lr \), etc. can be neither Onsets nor Coda-Onset sequences. In such cases, even if there’s no alternating vowel between the two C’s, an empty Nucleus is assumed. Let us see the CGP representation of the English words *stagnant*:

Let us now see the representation of an analytic and a non-analytic concatenation, using the English words *peeped* and *kept*; brackets, of course, enclose domains (cf. Kaye 1995:306ff):

According to Kaye, the domain-final empty Nuclei (underlined X’s) are empty by virtue of being domain-final: English licenses FENs. The form *kept*, being non-analytic, exhibits a short
vowel, since the p is in a “Coda”, i.e., the Rhyme branches, so the Nucleus cannot. Note that because pt is a well-formed Coda-Onset cluster, it is represented in GP as such, without an intervening empty Nucleus, since there never is a vowel between the two members of the sequence. In peeped, Kaye claims, the suffix is analytic, the concatenation being of the type [[A] B]. The internal domain is peep, ending in a FEN: as the root vowel is not in a closed syllable (the Rhyme is non-branching), it can be long. The φ function performs phonology on the string, after which the concat function adds the suffix; finally, phonology is performed on the entire string peeped. The pt cluster here is, therefore, a “pseudo-cluster”, which is why the root vowel may be long.

In order for the procedure to work, however, Kaye must make an important assumption: notably, that associations created in the inner domain must not be undone in the outer domain. Put another way, the results of applying the φ function to the inner domain (in this case, peep) cannot be modified by the application of the φ function to the entire form (i.e., peep). The procedure, therefore, is serial: apply phonology to the innermost domain, then perform morphology, then apply phonology to the result. This is an automatic consequence of the fact that Kaye must make crucial reference to domains.

As I already mentioned, Scheer points out that Kaye does, in fact, treat brackets as objects (even if he does not admit this). Brackets are none other than representatives of domain edges. Essentially, Kaye makes reference to domain-final (but not domain-initial) position, since this is where apparent phonotactic irregularities are found, such as monomorphemically illicit clusters (e.g., dreams) or long vowels before consonant clusters (e.g., peeped); for this reason, I will now limit the discussion to why “the end of the domain” is an object.

In standard GP, the model Kaye adopts, FENs are subject to the following parameter (formulated in (37), repeated here as (59) for convenience):

\[(59) \text{Word-final Empty Nucleus parameter: ON/OFF}\]

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127 The underlying d of peeped is devoiced as a function of the phonology. One can say, for instance, that “voice” in a suffix-initial C must be somehow supported by the stem-final C, or something of the sort. Kaye does not go into details about this.

128 Many of Scheer’s arguments in what follows are to appear in Scheer (forthcoming) in detail. I am not familiar with the exact details, and what I am going to say about his position I received in greater part directly from Tobias Scheer (personal communication). I use references only when this is not the case. I also indicate, of course, when the view I express is my own.
In fact, the term “word-final” is not quite appropriate. It is now possible to restate this parameter with reference to domains:

(60) Domain-final Empty Nucleus parameter: **ON/OFF**

That is, empty Nuclei are licensed by virtue of being domain-final in those languages which display final surface consonants. Now, the key to why “the end of the domain” is an object in Kaye’s model lies in the status of this parameter. Specifically, CGP treats it as a phonological one\(^{129}\). In other words, it is identical to other parameters in kind: it is a decision of the phonology whether it allows FENs or not, much like it is the decision of the phonology of the given language if it allows Codas (i.e., branching Rhymes in GP), branching Onsets or branching Nuclei, etc. There is, however, a serious problem with treating the Licensing of FENs on a par with these parameters. Whether a language allows long vowels etc. is clearly a phonological issue: it is a function of whether Nuclei may branch, which concerns Nuclei in general\(^{130}\). FENs, however, are special, precisely because they are domain-final. In other words, they are (by definition) adjacent to a morphological boundary. No other parameter makes reference to morphological boundaries. This claim may seem to be false at first sight. A language may allow a Coda-Onset cluster internally, but not finally, for instance (cf. Spanish: fuente ‘fountain’, parte ‘part’, etc. are OK, but no final *nt, *rt, etc.). But neither CGP nor CVCV makes reference to the “end of the word” when formulating this restriction: instead, both theories would say that such clusters must be licensed by a following full vowel (though the precise formulations in the two models are, of course, different). Indeed, these theories cannot refer to these clusters (or any C) being followed by the object “the end of the word” for the simple reason that consonants are never adjacent to that object: all words end in a Nucleus, whether empty or not.

Furthermore, the FEN-Licensing parameter was introduced in GP exactly because its silence could not be explained with reference to Proper Government. In other words, it is the end of the domain which licenses FENs (if it does), and the end of the domain is a morphological object, viz. a morpheme boundary. The FEN-Licensing parameter must therefore inevitably refer to a morphological entity. The brackets, therefore, perform pretty much the same function as the # boundary in SPE. This is the reason why Kaye has to make

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\(^{129}\) Scheer is not explicit on this point, though he seems to imply it.

\(^{130}\) In CVCV this boils down to whether a single vocalic melody may attach to two V positions straddling an empty c.
reference to the end of the domain: the phonology licenses empty Nuclei before this entity. To illustrate this, let us give a more formal formulation of the FEN parameter:

\[
(61) \quad \text{N} \quad \left| \quad / \quad \text{___} \right| \quad \text{x}
\]

In prose: a Nuclear position is possible before the end of the domain no matter whether melody is associated with its timing slot or not.

Note that without the bracket, the phonology would interpret (61) as “all Nuclei, whether full or empty, are licensed”. This would mean that there is no restriction on the distribution of empty Nuclei — a disastrous consequence indeed.

4.2. [A CVCV model of representing morphological effects on the phonology]

Scheer argues that this interpretation is wrong. Notably, as I already pointed out, he claims that a FEN is not before the end of the domain: it is the end of the domain. In order to understand why he makes this claim, a theoretical position essential to his model must be introduced: Representational Modularity.

The conception of grammar known as Representational Modularity (RM) is not Scheer’s invention: its chief representative is Jackendoff (see Jackendoff 1992, 1997, for example). The essence of RM is that phonology constitutes a separate, parallel module from the rest of the grammar, i.e., syntax, semantics, and morphology. The most crucial difference between phonology and the rest is that it “speaks a different language”. The other modules share a common language. Syntax, semantics and morphology all “know” what number, person, case, etc. are. Phonology does not understand or use these terms. No phonological process states, for example, “Turn a final labial into a coronal in the genitive/plural/preterite/etc.”. Conversely, phonological categories such as Onset, labial or voiced, etc., are incomprehensible for the other modules, and are not used either. There couldn’t possibly be a syntactic rule saying “Move α into SPEC CP if it begins with a voiced consonant/branching Onset/empty C/etc.”. The point made in RM is that this difference in idiom must be brought to its logical conclusion: phonology speaks a different language from the rest of the grammar; therefore, intermodular communication is only possible if morpho-

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131 The discussion of RM is based on Scheer 2004b.
syntactic (or semantic) “orders” are translated into phonological vocabulary. As Jackendoff writes,

‘Mixed’ representation[s] should be impossible. Rather, phonological, syntactic and conceptual representations should be strictly segregated, but coordinated through correspondence rules that constitute the interfaces. (1997:87f)

Furthermore, Jackendoff says,

The theory of Representational Modularity [posits], in addition to the representation modules proposed above, a system of interface modules. An interface module communicates between two levels of encoding, say L₁ and L₂, by carrying out a partial translation of information in L₁ form into information in L₂ form. (1997:42)

I leave aside the question as to how exactly the translation is performed, since it is not relevant for this thesis. Let us see examples as well as what may be translated into the “phonological idiom”.

It is boundary information that is to be translated. The phonology doesn’t understand what a “morpheme boundary” is (nor does it understand what a “morpheme” is, to begin with): it is not a phonological object. Now, Scheer assumes the following: morphology decides if boundary information is sent to the phonology. The phonology does not make any decision: it merely “obeys” morphosyntactic orders. This is not a new conception: it is basically assumed in SPE as well. In SPE, the morphosyntax may choose to represent a morphological boundary as +, =, or #. As we saw in Chapter 2, phonological rules do or do not make reference to these boundaries. If, for example, a phonological rule does not include # in its structural description, its application will be blocked by the presence of a #, but not by the presence of a +. Indeed, if a rule does not mention any boundary in its structural description, it will apply to all strings enclosed between #’s, no matter whether the string contains a +. This formalism achieves the same effect as Kaye’s non-analytic vs. analytic division. Non-analytic domains correspond to SPE #....(+)....# strings. An analytic domain is the equivalent of SPE #....#....#. Yet, as pointed out, rules in SPE may make reference to + (such as Laxing before +ic), in which case they will not apply if that boundary is absent. No
such possibility exists in Kaye’s (or Scheer’s or whoever else’s who does some sort of GP) model. Synthetic forms will always behave in the same way as monomorphemic ones.

To return to the discussion of RM, Scheer claims that boundary information (i.e., morphosyntactic division)

1) may or may not be represented in the phonology;

2) it is decided by the morphosyntax if boundary information is represented: phonology is entirely passive;

3) if it is represented, it must be encoded in phonological language.

Objects such as “#” or “[ ]” are not phonological objects: they are neither segments (melody), nor phonological constituents, nor phonological relations. The solution adopted by Scheer is that boundary information is represented by (i) inserting a phonological object into the string, (ii) modifying the properties of objects present in the string. Specifically, this means the following:

1) The “beginning of the word” is translated as the insertion of an empty CV unit into the string.

2) The “end of the word” is represented by modifying the properties of the final Nucleus. This boils down to three effects:\footnote{These three options are subject to an important condition: morphosyntax may only add something to the representation. For final Nuclei, this means that properties of lexically empty Nuclei may be enhanced, but not inhibited. The morphosyntax cannot deprive a final full Nucleus of its licensing potential, for instance. See, for example, Scheer 2004b for details.}

a) Final Nuclei may be externally governed\footnote{Scheer uses the term governed, rather than licensed, to bring out the parallel between FENs and their word-internal governed pairs: they are both silenced. This is problematic, too; I return to it later.} (externally = by morphosyntactic order, rather than “domestic” phonological action, i.e., PG);

b) FENs may be licensed externally to govern, i.e., to behave as full Nuclei with respect to Government;

c) FENs may be licensed to license, i.e., to behave as full Nuclei with respect to Licens-

We saw examples for these in Chapter 3. It is important to note that (2b) and (2c) are independent of each other (but, of course, both imply (2a)).

The essential point is that the representation modified in one or more of these ways is the \textit{input to the phonology}. It is not the phonology which turns “the beginning of the word” into an initial empty \textit{cv}. In fact, it does not know that this object \textit{is} the beginning of the word: the empty \textit{c} and \textit{v} in this sequence will behave just like word-internal empty \textit{c’s} and \textit{v’s}. The same goes for final position.
As we saw, not all languages choose to represent the “beginning of the word” phonologically. If the morphology of a given language selects the option of not sending this boundary information to the phonology, no empty cv unit will be inserted. In such languages, word-initial position is not different from word-internal position with regard to its phonological behaviour. For example, such languages allow both #TR and #RT (as well as #KT) clusters word-initially, just like word-internally. This contrasts with languages such as English, where the beginning of the word is represented in the form of an empty cv pair: here, only #TR clusters are possible, for example\(^{134}\).

Similarly, if a language does not allow FENs (i.e., all words must end in a pronounced vowel), “the end of the word” as such will have no phonological import: full vowels, whether final or not, behave alike. These facts lead to an important issue: privativity. The stance outlined here claims — in contrast with SPE, and, as we’ll soon see, Kaye as well — that only those boundaries are represented phonologically which have a phonological effect: why should we represent something that “isn’t there” for the purposes of sound structure? Indeed, what Scheer does when following this line of argumentation is that he takes the generally accepted principle of all versions of GP, *Privativeness*, to its logical conclusion.

In SPE, there is no way not to represent a word boundary: # is always present. As a result, the theory makes no prediction as to what effects # can have: it can, for example, prohibit #RT clusters (English); but it can license them as well (Polish); it all depends on the given language: no prediction is made.

Kaye’s framework, too, suffers from this problem: brackets indicating domain edges are always present. Furthermore, I take the opportunity to supplement Scheer’s observation by pointing out that domain-initial and domain-final position do not behave in a parallel fashion. The domain-final position in Kaye’s framework is special: it may serve as the environment of the FEN-Licensing Parameter, which, in turn, identifies the right-hand edge of the domain phonologically (cf. *dream-s*, where the domain edge is shown by the FEN between the morphemes). Domain-initial position, however, never does the same job: “the beginning of the word” is always “[” — whether phonologically relevant or not. This is a logical consequence of the fact that brackets come in pairs (since they enclose domains), so there must be a “[” for every “]”.

To sum up, Scheer proposes, based on RM and empirical observations about the phonological behaviour of domain edges, that morphosyntactic division *may* be represented

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\(^{134}\) It is Scheer’s specific claim that the insertion of an empty cv unit is a matter of parametric variation, a view not accepted by everyone, cf. Lowenstamm 1999.
phonologically, but whether it is or not is the autonomous decision of the morphosyntax, and it can only be represented in phonological language. Let us now elaborate on how this might be done.

In this section, I present a possible way of interpreting Scheer’s proposals. As I have already dealt with word-initial position, I will not be concerned with it here (especially because it is irrelevant for our purposes). It must be added that I am not aware of the details of how Scheer implements these proposals in relation to morpheme boundaries. In what follows I offer a possible interpretation and implementation, based on Rebrus’s representations of Hungarian synthetic and analytic concatenations. Whether Scheer’s solution differs from mine remains to be seen. It is an open question, too, to be verified or falsified by empirical data and theory-internal arguments, if this solution works on a general plane. It seems to yield the correct results for the set of (limited) data I have examined.

Let us assume, then, that a synthetic concatenation is sent to the phonology without any internal FEN(s), while an analytic one will include a FEN before the morpheme boundary. Therefore, the representation of kept vs. peeped is as follows (I omit word-initial empty cv’s for the sake of simplicity, but note that they are present in all English words that are C-initial on the surface); EG = External (morphosyntactic) Government.

\[ (62) \]
(a) \[ \begin{array}{c}
\text{PG} \\
\downarrow \\
\text{EG} \\
C \quad V \quad C \quad v \quad C \quad v \\
| \quad | \quad | \quad | \\
k \quad e \quad p \quad t
\end{array} \]
(b) \[ \begin{array}{c}
\text{Lic} \\
\downarrow \\
\text{EG} \\
\text{EG} \\
C \quad V \quad c \quad V \quad C \quad v \quad C \quad v \\
| \quad | \quad | \quad | \\
p \quad i \quad p \quad d
\end{array} \]

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135 He has not yet published/circulated anything on the matter. Scheer (forthcoming) will be partly devoted to the morphology-phonology interface, but this book is not available as yet. See, however, Scheer 2004a:583-96 for a treatment of Slavonic vowel ~ zero alternations without reference to domains.


137 I limit the discussion to suffixation, since prefixation is irrelevant for the present paper.
The analytic boundary is directly encoded into the phonological representation via FENs, receiving EG due to morphosyntactic order. For the phonology, whether an E-Governed Nucleus is final in the string or not does not matter: its properties (whether it can license or govern) are decided by the morphosyntax\textsuperscript{138}. In English, FENs are both licensors and governors. This explains why the FEN in (62a) may govern the preceding empty Nucleus, and why, in (62b), a long vowel is possible in the stem: the FEN can license the second V position of the long Nucleus to receive spreading melody (exactly in the same way as in \textit{peep}). Note that no bracketing is necessary, and, accordingly, there is no serialism: the phonological interpretation is done in one go.

One important thing must be pointed out: a restriction on where the morphosyntax may E-Govern empty Nuclei. Such intervention is strictly limited to morpheme edges. In case we talk about concatenations, the morphosyntax may only intervene at positions immediately adjacent to the morpheme boundary\textsuperscript{139}. No morpheme-internal intervention is possible. Furthermore, morphosyntax only has access to the skeletal level: subskeletal material (melody) cannot be influenced (see Scheer 2004b for a more detailed discussion).

A final note: though we cannot speak of \textit{domains} any more in the Kayean (technical) sense, I will retain the term as an informal label, together with the terms \textit{analytic}, \textit{non-analytic}, and \textit{synthetic}. Note, however, that domains have no phonological status in the model I adopt.

In what follows I refine the structures with regard to the skeletal structure of suffixes depending on whether they are concatenated with the stem analytically or synthetically. The discussion will be crucial for my analysis of Middle English Pre-Cluster Shortening later on.

As a starting point, let us see some further MoE examples, illustrating the behaviour of the suffixal morph \textit{-ing}, which is \textit{always} analytic: it leaves the phonological makeup of the stem perfectly intact. Figure (63) shows the representation of \textit{letting}, \textit{bottling} and \textit{drawing}\textsuperscript{140}. I indicate the boundary between the two morphemes with a hyphen, but note that this is to facilitate the reader’s job and it is not part of the structure.

\textsuperscript{138} The overall morphosyntax, I mean. That is, if the language gives licensing or governing potential to FENs, it will uniformly apply to all FENs: you cannot give such potential to FENs selectively. Recall also that FENs only differ from full Nuclei in that they may only govern/license a totally empty Nucleus, but not a vowel~zero alternation site (= floating melody).

\textsuperscript{139} See Section 4.4. for a proposal why this should be so.

\textsuperscript{140} Again, I do not represent initial empty \textit{cv}’s, since they are not relevant here.
The above data need some comments. First, note that the suffix -ing begins with an empty $c$. CVCV assumes that the skeleton of all morphemes consists of $(CV)^*$, so the assumption that the morpheme -ing begins with an empty $C$ is expected. (Though see counterarguments below
regarding synthetic suffixes.) Second, as I mentioned in the previous chapter, surface \( \eta \) is derived from lexical \( \eta \eta \), due to familiar distributional restrictions on \( \eta \) in English. CVCV formalises these restrictions by saying that the \( \eta \) in \( \eta \eta \) must be licensed by a non-empty Nucleus\(^{141}\). Otherwise, (63a) needs no special comment. In (63b), the stem-final \(-\eta l \sim -l\) alternation requires some attention. Lexically, there’s a floating schwa between the \( t \) and the \( l \). Being followed by a FEN, it cannot receive PG: FENs cannot govern but totally empty Nuclei. There are two possibilities: it is either realised (being connected to the V position), or it is uninterpreted phonetically, but in that case, the \( l \) must spread into the V slot in question, becoming syllabic. In (63c), admittedly, an incomplete representation is given, according to the pronunciation of \textit{drawing} in rhotic accents of English, such as General American, as well as those non-rhotic ones which have no R-intrusion (e.g., US South). For R-inserting accents, such as Southern British English, an alternative solution must be sought. Such a representation is given in (63d). Following usual practice in autosegmental formalism, I represent the alternating \( r \) as a floating segment, which attaches to a following C position, if there’s one available. The suffix \(-\text{ing}\) begins with an empty C, as all skeletons are made up of strictly alternating CV units in CVCV; hence, it does provide the floating \( r \) with an anchor. Such an analysis is used, for instance, in Harris 1994:248ff (though in a CGP framework)\(^{142}\). The question if all morphemes do actually begin with a C on the skeletal level will be discussed shortly.

Let us now consider the representation of a concatenation with a vowel-initial synthetic suffix. A good example is \textit{longer} ‘\textit{long}-\textit{Comp}’. That it is a synthetic concatenation is shown by the presence of \( \eta \), cf. \textit{longing} ‘\textit{long}-\textit{η}', with an analytic suffix, or the near-homophonous \textit{longer} ‘\textit{longer} ‘someone who longs’, where the phonetic makeup of the morphemes is lexically

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\(^{141}\) Please note that the \( \eta \) is associated with the position of \( \eta \), too. This is because the cluster is a homorganic one, where the two consonants share their place element. Harris (e.g., 1994:69) calls such clusters \textit{partial geminates}.

\(^{142}\) The reader may have noticed that the \( \eta \) in \textit{drawing} spreads into the E-Governed FEN, according to how long vowels are represented in CVCV. This gives rise to a difficulty. V positions ought to be \textit{licensed}, not governed, to receive spreading melody. In Scheer’s version of CVCV, FENs are \textit{governed}, for reasons explained in Footnote 133. It appears that the external influence on FENs is, in fact, neither Licensing nor Government, but something else. This isn’t really surprising, though, since Licensing and Government are phonological relations, and FENs receive extraphonological influence. I will retain the term E-Government, but note that EG is used here as a label to refer to a force whose real identity is yet unknown.
identical (allegedly: but see below), yet the the two *longer’s* are different: in the noun, the schwa is followed by a FEN, which is unable to license it. It appears that in the adjective *longer* the schwa is licensed: but how?

Recall that CVCV assumes that all skeletons are necessarily C-initial (and V-final). Let us attempt to represent the synthetic adjectival form *longer* in this way. This is done in (64).

As the reader can verify, the structure in (64) is ill-formed. The last Nucleus is filled, so it can govern the preceding empty Nucleus — which happens to be the last skeletal position in the stem\(^{143}\). A governed Nucleus, however, may not license a schwa in an \(\emptyset\) cluster. Hence, the schwa is expected to drop, but it does not. Moreover, it cannot govern the empty \(v\) inside the \(\emptyset\) cluster. As a result, (64) cannot be the representation of the comparative form of the adjective *longer*.

Two well-known proposals have been put forward to heal the problem. The first is that of Gussmann & Kaye (1993; formulated, of course, in CGP formalism, i.e., with reference to the constituents Onset and Nucleus rather than C and V positions, but the essence is the same). They introduce the notion of *Reduction* (1993:433), which deletes an empty NO sequence (= \(vc\), in our terms; indicated by being encircled in (64)) to ensure the regular ON alternation. If the empty \(vc\) is erased, the problem is solved: the schwa will be adjacent to the empty \(v\), and, being a full vowel, it can both license and govern. The post-Reduction situation is shown in (65):

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\(^{143}\) Note that it is not a FEN, since the morphology does not represent boundary information in synthetic concatenations, cf. also *kept* in (62a).
There are problems with this analysis, though, as Szigetvári (1999:102) points out, referring to Polgárdi (1998:37); see these works for details. One difficulty is that Reduction clearly violates the Structure Preservation Principle, as lexically established relations are altered. Szigetvári (1999:106), using skeletons made up of VC, rather than CV, units, argues that there is no problem of this kind in his framework: as skeletons end in a C, and begin with a V, the skeleton of longer (Adj) will be vCVCvC-Vc, which yields the expected structure, and no Reduction needs to be assumed. He points out, furthermore, that non-analytic suffixes are typically V-initial, which is easily accommodated in his theory.\textsuperscript{144}

The chief problem, for our purposes, is that this analysis makes reference to the fact that Reduction happens over domain boundaries — but the essence of non-analytic morphology is that it does not project its morphological complexity into the phonology. In other words, synthetic forms should behave just like monomorphemic ones. The presence of the empty vc sequence betrays the complexity of the form, as such a sequence could not possibly exist inside monomorphemic forms. Therefore, if we want to maintain the claim that the morphology has no effect on synthetic forms, the Reduction solution is ruled out.

In a CVCV model, this does not appear to be a possible solution. In what follows I would like to argue that (65) is the correct representation for longer (Adj), but it’s not the result of Reduction. Instead, based on Rebrus (2000:816f, 831f) I assume that this representation is the lexical one. I will elaborate on this view in what follows.

This proposal may seem to contradict the basic principle of CVCV that the skeleton of each morpheme is composed of CV pairs: taking (65) as an example, we must assume a CVCvC-V skeleton, exhibiting two violations of this principle: the stem ends in a C, and the suffix contains but a V.\textsuperscript{145}

\textsuperscript{144} The word typically is important, as there are C-initial non-analytic suffixes, cf. kep-t, leng-th, etc. (at least on the surface: more on this shortly).

\textsuperscript{145} Plus a floating r, but that is not associated with any skeletal position. In rhotic accents, -er is, of course, VCv. This difference between rhotic and non-rhotic accents is irrelevant: in both cases, the suffix is vowel-initial in this formalism.
Note, however, that the basic point in synthetic affixation is that it is indistinguishable from monomorphemic forms. I propose that this claim be taken literally, and no difference between synthetic and monomorphemic forms must be posited. As empty *vc* sequences are not found in the latter, they are not to be assumed for synthetic forms, either. In other words, I claim that the essence of synthetic morphology is that it may concatenate *incomplete* skeletons. This is in line with the fact that synthetic affixes may attach to bound stems (though they may attach to free ones, too, e.g., *obēse ~ obēs-ity*). Analytic ones never do. I suggest that this fundamental property of synthetic morphology is reflected in the possibility of concatenating incomplete skeletons. Put another way, the point is that the full form itself must have a (CV)* skeleton. This has a serious consequence for *longer*: it must be assumed that the adjective *long* has two allomorphs: a free and a bound one, viz. *lon_g_* and *lon_g*, respectively (where the underlined spaces indicate empty *v*’s). The latter occurs in the comparative and the superlative. This means that the alternants are not derived from a common initial representation via the phonology: instead, this is allomorphy proper. The boundedness of a root morpheme is represented phonologically by a C-final (incomplete) skeleton. In other words, word-final position is represented phonologically as an empty *v*; root-final position (with bound roots, of course) translates as final C: this equals the fact that bound roots are not possible skeletons in themselves. Note, however, that this is *not* quite the same mechanism as the ones indicating word boundaries: the latter are characterised by the restriction that they may not *deprive* the representation of any of its properties. The analysis of bound roots and synthetic affixes appears to contradict this restriction. I claim, however, that the marking of bound roots by a final -C is not the result of such a mechanism: they are not “deprived” of their final *v*. Instead, bound roots are, by their nature, not autonomous lexical entries: they always appear in the lexicon in combination with some suffix. Specifically, *longer* is stored in memory as CVCvCV. The bound root *long*- isn’t stored anyhow, and neither is its suffix, so there’s no violation of strict (CV)* structure, because it only restricts the skeletal content of lexical entries. The analysis I propose establishes a clear connection between boundedness and skeletal structure. This leads to a generalisation, described in (66) below:

(66) The Lexical Entry Principle (LEP)

Lexical entries are organised along a (CV)* skeleton.

The essence of LEP is that autonomous lexical entries must be assigned a skeleton consisting of strictly alternating (CV) units. Let us now go on to discuss some problems.
I claimed that bound roots are not independent lexical entries (which is actually in line with Kaye 1995:310ff). This is probably the case in English and all other languages where the regular morphology is typically word-based. The question is whether the same claim can be made about languages such as the classical Indo-European ones, e.g., Latin, Sanskrit, Old Church Slavonic, etc. These languages have very few free roots, these being limited to underived adverbs, certain pronominal forms, and the like. Major categories — verbs, adjectives, and nouns — virtually always have bound roots. It would be strange to claim that all inflected forms in these languages are lexicalised. A possible conclusion is that the root, though bound, must after all be given the status of an independent lexical entry. This would contradict the LEP (66). Yet, I do not think this conclusion is necessary.

It has been long noted that this type of morphology differs from the word-based one precisely in that it fails to lend itself to Item-and-Process (or Item-and-Arrangement) type analyses: instead, the Word-and-Paradigm (WP) model (basically the model of inflection followed in classical grammars of Latin, Greek, etc.) has been proposed. This means that all word forms are potential full lexical entries, but roots are not. I propose that such morphologies make the possibility of C-final roots a general organising principle, not restricted to particular lexicalised items. There seems to exist evidence that lexemes are not memorised as uninflected roots in such languages. Instead, a particular inflected form is taken as a “base form” which is stored in memory, and other inflected forms are computed on the basis of that form (using other bits of information such as gender, inflectional class, etc.). In the case of Latin nouns and adjectives, for example, it seems likely that the base form was the Accusative Singular. Speakers stored each noun and adjective in this form, instead of storing the root as a lexical item. It is a remarkable fact about Latin that (aside from recessive, i.e., non-productive, minor paradigms, such as ē- and ū-stems, which were probably treated as exceptions and hence possibly lexicalised) major (and productive) nominal paradigms can be fully identified based on two bits of information: (i) the AccSg form, (ii) gender. To sum up, a synthetic form need not be irregular or lexicalised, and yet, it is a legitimate claim that bound roots are not independent lexical items in WP-type morphologies, from which it follows that bound roots can be conceived of as ending in a C on the skeletal level: they, as non-entries, are not subject to the LEP.

147 See Bybee 1985:49-79; she does not deal with systems employing bound roots, but her model is fully compatible with the idea outlined here.
The question emerges how *consonant-initial* synthetic suffixes, such as English -t characterising irregular verbs (e.g., kept, left, etc.) fit into the picture depicted above. They seem to be problematic: they are not (surface) vowel-initial. Yet, the problem is apparent: there is no particular reason to believe that such suffixes are, on the skeletal level, C-initial. Indeed, consider the representation I proposed for kept in (62a), repeated here as (67) for convenience:

![Diagram](image)

The representation in (67) infers no obligation on us to analyse the Properly Governed v as belonging to the root. The essence of synthetic concatenations is that they include no E-Governed Nuclei inside the full domain. There is no way to distinguish them from monomorphemic forms. Therefore, the form says nothing at all about whether the empty Nucleus, adjacent to the morpheme boundary, is part of the root or the suffix. It is perfectly possible to say that it is actually suffix-initial. We can, therefore, maintain the claim that synthetic affixes *always* begin with a Nucleus, but that Nucleus can be empty.

Note, however, that we mentioned an observation: notably, synthetic suffixes are typically vowel-initial on the surface: i.e., they begin with a full Nucleus. This is indeed the case in the overwhelming majority of cases, i.e., empty v-initial synthetic suffixes seem to be rare. I would like to point out that my model, in fact, predicts this; let me explain why. A CV sequence is not a phonotactic domain, but a CC (= CvC, structurally) sequence often is. In other words, languages typically do not show phonotactic restrictions inside CV sequences: in general, any C can stand before any V\(^{148}\). This is not true with respect to a consonant cluster. In a synthetic concatenation, the same phonotactic restrictions hold for any (surface) CC sequence straddling the morpheme boundary as for identical sequences within monomorphemic forms. Therefore, it is no wonder that synthetic suffixes prefer to be surface

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\(^{148}\) Called the “Principle of Free Cooccurrence”; see, e.g., KLV:200. There are some (possibly apparent) exceptions. In Proto-Slavonic, for example, CV sequences may not combine palatal segments with velar ones. I analysed this in a CGP framework in Kristó 1999. Later, however, I came to the conclusion that such restrictions are probably the result of interaction between melodic primes rather than due to relations holding on the skeletal level; see Kristó 2003. For a general theoretical background, see Scheer 2000b and Ségéral & Scheer 2001b.
V-initial: the initial (full) V is free to combine with virtually any root-final C. This is not true for C-initial suffixes. As far as I can see, my model predicts this behaviour. By contrast, any alternative which treats (synthetic) bound root + suffix combinations on a par with (analytic) free root + suffix concatenations fails to make this prediction. In such models, the fact that synthetic suffixes are generally V-initial is merely observed. In the model I propose, it is explained.

I would like to add one final note. Observe that C-initial synthetic suffixes (as expected) are often subject to severe restrictions as to what kind of root they can combine with. This is predicted by the framework advocated here. For example, the English synthetic preterite marker -t is restricted to roots ending in -p, -f, -s, -m, -n, -l, as in kept, left, lost, dreamt, meant, felt, respectively. These Ct sequences are found monomorphemically, too, cf. apt, soft, cost, asymptote, Lent, melt, respectively. It is open to debate whether forms such as met, led, and others in -t/-d include a suffix -t, which assimilates to a root-final -d then the geminate is shortened. This is certainly the SPE-type interpretation. Note, however, that SPE derives the infinitive and the preterite from a common lexical representation, an option which is outlawed in GP. If met, led & Co. are separate lexical entries, there’s no reason to assume a suffix. I prefer the solution that there is no suffix; instead, the preterite is marked by the difference in the root vowel. In this sense, these verbs line up with historically strong ones.

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149 This is also valid for Szigetvári’s (1999) theory: according to him, all roots are C-final and all suffixes are V-initial. Hence he doesn’t make a difference in this respect between synthetic and analytic concatenations, either.

150 And vowel-final ones such as taught, brought, etc., if one accepts the analysis of the final -t here as a morpheme. The cluster -rt, in rhotic accents, could be possible, but there’s no example (the only weak irregular verb in -r is hear, but its preterite is heard hard, rather than *hærd). I assume that this is a historical coincidence, much like the lack of irregular preterites in -kt, which could be possible (cf. act).

151 Asymptote can be pronounced with a mpt cluster, too, but the p is entirely optional according to Wells 1990. The same goes for Hampton, which, though historically polymorphic, is clearly lexicalised and probably should be treated as monomorphic in Present-day English. I conclude that mt is licit monomorphemically in English (even if rare). This is counter-predicted by Kaye 1995:311, who claims that NT clusters (where N = nasal and T = plosive) must be homorganic within a non-analytic domain. Kaye is actually forced to make this claim for reasons internal to CGP theory. CVCV (at least in the form I use here) does not make such a prediction.

152 Similar restrictions seem to be valid for the Classical Latin NomSg suffix -s, as in rēx (= rēg-s) ‘king’, princep-s ‘prince’. It is combined, for example, with -t/-d-final roots in such a way that the dental stop is “dropped”, e.g., miles ‘soldier’ (cf. AccSg milit-em), lapis ‘stone’ (cf. AccSg lapid-em). Note also the vowel alternation in ‘soldier’. Not unexpectedly, the clusters ks and ps are monomorphemically licit (though rare, but I assume this is due to historical coincidence), cf. sex ‘6’ and laps-us ‘fault’. No ts cluster is attested monomorphemically (ds is out on independent grounds since the d would be automatically devoiced before a s, cf. rēx recks ‘king-NomSg’ vs. rēg-em regē.
4.3. [Ambiguities]
Kaye assumes that “syntheticness” and “analyticness” is the property of given affixes. In what follows, I will consider this statement. It may appear, based on some data, that this may not be true: there are cases when a suffix is sometimes analytic, sometimes synthetic, depending on what it is concatenated with. We may conclude that analyticness, therefore, is not a lexical property of affixes: instead, particular concatenations are analytic or synthetic. I will argue that appearances may be deceptive, suggesting an alternative whereby Kaye’s claim can be maintained.

Consider first the well-known instance in English of the suffix represented orthographically as -able or -ible, pronounced -\(b(\alpha)l\). In his highly influential (1976) book, Mark Aronoff presents a detailed analysis of this suffix within a classical generative framework (1976:120-29). He observes that the suffix in question shows a dual behaviour phonologically, morphologically, as well as semantically, and proposes that two able’s should be posited: #able and +able. In our terms, there is an analytic -able and a synthetic -able. Aronoff enumerates phonological, morphological and semantic arguments to support his claim that there are two, homophonous and synonymous, suffixes. Often, he says, a given stem may appear in combination with either suffix, e.g., cómpar+able vs. compár#able. This is reflected in stress placement as well as in the fact that cómpar- is not a free stem. Furthermore, the meaning of compár#able is transparent (‘able to be compared’), while the same is not true for cómpar+able (it can also mean ‘equivalent’). Such differences are not limited to these particular words. Most importantly for us, #able always attaches to free stems, while +able does not.

Recall the analysis I proposed for longer\(_{\text{Adj}}\). I claimed that it is a lexical entry. This means that its morphemes are not concatenated by the productive morphology. In fact, to take this argument to its logical conclusion, I suggest that this word is not concatenated anyhow: it’s listed. The morphology in such cases serves as a parser, not as a concatenator. This idea is by no means mine: Aronoff himself argues (1976, passim) that derivational morphology, i.e., word formation, is word-based, and that words which are apparently “formed” by concatenating bound stems with an affix are not morphological products but lexical entries, and the morphology is merely used to analyse such sequences. Rebrus (2000:832) also proposes (in his analysis of Hungarian morphophonology) that synthetic affixation should be referred to the lexicon, whether inflectional or derivational.

\(^{\text{id.-AccSc}}\). No wonder that this suffix (the only C-initial nominal suffix of Latin attaching to C-final roots) is recessive, and in Vulgar Latin, it is replaced by -is added to the Acc stem, i.e., VL NomSg *militis, *lapidis.
In fact, there are so many instances of non-uniform behaviour with regard to suffixes that for a great many of them, one would be forced to assume two, homonymous and synonymous morphemes — like in the case of +able/#able. There is a problem with this, however. Notably, homophony itself is a widespread phenomenon in language; but it does not normally go hand in hand with synonymy: instead, what one normally calls a pair of homonyms is *same signifiant but different signifiés*. In English, for instance, the regular nominal Plural suffix is homonymous with the 3Sing marker in the Present Indicative of verbs. Aronoff’s solution is, therefore, rather unusual, since such pairs are homonymous and synonymous. However, he is forced to claim what he does by a simple fact: the theoretical framework he uses. The different behaviour of +able/#able &Co. must be encoded in the suffixes themselves in the form of boundary diacritics. As these diacritics are part of the structure of the suffixes, they are, after all, not homonymous in a structural (abstract) sense.\footnote{It must be emphasised, though, that Aronoff explicitly denies the interpretation of boundary markers as phonological objects (1976:121f).}

In the framework I adopt here, positing two synonymous and homonymous morphemes is not inevitable. Capitalising on the observation that +able (i.e., the synthetic -able) always co-occurs with non-entries, i.e., it is always part of a lexical entry\footnote{And, as such, it is V-initial on the skeletal level, combining with C-final stems.}, we can claim that -able, whenever it is concatenated with its stem morphologically, i.e., the stem is an autonomous lexical entry, is always analytic. Using Aronoff’s formulation of word formation, one may represent this affixation process as follows:

(68)  $[[X\text{V}able]]_{\text{Adj}}$ ‘able to be X-ed’

*In prose:* add -able to a transitive verb to form an adjective with the specified meaning.

As X is always a free word form, it is subject to the Lexical Entry Principle: its skeleton must be (CV)*. The representation of lockable is as in (69):

(69) \[
\begin{array}{cccccc}
\text{EG} \\
\text{EG} \\
\text{cv} & \text{C} & \text{V} & \text{C} & \text{v} & \text{C} & \text{V} & \text{C} & \text{v} \\
\text{lockable} & l & d & k & o & b & l
\end{array}
\]
I assume that all regular morphology (whether inflectional or derivational) in English is always analytic. Let us check, however, the comparative and superlative of adjectives: considering *longer* (and other adjectives in *-ng*), one must treat the suffixes (viz. *-er* and *-est*) as synthetic. They also attach to free stems, though: cf. *nicer/nicest, freer/freest, etc.* It might seem that it is analytic here. Recall, nevertheless, that I have not claimed that synthetic affixes always attach to bound roots: they can do so, but this is not obligatory. I proposed merely that analytic affixes may not attach to bound roots. Considering forms like *nicer, freer, etc.*, it must be observed that we cannot really tell if they are synthetic or analytic forms based on their phonetic shape. Both analyses yield the same result. In other words, such forms do not display any monomorphemically illicit sequence. For example, the form *freer*, i.e., *frι:ə*, might as well be monomorphemic, cf. *Korea koˈria:*. The only way to decide if *-er/-est* can be analytic is to find forms which can only be analysed as analytic. One instance when the question can be unambiguously settled is a stem ending in a syllabic consonant. If the syllabic consonant can be retained in the suffixed form, the suffix can only be analytic. The suffix *-ing* serves as a good illustration, cf. *travelling*, phonetically ˈtrævəlɪŋ ~ ˈtrævɪŋ*. It is not so easy to find adjectives, liable to suffixal comparison, that end in a syllabic consonant, however. One such rare adjective is *simple*. Here, the comparative/superlative form is ˈsɪmplə/ˈsɪmpləst, with a non-syllabic *I*: indeed, these forms are not possible with a syllabic one\(^{155}\).

Note also that these adjectival suffixes are very restricted in their use: they can only be used with maximally disyllabic stems, and even there, they are on the retreat: it is not at all unusual nowadays to hear form such as *more happy, more rare*, or even *more nice*. Based on these observations, I conclude that the affixal expression of comparison is basically irregular in Present-day English. In other words, the only productive pattern is with *more/most*. I regard, therefore, all suffixed comparative/superlative forms as lexicalised, i.e., historical residues which are gradually disappearing from the language. There is no morphological rule for them comparable to (68).

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\(^{155}\) A possible counterexample is *commoner/commonest*, where the schwa is retained. It must also be added, however, that these forms are increasingly recessive nowadays, being replaced by *more/most common*. The adjective *evil*, for example, cannot take a suffix at all.
4.4. [Pinker & alia: the Words-and-Rules model]

The above line of argumentation about the lexical vs. morphological status of affixation is by no means forced upon us by the theory itself. It is supported by a great number of psycholinguistic experiments, too. To illustrate this, let me briefly describe a model of grammatical organisation known as the dual mechanism model or words and rules model. Its best known representative is probably Steven Pinker, known primarily as the author of the bestselling book *The language instinct* (though the book is not about this model). The theory, which I will refer to as the Words-and-Rules Model (WRM) from now on, was originally proposed by Pinker & Prince 1988, although the basic idea goes back to much earlier (in fact, Aronoff’s 1976 theory is probably the first well-known application). The most exhaustive and accessible exposition is Pinker 2000.

The claim of the theory is actually very simple: it states that inflected forms (and derived ones as well, though Pinker is mostly concerned with inflection) are produced in two ways. Irregular inflection is retrieved from the memory, while regular inflection is the product of the application of general, symbolic rules. The term *symbolic rule* means that the rule is not applied to individual lexemes: instead, it applies to a category, “with blind necessity”\(^{156}\), as it were. In fact, the morphological rules of Aronoff (as in (68)) are such symbolic rules. Pinker is mostly concerned with verbal inflection. To use Aronoff’s formalism, we could, for example, formulate the regular English preterite formation as in (70):

\[
(70) \quad [[X]\text{v}\text{ed}]_V \quad \text{‘X-PRETERITE’}
\]

As the reader can see, this rule applies to any verb. It is possible to interpret this in an extreme way, in line with traditional generative assumptions: *all regular forms are always generated “on line”, and never stored in memory*. This interpretation is, however, probably wrong, for reasons I will discuss below\(^{157}\). Irregular forms are retrieved directly from the lexicon. To return to the comparison of adjectives, there is no active morphological rule for it: it is performed by the syntax in regular cases, and suffixed forms are stored in memory, retrieved from there if needed.

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\(^{156}\) The formulation “with blind necessity” — “mit blinder Notwendigkeit” — goes back to the Neogrammarians: they applied it to the operation of sound laws.

\(^{157}\) I must emphasise that Pinker et al. do not adopt this interpretation.
There are some problems, though. First, if symbolic rules are “blind”, why cannot they generate forms such as *bringed, *teached, *comed, *eated, etc.? There are two solutions I am aware of. The first one is proposed by Aronoff (1976:43): blocking, which means that the general morphological process is blocked by the existence of a lexical entry with which it would be entirely synonymous. As for derivation (which is what Aronoff is concerned with), we can cite the non-existent adjective *ungood: it is not “wrong” because it is structurally ill-formed, but because there is already a word, viz. bad, which means precisely the same. The same is valid for non-forms such as *eated: it is blocked by the existence of ate. The blocking solution, however, suffers from a serious flaw, at least in case we want to maintain that symbolic rules are indeed symbolic, i.e., blind to lexical identity. Specifically, it assumes that the general rule must check the lexicon before applying to see if there’s something to block it. This means, however, that the rule is not blind after all, and must make crucial reference to the lexical identity of the base X. This fact apparently invalidates the whole WRM.

Yet, there’s another solution, which I find convincing. This solution uses blocking, too, but removes an age-old assumption about the application of lexical retrieval (= irregulars) versus on-line generation (= regulars): that their application is an “either — or” choice, i.e., either one or the other is applied to produce an inflected form, depending on the lexical identity of the base. Proponents of WRM claim that this is not the case: the two mechanisms, retrieval and generation, are invoked simultaneously, and the faster one wins. That is, to take a specific example, the speaker wants to express ‘walk-Pret’. Search for a possibly existing lexical entry and generation of a regular form start at the same time; as there’s no lexicalised form, retrieval fails to come up with anything, so the regular form walked will be produced. In case there is a lexicalised form, as in the case of go, retrieval produces the form went before the regular morphology manages to assemble a form *goed. The reason why lexical retrieval is faster is that the item went is frequent: it is retrieved fast and easily. The explanation relies on a well-known fact about irregulars: they are among the most frequent items in the language. Crucially, if an irregular item is not frequent enough to make retrieval the sure winner of the race, regular forms can be produced. Indeed, there are several verbs in English (and probably all over the place) which have two preterites: an irregular and a regular one, such as dreamt ~ dreamed. The appearance of dreamed is easily explained with the “race between the mechanisms” model. That is, blocking may not be a principle of grammar at all: it

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159 Pinker 2000:123f provides a striking fact about English, based on a corpus of 1 million words of text: the ten most frequent verbs are all irregular (not surprisingly, be leads with 39,175 occurrences, get is the last one with 1,486).
may be but a side-effect. The fact that *goed* et alia are always blocked is a consequence of the invincibly fast retrieval of *went* (except in child language, where regular forms are generally produced at a given stage of acquisition)\(^{160}\).

As I mentioned, there is a (still widespread) view that regular forms are *never* memorised but always generated on line. In fact, it appears that this view is not correct. For one thing, if it is the case that “memory is constantly working alongside rules” (Pinker 2000:137), it would be strange if regular forms could not be memorised, especially very frequent ones\(^{161}\). Moreover, as Pinker (ibid.) points out, WRM only claims that “people don’t *depend* on stored past-tense forms, not that they are *incapable* of storing them” [emphasis original]. Second, the phenomenon of lexicalisation assumes lexical storage. Let me elaborate on this. Lexicalisation is understood in the following sense: morphologically complex forms acquire unpredictable properties, i.e., they become arbitrary\(^{162}\). For example, *readable* has become lexicalised in a semantic sense: it is opaque. Specifically, it “ought to mean” ‘able to be read’, based on the semantics of the base and the suffix, but it does not: instead, it means, ‘enjoyable as a reading’. If one assumes that regular formations are never stored, we encounter a problem I call the *Lexicalisation Paradox*\(^{163}\): if *readable*, to use this particular example, is always generated on line (and then erased from memory), how can it acquire an unpredictable meaning? It cannot. This paradox is, however, apparent: if one assumes that regulars may be stored, there’s no paradox: *readable*, a stored item, may take on peculiar properties. Aronoff (1976:18) gives an excellent formulation:

> Words, once formed, persist and change; they take on idiosyncrasies, with the result that they are soon no longer generable by a simple algorithm of any generality. *The word gravitates toward the sign* [emphasis mine - L.K.](...) Words, though they may be formed by regular rules, persist and change once they are in the lexicon (...)

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\(^{160}\) The side-effect status of blocking may not be entirely true, though. Notably, it has been observed that language avoids total synonymy, and blocking is probably a derivative of this avoidance (Aronoff & Anshen 1998:240). Yet, pairs such as *dreamt ~ dreamed* are synonymous. I suspect that blocking is the result of an interaction of (at least) these two factors.

\(^{161}\) One often memorises sentences, too, though they are undoubtedly generated. Or, to take another example, think of the words of TV advertisements which one hears all the time: we often memorise them, even if we do not intend to do so. In other words, whatever is frequent enough will be stored.

\(^{162}\) *Lexicalisation* is often used in another sense which is also called *lexical coding*, i.e., the fact that there is a lexeme for a particular concept in a language.

\(^{163}\) Kristó 2004:267f.
The emphasis in the quoted passage is important. I understand it in the sense that the word, once stored, tends to become a Saussurean, i.e., arbitrary, sign: arbitrariness, a truly basic principle, constantly attracts items as a kind of “black hole”\(^{164}\).

To sum up this lengthy discussion, there seems to be abundant evidence, structural, semantic, and psychological, to support the assumption that in languages such as English the regular morphology is always analytic, while irregular morphology is synthetic (the latter point is explicitly made by Kaye 1995:311). This property of English morphology is, in the light of WRM, not accidental. As I mentioned earlier, Scheer assumes that morphology decides if boundary information is shipped off into the phonology or not. Putting the overall picture together, it seems that the morphology, after all, may not be an autonomous decision-maker. Instead, regular morphology (at least inasmuch as it operates symbolically) cannot choose not to represent boundary information phonologically, for the simple reason that it takes the symbolic form exemplified in (68) and (70), the latter repeated here as (71) for convenience:

\[
\text{(71) } \text{[[X]ed]}_v \quad \text{‘preterite of X’}
\]

Such rules are word-based: they do not manipulate stems. X is an autonomous lexical entry, represented phonologically in accordance with the Lexical Entry Principle, requiring that all entries have a (CV)* skeleton. If X ends in an empty \(v\) (FEN), so will it if concatenated with -\(ed\). The regular morphology may not modify this property of the base. I assume that this is probably cross-linguistically true for all regular word-based morphological processes that are statable as a symbolic rule\(^{165}\).

Recall, however, our discussion of Word-and-Paradigm type morphologies. Can we claim that in such systems, there are symbolic rules? Take Latin nominals, for instance. Following Bybee, I assumed that a particular inflected form is used as a “basic” one: it is in this form that the word is stored in memory. I presume that it is the AccSg for each nominal. Now, in order to know what the Nominative Plural of ‘apple’ is, you need two bits of  

\(^{164}\) It is impossible for me not to quote Saussure himself here (1983:68): “No one disputes the fact that linguistic signs are arbitrary. But it is often easier to discover a truth than to assign it to its correct place. The principle stated above is the organising principle for the whole of linguistics (…) The consequences which flow from this principle are innumerable. It is true that they do not all appear at first equally evident. One discovers them after many circuitous deviations, and so realises the fundamental importance of the principle.” It is hardly possible for a linguist not to be moved by the depth of insight expressed by these words of the greatest master of linguistic thought.

\(^{165}\) Rebrus, Siptár, Szigetvári & Törkenczy 1996 suggest that in a rich inflectional morphology like that of Hungarian, things may not be as simple as that. See also Rebrus 2000.
information: the AccSg form \( mālum \), plus the fact that it is a Neuter noun. Based on this information, one can predict the NomPl, which happens to be \( māla \). The same AccSg form, combined with “Masculine”, identifies the lexeme ‘mast, pole’ and yields the NomPl \( māli \). An AccSg form \( terram \) ‘earth’ (Feminine) leads to NomPl \( terrae \) \(<ae> = \text{ai}\); etc. Crucially, since I assume that the root is not a lexical entry on its own, there’s no way to say that the NomPl of, e.g., ‘apple’ can be formed using a symbolic rule such as the one in (72):

\[
(72) \quad [[X]_{N} a]_{N} \quad ‘\text{NomPl of } X’
\]

where \( X \) is a Neuter o-stem noun.

Note that \( X \) not being a lexical entry, (72) is not possible. WP-type inflection, however regular, must make reference to the basic word form to identify the paradigm. Furthermore, root-based morphology, by definition, means that (bound) roots are not assignable to a syntactic category: it is only by virtue of being combined with a stem-forming element that they acquire “nounhood” or “verbhood”, etc\(^{166}\). This, nevertheless, is not tantamount to the claim that Latin and similar languages do not have any symbolic rules at all (syntactic rules, for example, certainly are symbolic).

4.5. [Morphological typology]

The final part of this chapter is devoted to two problems which are interrelated: the nature of fusional vs. agglutinating concatenations as well as the domain of phonology. The division of morphologies into fusional and and agglutinating is one of the oldest classifications in linguistic science, going back as early as the first half of the 19\(^{th} \) century. The model of morphological typology upon which the modern division is based was originally proposed by Schlegel, but it was Humboldt 1836 who introduced the classification in what is virtually its modern form. The Humboldtian classification, as well known, sets up four language types, viz. isolating, agglutinating, fusional, and polysynthetic. I will be concerned with agglutination and fusion only, since isolating languages (ideally) have no morphology (apart from, possibly, compounding): there is a one-to-one correspondence between words and

\(^{166}\) In Aronoff’s 1976 generative model, it is actually possible to state such rules, provided one assumes a truncation rule deleting the AccSg marker. Under such an analysis, \([X]_{N} = \text{the AccSg form, a full entry, and it is concatenated with the selected NomPl suffix; then the AccSg suffix is truncated, so: } māl-um \rightarrow māl-um-a \rightarrow māl-a. \text{ I don’t know if the use of truncation rules can be independently motivated for Latin nouns. For truncation rules, see Aronoff 1976:88-97. Aronoff 1994 offers a detailed (different) analysis of Latin inflectional morphology.}
morphemes. Polysynthesis is probably but an extreme case of fusion, i.e., its independent status is dubious, and it does not occur in the languages under investigation anyway.

It soon became clear, though, that languages do not quite fall neatly into any of these idealised types. Sapir 1921 attempts to make the classification more detailed by separating several aspects of typology, thereby making it possible to categorise a language in a more precise way. He, however, was concerned with the general architecture of particular languages, a problem I do not wish to deal with, so his model is not quite relevant for us. The same goes for later attempts such as Greenberg 1960. The history of the issue is presented in some detail in Goyvaerts 1975.

Considering the fact that the agglutinating/fusional division of affixation is received wisdom in linguistics, being referred to and used all the time, it is somewhat surprising to discover the terminological and conceptual confusion surrounding it. Some definitions will suffice to illustrate the point. Bybee (1985:45) defines the difference as follows:

In agglutinative languages, morphological boundaries coincide with phonological boundaries (especially syllable boundaries) to an extent that makes segmentation of morphemes transparent. In fusional languages, there is greater fusion of morphemes characterized by sandhi at boundaries, allomorphy, and simultaneous expression, all of which make morphological segmentation more opaque.

Bybee’s definition is mainly phonology-based. By contrast, let us quote the position of Malmkjær (1991:273):

An agglutinating or agglutinative language is one which attaches separable affixes to roots, so that there may be several morphemes in a word, but the boundaries between them are always clear. Each morpheme has a reasonably invariant shape. (...) An inflecting, flectional or fusional language167 is one in which morphemes are represented by affixes [emphasis mine — L.K.], but in which it is difficult to assign morphemes precisely to the different parts of the affixes.

167 All these terms are in use with the same meaning. The terms inflecting or flectional, however, can give rise to misunderstanding, because they imply that the language in question has inflection in general, and that’s also true for agglutinating languages. I use fusional, therefore.
This formulation seems at first sight not to differ in essence from Bybee’s. Yet, the highlighted part (morphemes are represented by affixes) makes it clear that Malmkjær understands the term morpheme in an abstract sense, i.e., corresponding to a function or inflectional category, such as Accusative or Plural, for instance. This is not true for Bybee’s formulation, which implies a definition of morpheme in a more concrete sense: a morpheme must have phonetic shape. This is in line with Bybee’s general position on grammar. Crystal 2003:233f, too, bases his definition of fusional (inflecting, in his terms) languages on an abstract interpretation of the term morpheme, as opposed to morph, saying that words in such languages typically contain more than one morpheme, but, unlike in agglutinating languages, there is no one-to-one correspondence between these morphemes and the linear sequence of morphs (...) the inflectional forms of words may represent several morphological oppositions (...) 

Comrie (1989:43f) defines the difference basically along the same lines, although he does not adhere to the abstract interpretation of morphemes, saying that “the expression of different categories within the same word is fused together to give a single, unsegmentable morph”. Note the use of the term category rather than morpheme. Comrie (ibid.) uses Turkish and Russian as examples for an agglutinating vs. a fusional language. The DatSg and DatPl forms of Turkish adam ‘man’ is adam-a and adam-lar-a, respectively: the Dative is expressed uniformly by -a, and -lar is the regular exponent of Plural. In other words, Turkish nouns do not really have a DatPl form as an autonomous member of the paradigm; indeed, there are no paradigms in the WP sense. Compare this to Russian, where stol ‘table’ has a DatSg stol-u and a DatPl stol-am: the suffixes simultaneously express Number and Case (and, in the case of -u, Gender as well). This is what is meant by Comrie’s definition of fusional morphology: categories are “packed together” in one morph — hence the term fusional. The simultaneous expression of categories in one morph has also been called cumulative/multiple exponence, and such morphs are referred to as portmanteau morphs.168 We may represent this as in (73):

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Fusional morphology tends to exhibit interaction across morphs, too. This is called extended or overlapping exponence, and Spencer (1991:51f) illustrates it using the English Past Participle written (reproduced here as (74); the Latin Present Perfect Sg1 form rēxī ‘I (have) ruled’ illustrates both in abundance (75)):

(74) Extended exponence

<table>
<thead>
<tr>
<th>Meaning (category):</th>
<th>‘write’</th>
<th>‘PastPart’</th>
</tr>
</thead>
<tbody>
<tr>
<td>Form (realisation):</td>
<td>rīt</td>
<td>ən</td>
</tr>
</tbody>
</table>

In (74), we can see that ‘PastPart’ is realised not only as a suffix but as a different allomorph of the root, too. In (75), ‘Perfect’ is expressed by a suffixal morpheme as well as a root with long vocalism plus the selection of Person/Number suffix (cf. rēg-ō ‘I rule’, with a short vowel in the root and an Imperfect Present suffix -ō)\(^\text{169}\).

\(^\text{169}\) The devoicing of the final plosive of the root is, of course, the result of an automatic phonological process, having nothing to do with the morphology.
I will now attempt to give a working definition of fusion. First, however, a clarification is needed. I use the term *morpheme* in a non-abstract sense, i.e., morphosyntactic categories such as Accusative or Plural are not morphemes: a morpheme must have phonological substance. Accordingly, the Russian DatPl suffix *-am* is one morpheme, since it cannot be segmented according to the two categories it signifies. A possible formulation of fusion is found (76):

(76) *Fusion means the exponence of two or more meanings in one morpheme.* It can manifest itself in two ways:

1. **Affixal fusion:** an affix morpheme represents two or more meanings.
2. **Radical fusion:** a root morpheme represents two or more meanings.

Affixal fusion is exemplified by Ru *-am* ‘DatPl’. As the reader can see, it always involves non-lexical meanings, i.e., morphosyntactic categories, in the case of inflectional morphology. Radical fusion is exemplified by *written*, where the root √*ritt* fuses ‘write’ and ‘Past Participle’. In this case, it is accompanied by extended exponence, since ‘PastPart’ is also expressed by suffixation. This, however, need not be the case: in the case of *wrote*, there is no suffix, and the root itself expresses ‘write’ as well as ‘Preterite’. An important note is in order here: extended exponence, it appears, necessarily implies fusion. If this was not the case, we would have instances where a single meaning is represented by two morphemes, and neither morpheme expresses any further meaning. This does not seem to hold, at least not for inflectional morphology. Extended exponence, therefore, can be treated as a side-effect of the simultaneous presence of radical fusion and affixal realisation.

Let us now turn our attention to the highly problematic question of when one can talk about radical fusion. To understand why the question is problematic, consider the variation in the phonetic shape of a root caused by its being concatenated with a suffix. First, there are purely

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170 From now on, I will use the terms *form* and *meaning* in the following sense: *form* is understood as a phonetic string (morph); *meaning* includes grammatical categories (e.g., Accusative) as well as lexical meaning.

171 Affixal fusion in derivational morphology is much rarer: generally, derivational morphemes express one meaning. Counterexamples include, for example, agentive suffixes distinguishing natural and/or grammatical gender, such as F *-eur* ‘agent-Male’ vs. *-rice* ‘agent-Female’. I will neglect derivational morphology here.

172 I will henceforth use the symbol √ to indicate bound roots. A free root will be given without this symbol.

173 It may hold, however, for semantically opaque derived forms such as *cômparable* ‘equivalent’. This is a matter of analysis. Within the domain of inflection, circumfixes could serve as an example, but only if we treat them as two separate morphemes rather than a single discontinuous one. I leave these questions open.
phonologically governed alternations. In English, for instance, the phonetic form of the morpheme *tell* differs in *telling* vs. *tells*: the former contains a clear*l*, the second a dark*l*. Could we say, for instance, that the variant *tet* fuses ‘tell’ and ‘3SgPresInd’? No, because the variation is an automatic consequence of an independent (and mechanical) phonological process, and as a result, *tet* does not identify ‘3rdSgPresInd’. This is not even a case of allomorphy: the morphology does not know about the alternation.

Let us now consider the adjective *long* and its comparative form *longer*. The latter is pronounced with a *g*, the former without it. I discussed this phenomenon earlier, and came to the conclusion that *longer* is lexicalised, rather than being the product of the active morphology. In this case, however, one might rightfully ask, whether we could say that the variant √ *long* is, in fact, a simultaneous representative of ‘long’ as well as ‘Comparative’? In order to understand what is at stake, consider the long-vowel variants of certain Latin roots in the Perfect, such as in *rēxī*. This is not the only verb which uses a lengthened root in forms of the Perfect, cf. *vēn-ī* ‘I have come’ (vs. *vēn-iō* ‘I come’), *vīd-ī* ‘I have seen’ (vs. *vīd-eō* ‘I (can) see’), etc. In the Latin case, the long root variant is clearly associated with the Perfect. The lengthening is not a consequence of the fact that it is concatenated with a given suffix; indeed, notice that ‘see’ and ‘come’ do not use a suffix -*s* to denote ‘Perfect’. As a result, the term “lengthening” is not quite appropriate: the vowel does not lengthen. Its length is lexically given. I assume that √ *vēn* and √ *vēn* (and similar pairs) are, in fact, different morphemes. This assumption may sound radical, but let me elaborate on it. Allomorphs of a morpheme ought to have an identical meaning; √ *vēn* and √ *vēn* do not, because the former means ‘come-Imperf’ while the latter means ‘come-Perf’. *The aspectual difference is lexically encoded in the root.* In other words, this is an instance of suppletivism (understood, of course, in a non-historical sense, i.e., phonological unrelatability, cf. Chapter 2), much like E *sing* and *sang*: ‘come’ in Latin is expressed by two morphemes.

Returning to *longer*, the same cannot be claimed. The bound root √ *long* does not lexically encode ‘Comparative’ — it is found in the Superlative, too; cf. also *simple* √ *simpl*, whose bound variant, √ *simpl*, is found not only in the Comparative and the Superlative, but in the adverb *simply* as well. In these cases, the alternation is a consequence of the fact that the bound root sits together with a suffix within a synthetic domain, and the difference in phonetic interpretation follows from this. As I said earlier, I regard the variants *long* and √ *long* as

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174 They are not autonomous lexical entries, though, in the sense that both are bound roots.
allomorphs, the context for the allomorphy being the lexical vs. morphological nature of the concatenation.

Let me now attempt to define what a fusional concatenation is. I propose that a morphologically complex word form is fusional if and only if it exhibits fusion, either affixal or radical, as defined above. This has an important consequence: the terms fusional and synthetic are not synonyms. It may be true that fusional morphology tends to be synthetic, which is probably a reflection of the fact that fusion in the morphological sense (which inevitably involves function or meaning, as it follows from the very definition of fusion) is preferably not projected into the phonology (by not marking boundaries); but there is no equivalence. Neither is agglutinating the same as analytic. An agglutinating concatenation is one which fails to exhibit any fusion; an analytic concatenation is one whose phonology reflects its morphological complexity. Let me now give some examples to convince the reader that the Kayean classification analytic/synthetic is not a reincarnation of the old agglutinating/fusional division: instead, using the definitions I have provided, it is possible to draw a line between the two. (The line, alas, will not always be clear in practice, but the principle should be clear; I will return to the problematic cases later on.)

First, consider the English 3SgPresInd suffix -(e)s, as in eats, watches, bleeds, etc. According to the Kayean classification, the suffix is analytic, just like the homophonous nominal Plural -(e)s: the allomorphy it exhibits is phonologically conditioned, and, more importantly, its concatenation with the base yields monomorphemically impossible sequences, such as dz, mz, ts, etc., as in bleeds, comes, eats, respectively\(^{175}\). Yet, it exhibits multiple affixal fusion: it simultaneously expresses Person, Number, Tense and Mood. It is universally accepted that multiple exponence is symptomatic of fusional affixation, and I have adopted this view as well: as a result, we have an analytic non-agglutinating suffix. Note that the Plural -(e)s is agglutinating, as it only expresses Number.

For a reverse situation, consider the Hungarian nominal Plural, realised (in regular cases) as -k or -ok/-ek/-ök, cf. hajó-k ‘ships’, pár-ok ‘pairs’, kép-ek ‘pictures’, kör-ök ‘circles\(^{176}\). This suffix is par excellence agglutinating in the sense that it only expresses ‘Plural’, and it takes further suffixes to mark cases, cf. hajó-k-nak ‘ship-DatPl’, compare hajó-nak ‘ship-DatSg’ (just like Turkish). On the other hand, this suffix behaves synthetically

\(^{175}\) Though watches and other cases where the base is sibilant-final are problematic, since there is obviously a melodic restriction at work. I leave this question open.

\(^{176}\) The analysis of Hu data is based on Rebrus 2000. Note that the choice between -ok/-ek/-ök is dictated by vowel harmony; the details are irrelevant.
in many (if not all) cases. For example, if the last syllable of the stem contains an alternating vowel (floating segment), it regularly remains silent in the plural, e.g. \textit{bokor} ‘bush’ \textasciitilde \textit{bokør}, \textit{tükör} ‘mirror’ \textasciitilde \textit{tükørək} ‘id.-Pl’, etc. (where the \textit{ø}, of course, indicates zero, not a front rounded vowel). Compare this to the Terminative suffix -\textit{ig}, e.g., \textit{bokorig} ‘bush-Term’, \textit{tükörig} ‘mirror-Term’. The latter behaves just like English -\textit{ing}: it’s analytic, which is why the \textit{ø} surfaces (attaches to its V position): it is followed by a C + an E-Governed FEN: and FENs cannot govern floating melodies. In \textit{bokrok}, however, it is followed by a C + a full vowel within the same domain, hence it is doomed to remain silent via PG (this is exactly parallel to E travelling vs. simpler). Let me give the full representations for the sake of clarity\textsuperscript{177}:

\textbf{(77) (a)}

\begin{center}
\begin{tabular}{cccccccc}
\hline
\text{PG} & E & G & E & G \\
\text{C} & V & C & V & C & v & C & v \\
\hline
\text{b} & o & k & o & r & i & g \\
\end{tabular}
\end{center}

\textbf{(b)}

\begin{center}
\begin{tabular}{cccccccc}
\hline
\text{PG} & E & G \\
\text{C} & V & C & V & C & V & C & v \\
\hline
\text{b} & o & k & o & r & o & k & \emptyset \\
\end{tabular}
\end{center}

Here, then, we have a situation where an agglutinating suffix is synthetic. It would not be possible to claim that \textit{\sqrt{bokr}} is an exponent of the category ‘Plural’: it is found with all synthetic affixes.

The above examples are but illustrations; further empirical evidence is needed to support or falsify my claim. Let me now turn to problematic cases. I take the English irregular weak Preterites, such as \textit{fed} or \textit{kept}, as an example. Consider the former first, i.e. \textit{fed} (cf. \textit{feed}). The only difference between the Present and the Preterite is in the root vowel, much like in the case of strong verbs: there is no overt suffix. It appears that this form is, therefore, fusional:

\textsuperscript{177} The \textit{ø} in \textit{-ok} is analysable as a floating segment itself; this is not important for us here.
the single morpheme fed means both ‘feed’ and ‘Preterite’, exemplifying radical fusion. Take now kept: what do we do with this form? As there is a Preterite suffix, whether the vowel difference is a simultaneous significant of ‘Preterite’ or just a side-effect of the concatenation boils down to the answer we give to the following questions:

1. Can the difference be exhaustively derived from the fact of kept being synthetic? In other words, is the difference in the form of the root an automatic consequence of the syntheticness of the concatenation (as in the case of long ~ longer)? If so, the form is not fusional. If not, a further question must be considered.

2. Is the i ~ e alternation unique to the preterite or not, in a parallel fashion to the short ~ long difference in the Latin Imperfect ~ Perfect roots? If yes, the form is fusional; if not, the problem is rather complicated.

Let us consider kept in this light. Our answer to the first question, given the framework we have adopted, is a definite “no”. There is no possibility to derive \( \sqrt{\text{kept}} \) from kitp, or vice versa, or to derive both from a third form. The simple reason for this is that the vowels are qualitatively too different. Phonology, in our model, may not modify melody in this way unless there is an environment providing melodic trigger. It is perfectly possible, for example, to phonologically express an alternation between a high front and a mid front vowel if there is an adjacent segment with an appropriate melodic content. For example, imagine a language in which a mid front vowel of the root alternates with a high front one if there is a high front segment in the following syllable. In fact, Germanic exhibits precisely this kind of process. In Proto-Germanic, an e became i if the following syllable contained an i or a j. The root *\( \sqrt{\text{et}} \) ‘eat’, for instance, appeared in this form in the infinitive *etana. In the 2\(^{nd}\) and 3\(^{rd}\) person of the SgPresInd, on the other hand, it appears with an i: *it-is, *it-iþ, respectively. As long as the alternation is phonologically transparent, one can analyse it within GP easily: the element I, present in the vowel of the suffix, creates a natural phonological environment for the raising of e to i. But in the case of keep ~ kept, there is no environment. The phonological treatment of this alternation is therefore ruled out by the Non-arbitrariness principle: phonological events must have phonological causes. Note that in the CVCV version I adopted, the length difference itself is not problematic. Indeed, to anticipate the next chapter, the ME ancestral

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179 In fact, traditional reconstructions would give the infinitive form *etanam. I am reluctant to accept this reconstruction, for the simple reason that there is no proof in the Germanic languages for the final -m. It is reconstructed because PIE did have it; but this is no reason to assume it for PGmc. Reconstruction should go backwards, not forwards, in time (see Hall 1950 for arguments).
180 I leave aside the question of how exactly this process would be formalised in GP.
forms kēp(en) and kēpte, displaying the same vowel (= e) qualitatively speaking, can be derived from a lexical form containing a long vowel, assuming that the Preterite form is synthetic (for the sake of simplicity, I will treat the Infinitive as synthetic, too; see the next chapter); the alternants are shown in (78):

(78)  (a)  Lic  C V c  v₁  C  C  v
      \      |  |  |  |  |
      k e   p a n

(b)  Lic  PG  C V c  v₁  C  v₂  C  v
     \      |  \  |  \  |  |  |
     k e   p t a

The e in kēpte may not spread into the empty v₁ position because the position is unlicensed, v₂ being properly governed. In ME, therefore, we can exclude the possibility that the Preterite form kēpte is fusional: the shortness of the vowel is an automatic consequence of the syntheticness of the form. In MoE, however, this solution is not available: keep and kept are not related to each other phonologically. Hence we must ask ourselves the second decisive question: is the alternation unique to the Preterite?

At first sight, the answer is no: the same alternation occurs in the PastPart, as well as in many other instances, cf. serēne ~ serēnity, deep ~ dēpth, etc. Yet, as I pointed out already, there are instances (e.g., fēd, mēt, etc.) when the alternation is the only exponent of the Preterite. I would therefore make a very tentative suggestion that I term the Uniform Interpretation Principle:

(79) The Uniform Interpretation Principle (UIP)
If a morphosyntactic category x for a given word class Y is expressable by radical fusion only, where α is the alternation formally realising the fusion, then all occurrences of α for members of Y specified for x are to be interpreted as realisations of the same fusion.
Specifically, as the \( i \sim e \) alternation (= \( \alpha \)) may for a given word class \( Y (= a \) lexically defined subset of Verbs) serve as the sole exponent of the Preterite (= \( x \)), as in \( fed, met \), etc., the vowel alternation in \( keep \sim kept \), as well as \( dream \sim dreamt, leave \sim left \), etc., is to be interpreted as the realisation of radical fusion. Preterites of the \( kept \) type, therefore, are understood as instances of extended exponence.

To mention an example of the same kind from another language, I would claim the same for Umlaut as the expression of ‘Noun Plural’ in German. As the vowel alternation can serve as the only exponent of ‘Plural’, as in the case of \( Vater 'fattu \) ‘father-NomSg’ \( \sim Väter 'fettu \) ‘father-NomPl’ etc., I take it to be as the exponent of the same category even if there is an overt suffix, cf. \( Stuhl 'stul \) ‘chair-NomSg’ \( \sim Stühle 'stylo \) ‘chair-NomPl’. Note that Umlaut is by far not unique to noun plurals in German.

The principle I propose may seem to be but a tricky “way out” of the trap. Yet, it embodies a very strong claim: notably, to take an example, that for speakers of English the vowel alternation is as much part of the expression of the Preterite as the presence of the suffix. Intuitively speaking, this sounds reasonable, and it has an advantage: it can be tested (i.e., verified or falsified) psycholinguistically\(^{181}\). Furthermore, it is not even a new idea: instead, it is but one instantiation of a recurrent theme, the most famous expression of which is the “once a phoneme, always a phoneme” principle. Once radical fusion, always radical fusion.

We are still left with a few unresolved cases. English abstract nouns in -\( th \), for instance, always show vowel alternations if the base adjective has a long vowel, including \( i \sim e \), as in \( deep \sim depth \). Yet, the suffix is always present. Is suffixation with -\( th \) an instance of fusion or not? We could say that it is by relaxing UIP, claiming that if the alternation can serve as the unique exponent of some meaning, it is always to be interpreted as an instance of fusion (for any instance independent of class membership or morphosyntactic/lexical meaning). I am not sure whether this relaxation is desirable, and I leave the question open.

This, however, is not the end of the story. I have argued for \( kept \)-type preterites being fusional based on the following assumption: alternants are not related to each phonologically. I

\(^{181}\) Umlaut in nominal plurals in German has been spreading. The word \( Baum \) ‘tree’ (cognate with E \( beam \)) “ought” to have a Pl \( Baum \), but it has \( Bäume \) with Umlaut (it did not have it in Middle High German \( (boum \sim boume) \), see Wright 1955:28,196. In colloquial German, one often hears the noun \( Hund \) ‘dog’ in the Plural form \( Hunde \), not yet accepted in the literary language. These are but two examples, and the spreading of Umlaut may support UIP. Indeed, it is in line with Kuryłowicz’s laws of analogy.
tacitly assumed a GP, more precisely, CVCV, perspective during the discussion. As I pointed out, however, the question whether particular alternants are relatable to each other phonologically is theory-laden. As I relied heavily on phonological relatability when defining fusion, it is impossible to avoid this problem.

Consider the model of phonology we discussed in Chapter 2: that of SPE. In this model, \textit{keep} and √\textit{kep} are derived from a common underlying representation via phonological rules proper. Recall the derivation in (29), repeated here partially as (80) for convenience:

(80) \textit{keep} and \textit{kept}: derivations à la SPE

\begin{tabular}{lll}
\textbf{UR} & \textbf{keep} & \textbf{kept + t} \\
\textbf{PCS} & N/A & kep + t \\
\textbf{VS} & ki:p & N/A \\
\textbf{Voicing assimilation} & N/A & N/A \\
\textbf{SR} & ki:p & kept \\
\end{tabular}

The crucial point is the following: if one adopts a model like classical generative phonology, \textit{kept}, very simply, is by no means fusional. The reason for this is that the phonology is responsible for the difference in form: as we can see from (80), the SR’s of the root are derived from an identical UR. Morphologically speaking, there’s no difference: the root alternants are not different morphemes — not even “real” allomorphs, in fact! By contrast, I claim that (using the CVCV model as I have done) the alternants are, in fact, separate morphemes.

So far, this is probably not very surprising: SPE and CVCV are a world apart in their conception of what phonology is and what it is not, which is why the difference is almost self-evident. I would like to point out, however, that even seemingly little divergencies may count. Specifically, I proceed to compare the status of ME \textit{kĕpte} with regard to fusion in the light of CVCV vs. CGP. The basic assumptions as to what constitutes the domain of phonology are, by and large, common to both theories. Yet, if one attempts to derive ME √\textit{kĕp} as in \textit{kĕpen} and √\textit{ kep} as in \textit{kĕpte} in a Kayean CGP framework, one runs into difficulties. First, let me remind the reader that in the model I assume the two ME root variants are relatable phonologically, cf. (78). Let us now try to do the same in a CGP formalism.
First, *kēpte* must be considered synthetic: otherwise, we would have no reason for the shortening. If, however, it is synthetic, it must be represented as in (81):

(81)

\[
\begin{array}{cccc}
R & R \\
O & N \\
X & X & X & X \\
k & e & p & t & o
\end{array}
\]

Note that monomorphemic occurrences of *pt* are Coda-Onset clusters in CGP; accordingly, they must be represented as such in synthetic domains. Compare this to the CGP representation of *kēpen* in (82):

(82)

\[
\begin{array}{cccccc}
R & R & R \\
O & N & O & N & O & N \\
X & X & X & X & X & X \\
k & e & p & o & n
\end{array}
\]

There is a fundamental problem with relating the two forms to each other phonologically: such a step would clearly violate the Structure Preservation Principle, since constituent structure should be altered. The Nucleus in the root should be deprived of its second X slot, and the *p* would have to move from an Onset into a Coda. Therefore, a phonological derivation of the root alternants from a common underlier is rendered impossible.

As the alternants are not relatable phonologically, the vowel alternation will inevitably be interpreted as lexically given. As a result, one must rely on the Uniform Interpretation Principle. As in ME (at least in early ME) there are no forms in which the shortening alone denotes the Preterite (Preterites such as *fed, met* had a geminate, i.e., *fed-də, met-tə*), we come to the conclusion that the ME forms are not fusional. Accidentally, therefore, CVCV and CGP arrive at the same result, but only accidentally, because the two theories predict the possibility of different results, and it is only due to the UIP, a principle independent of GP or any other phonological theory, that CGP yields the same result as CVCV.
In this long chapter, I hope to have presented a plausible and coherent model of how morphology and phonology interact, based on Kaye’s and Scheer’s models as well as the Words-and-Rules approach of Pinker and others. I proposed a definition of fusion, but it must be noted that it is not in itself sufficient to decide on particular cases: it must be supplemented by a theory of phonology. The choice of different theories may result in different results.

Let us now turn our attention to the morphological structure of Old English, and the way it transformed into a system that is basically identical to the one we know.
The restructuring of nominal inflection

This chapter is devoted to the nature of nominal inflection in OE and is subsequent transformation in ME. The conclusion I will draw is that the traditionally assumed radical difference between OE and ME as regards nominal morphology is somewhat exaggerated. While it is true that OE nominals decline quite differently from ME ones (their inflection is much richer, for one thing), they already show characteristic features that later come to dominate English nominal inflection. Most importantly, the traditional idea that OE nominal morphology is stem-based must be abandoned as an overall claim, and we need to recognise the fact that the system exhibits several important traits of word-based (analytic) morphology, especially those declensional patterns which later become generalised. In the final section of the chapter, I devote my attention to some theoretical implications of the analysis and a parallel case will be presented, too: the restructuring of nominal morphology in Vulgar Latin and West Romance.

5.1. [Preliminaries]

A few notes are in order before I set out to examine the historical development of nominal declension in Early English. My approach will be primarily morphophonological; I will also be concerned with the fusional vs. agglutinating nature of inflection. As a result, I will not be dealing with issues purely morphological, such as gender or case, except where they are relevant for our purposes. Put differently, since my ultimate goal is to find out how the inflectional system is restructured by Middle English, I will only be concerned with categories and forms that are relevant in this respect. For example, the dual, present in some OE pronouns, disappears completely, which is why it has no significance for the present thesis. On a more general plane, I will neglect pronouns: their declension is highly irregular in OE already, meaning that their forms are lexicalised. I will not be dealing with adjectives, either, since they decline similarly to nouns (apart from some cases), and, even more importantly, adjectival declension does not survive the OE period: in ME already, only traces of it remain. I will not treat minor (irregular) OE nominal declensions, either: even in OE, they are clearly historical residues, unproductive and liable to joining the major paradigms. To sum up, I will
concentrate on those inflectional patterns which can be claimed to have a general currency in OE; within these, my attention will be focussed on forms which survive the OE period.

I will assume a CVCV perspective in my investigation. This, of course (as shown in the previous chapter) has an important consequence on morphophonological discussions. For one thing, several items or types I regard as irregular phonologically speaking can be conveniently analysed as regular (phonologically derivable) in an SPE-type framework. The reader is referred to Lass & Anderson 1975 and Anderson & Jones 1977 for classical generative analyses.

5.2. [An overview of OE nominal declension]

Before setting out to analyse the morphophonological behaviour of OE nouns, I provide an overview of nominal morphology. In order to do this, however, some notes are in order concerning traditional presentations of OE inflection.

If one checks the morphology chapters of OE grammars, one will immediately notice a striking fact: the classifications are clearly historically based. Nouns, for example, are divided into stem classes, such as a-stems, o-stems, i-stems, root-consonant stems, etc. Within the individual classes, sometimes further subdivisions are made: for example, a-stems are divided into pure a-stems, ja-stems and wa-stems, etc. This classification reflects PGmc (or even PIE) divisions. However, as Krygier 1998 points out, there are serious problems with this, synchronically speaking.

In traditional Indo-Europeanist discourse, stem classes are identified according to the thematic (stem-forming) elements added to roots. The majority of nouns in the classical IE languages (as well as in PIE) do not add inflectional suffixes directly to the root, but to a stem, consisting of the root + a thematic element. Schematically (where T = thematic element):

\[
\text{Root} + \text{T} + \text{Inflection}
\]

The terms a-stem, o-stem, etc., refer to the thematic element. For example, the PIE word for ‘horse-NomSg’, reconstructed as *ekwos, has the structure shown in (84a), while its NomPl form, *ekwoi, is represented in (84b):

---

182 For this reason, nouns of this type are called thematic, as opposed to a smaller class which does not use a thematic formative but adds the inflectional suffixes directly to the root; these are referred to as athematic.


184 PIE roots are taken from Watkins 1985.
As the reader can verify, inflection is fusional in PIE, as the suffix expresses Number and Case simultaneously. At the same time, the structure of word forms is transparent, and we can easily understand why ‘horse’ is an o-stem. It must be added, however, that already in PIE, the transparent structure suggested here was not implemented in all inflected forms\textsuperscript{185}. Nonetheless, the overall pattern is as described in (83). The same pattern is continued into PGmc with a high degree of consistency, i.e., the majority of forms in PGmc is quite analysable along (83)\textsuperscript{186}. As opposed to this, the system we find in the attested Gmc languages, including the earliest one, Gothic (known primarily from Wulfila’s translation of the New Testament dating from the 4\textsuperscript{th} century AD), shows a breakdown of this transparent system. Look at the following Gothic paradigms, for example (traditional Gmc stem classes and genders are shown in parantheses)\textsuperscript{187}:

\begin{itemize}
\item[(a)] \*ekw o s \‘horse’ T ‘NomSg’
\item[(b)] \*ekw o i \‘horse’ T NomPl
\end{itemize}

\textsuperscript{185} A notable case in point is the NomAccPl of Neuter o-stem nouns, realised as the suffix \(-\ddot{a}\), added directly to the root, e.g., ‘yoke’ NomAccSg = \*yug-o-m (transparent), but NomAccPl = \*yug-\ddot{a}, cf. La iugum, iuga or OCS igo, iga. For non specialists, I would like to point out that (i) all neuter nouns take \(-\ddot{a}\) in PIE in the NomAccPl, irrespective of class membership, (ii) the Nom and the Acc for all Neuter nouns is always identical in each number. See Szemerényi 1990:168ff.

\textsuperscript{186} A note to clarify a confusing point of terminology in Indo-Europeanist vs. Germanicist discourse: in PGmc, \(*\ddot{o}\) and \(*\ddot{a}\) merge in \(*\ddot{a}\), while \(*\ddot{e}\) and \(*\ddot{a}\) merge in \(*\ddot{e}\) (called the Low-Back Merger, see Lass 1994:18 for examples). As the thematic element of o-stems in PIE is short (\(=\ddot{o}\)), it shows up as \(\ddot{a}\) in Gmc; the thematic element of a-stems is, on the other hand, long (\(=\ddot{a}\)), yielding Gmc \(\ddot{e}\). For this reason, PIE o-stems become a-stems while PIE a-stems become o-stems in Gmc. In Germanicist discourse, therefore, the term a-stem is used to refer to Gmc nouns deriving from PIE o-stems, and vice versa.

\textsuperscript{187} Data from Wright 1954:85-94. Note on phonetic values: \(<i> = \text{i}, <ei> = \text{i}, <ai> = \text{e}, <au> = \text{u}, <u> = \text{u/uu}, <b> = (intervocally) \text{\textbeta} \text{or v}\). The rest = their IPA value.
(85) *Four Gothic nominal paradigms*

<table>
<thead>
<tr>
<th>Case</th>
<th>'stone' (a-Masc)</th>
<th>'guest' (i-Masc)</th>
<th>'gift' (o-Fem)</th>
<th>'hand' (u-Fem)</th>
</tr>
</thead>
<tbody>
<tr>
<td>SgNom</td>
<td>stain-s</td>
<td>gast-s</td>
<td>gib-a</td>
<td>hand-us</td>
</tr>
<tr>
<td>Acc</td>
<td>stain</td>
<td>gast</td>
<td>gib-a</td>
<td>hand-u</td>
</tr>
<tr>
<td>Gen</td>
<td>stain-is</td>
<td>gast-is</td>
<td>gib-os</td>
<td>hand-aus</td>
</tr>
<tr>
<td>Dat</td>
<td>stain-a</td>
<td>gast-a</td>
<td>gib-ai</td>
<td>hand-au</td>
</tr>
<tr>
<td>PlNom</td>
<td>stain-os</td>
<td>gast-eis</td>
<td>gib-os</td>
<td>hand-jus</td>
</tr>
<tr>
<td>Acc</td>
<td>stain-ans</td>
<td>gast-ans</td>
<td>gib-os</td>
<td>hand-uns</td>
</tr>
<tr>
<td>Gen</td>
<td>stain-e</td>
<td>gast-e</td>
<td>gib-o</td>
<td>hand-iwe</td>
</tr>
<tr>
<td>Dat</td>
<td>stain-am</td>
<td>gast-am</td>
<td>gib-om</td>
<td>hand-um</td>
</tr>
</tbody>
</table>

It is only possible in some instances (boldfaced) to identify the alleged thematic vowel as different from the inflectional suffix (the DatSg *staina* and the GenPl *gibo* may not even belong here). Note that the i-stems are identical in the Singular to a-stems. The pattern is clear: Gmc (and all IE languages to a lesser or greater degree) is well on the way to give up the inflection based on the schema in (83); instead, they fuse T with Infl, yielding a simple “Root + Infl” type morphology which is basically of a WP kind, though in some cases the root is free, so that the beginnings of a word-based inflection can be detected as well.

The situation is even more advanced in OE (and the other early West and North Gmc dialects). As we will see shortly, it makes virtually no sense to talk about the whole of OE inflection as stem-based. Krygier points out that for this reason, the traditional model of OE inflectional morphology should be given up as a synchronic one, and be replaced by a categorisation based on relevant and productive categories. He points out (1998:120) that the traditional classification was set up by 19th century historians who were interested in a “backward-looking” classification, since their ultimate aim was to reconstruct Proto-Germanic, and, ultimately, Proto-Indo-European. The stem-based classification certainly does facilitate comparison with other Germanic (and IE) languages and makes the clarification of historical origins possible. As his paper is but a programmatic statement, Krygier does not go into details on how the reorganisation ought to be done. Interestingly, he does not mention Hogg 1992b, who implements the same idea and suggests a re-classification based on Plural.

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188 Masculine i-stems, to be precise: feminine i-stems are different.
formation, just like Keyser & O’Neil (1985; Henceforth K&O)\(^{189}\). I will follow Hogg’s and K&O’s line of thought here, especially because I am interested in the “afterlife”, rather than the origins, of OE inflectional morphology.

Before I set out to a presentation of OE morphology, one important point must be clarified. Notably, according to usual practice, I will present OE forms according to the traditional edited orthography used in OE grammars and historical discussions. This does not pose any serious difficulty for those who are not familiar with OE spelling conventions, since the system is highly phonemic. I will only use IPA transcriptions in representations of phonological structures in a CVCV framework, or if I wish to lay particular emphasis on the phonetic form. A fairly consensual view on how OE is supposed to be read (phonetically speaking) is presented in Appendix I\(^{190}\).

Hogg 1992b:122-37 presents a historical overview of how the traditional stem types were restructured and yielded a new categorisation based on Plural formation. The details of the pre-history are, however, irrelevant for our purposes, so I present Hogg’s synchronic classification only, making historical notes when necessary.

Hogg proposes four paradigmatic patterns; the gender associated with the individual patterns is shown in parantheses: (i) \textit{as-Plurals} (M), (ii) \textit{u-Plurals} (N), (iii) \textit{a-Plurals} (F) and (iv) \textit{n-Plurals} (M/F/N). The majority of n-Plurals is Masculine (more than two thirds), with only two Neuter items, and the Feminine ones accounting for a bit less than one third. The patterns in LWS (10\(^{th}\) — 11\(^{th}\) centuries) are as follows:

(86) \textit{Nominal paradigms in OE}

(a) \textit{as-Plurals}: stān ‘stone’, ende ‘end’, bearu ‘grove’

<table>
<thead>
<tr>
<th></th>
<th>Sg</th>
<th>Gen</th>
<th>Dat</th>
<th>Pl</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sg</td>
<td>stān</td>
<td>stānes</td>
<td>stāne</td>
<td>stānas</td>
</tr>
<tr>
<td>Nom/Acc</td>
<td>ende</td>
<td>endes</td>
<td>ende</td>
<td>endas</td>
</tr>
<tr>
<td>Gen</td>
<td></td>
<td></td>
<td></td>
<td>bearwas</td>
</tr>
<tr>
<td>Dat</td>
<td></td>
<td></td>
<td></td>
<td>bearwa</td>
</tr>
<tr>
<td>Pl</td>
<td>bearu</td>
<td>bearwes</td>
<td>bearwe</td>
<td>bearvum</td>
</tr>
</tbody>
</table>

\(^{189}\) It is true, though, that Krygier is concerned with complete grammars devoted to OE, and Hogg 1992b is but an outline, and K&O is not an Old English grammar, either, but a theoretical treatise using OE and ME phenomena.

\(^{190}\) I will also indicate vowel length according to the standard manuals and dictionaries, though Section 6.3. for problems in identifying length in OE.
(b) u-Plurals: *scīp* ‘ship’, *word* ‘word’, *wīte* ‘punishment’, *searu* ‘device’

<table>
<thead>
<tr>
<th></th>
<th>Sg Nom/Acc</th>
<th>Sg Gen</th>
<th>Sg Dat</th>
<th>Pl Nom/Acc</th>
<th>Pl Gen</th>
<th>Pl Dat</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>scīp</td>
<td>word</td>
<td>wīte</td>
<td>searu</td>
<td>wordes</td>
<td>searwe</td>
</tr>
<tr>
<td></td>
<td>scipes</td>
<td>wordes</td>
<td>wītes</td>
<td>searwes</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>scipe</td>
<td>worde</td>
<td>wīte</td>
<td>searwe</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>scipu</td>
<td>word</td>
<td>wītu</td>
<td>searu</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>sēipa</td>
<td>worda</td>
<td>wīta</td>
<td>searwa</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>sćiump</td>
<td>wordum</td>
<td>wītum</td>
<td>searwum</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

(c) a-Plurals: *lufu* ‘love’, *lār* ‘learning’, *sceadu* ‘shade’

<table>
<thead>
<tr>
<th></th>
<th>Sg Nom</th>
<th>Sg Acc</th>
<th>Sg Gen</th>
<th>Sg Dat</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>lufu</td>
<td>lār</td>
<td>sceadu</td>
<td></td>
</tr>
<tr>
<td></td>
<td>lufe</td>
<td>lāre</td>
<td>sceadwe</td>
<td></td>
</tr>
<tr>
<td></td>
<td>lufe</td>
<td>lāre</td>
<td>sceadwe</td>
<td></td>
</tr>
<tr>
<td></td>
<td>lufa</td>
<td>lāra</td>
<td>sceadwa</td>
<td></td>
</tr>
<tr>
<td></td>
<td>lufa/lufena</td>
<td>lāra/lārena</td>
<td>sceadwa</td>
<td></td>
</tr>
<tr>
<td></td>
<td>lufum</td>
<td>lārum</td>
<td>sceadwum</td>
<td></td>
</tr>
</tbody>
</table>

(d) n-Plurals: *nama* ‘name’ (M), *sunne* ‘sun’ (F)

<table>
<thead>
<tr>
<th></th>
<th>Sg Nom</th>
<th>Sg Acc</th>
<th>Sg Gen</th>
<th>Sg Dat</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>nama</td>
<td>sunne</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>naman</td>
<td>sunnan</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>naman</td>
<td>sunnan</td>
<td></td>
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<tr>
<td></td>
<td>naman</td>
<td>sunnan</td>
<td></td>
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<tr>
<td></td>
<td>naman</td>
<td>sunnan</td>
<td></td>
<td></td>
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<tr>
<td></td>
<td>namena</td>
<td>sunnena</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>namum</td>
<td>sunnum</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

The reader will have noticed that the examples given are all monosyllabic stems. We will come back to polysyllabics later on, since they may display Syncope, a phenomenon which requires special attention.

Some of the forms in (86) need comment. Let us examine them all in detail.

The as-Plurals in (86a) continue the PGmc a-stems; historical i-stems have joined them, too, by the time of our attested texts. These nouns are expected to have NomAccPl -e or zero historically speaking, e.g., *wine* ‘friend’, **ġiest** ‘guest’. There are very few instances of this Plural formation, however; *wine* (NomAccPl) does occur in poetic texts (Campbell 1959:241), along with some other nouns (but **ġiest** (NomAccPl) is never found),

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191 The difference between Masculines and Feminines in (d) is minimal: they diverge only in the NomSg. Neuter nouns which belong here are ēāge ‘eye’ and ēāre ‘ear’; they go like Feminines except that their AccSg is identical to the NomSg.

192 Recall that Masculine i-stems are already identical in the Singular to a-stems in Gothic.

193 Choice of -e vs. zero depends on the weight of the root; we’ll soon discuss this phenomenon in connection with Neuter NomAccPlurals and Femine (a-Plural) NomSingulars.
and *winas* is the usual form. The only systematic use of *-e* is attested in the case of names of nations, peoples, or tribes, such as *Engle* ‘Englishmen’, *Dene* ‘Danes’, *Seaxe* ‘Saxons’, *Sumorsæte* ‘the people of Somerset’, etc. These, however, are not used in the Singular and it might be the case that they were treated as collectives rather than “real” Plurals. At any rate, they constitute a special subclass. All other historical *i*-stems show a strong tendency to join the as-Plurals completely, to the extent that most of them had become indistinguishable from historical *a*-stems. 

Within the as-Plurals, three subclasses can be distinguished according to the NomAccSg form: (a) C-finals, (b) *e*-finals, (c) *u*-finals, cf. *stān*, *ende*, *bearu*. The C-final nouns of this class are straightforward as to their morphological makeup: the NomAccSg serves as a free base to which inflectional suffixes are added. The other groups are more problematic. In group (c), the difficulty is not great, though. Observe that the stem in the inflected forms always ends in a *w*. As glides do not appear in the context *C__#*, the obvious solution is that the glide vocalises in the uninflected (NomAccSg) form. This is proposed by Hogg 1992b:129, too. In other words, the final *-u* in the NomAccSg is undoubtedly part of the stem, rather than an inflectional suffix. Group (e), therefore, can be said to follow the same pattern as group (a). The *e*-final nouns of this class are, however, problematic. Historically, the *-e* is not part of the stem, but derives from a thematic element. In synchronic terms, its status is dubious: is it stem-final or is it a NomAccSg suffix? Both answers are possible and both have, indeed, been proposed. We will come back to a detailed analysis shortly; for the time being let me point out the difference between the two possible analyses.

If one assumes that the *-e* in *ende* &Co. is a suffix, one can eliminate stem allomorphy by reducing the number of stem variants to one, viz. *end*-. The price to pay for this option is that nouns of this group will deviate from the rest of the as-Plurals in a very important way: they will exhibit overt marking for NomAccSg. As for the second option, viz. that the *-e* is part of the stem, the problem is that one must assume two allomorphs, i.e., *ende* and *end*-. This solution forces us to admit that such nouns add the inflectional suffixes to a bound stem, not the free NomAccSg form, which otherwise characterises as-Plurals. Notice, however, that the first option suffers from this flaw as well: there, *all* inflections are added to a bound root, e.g., *end*-. I will argue that there are more supporting arguments in favour of analysing *ende* as one morpheme than for a *stem-suffix* segmentation. First, however, let us go on to the other nominal inflectional classes.

The class represented in (86b), i.e., (Neuter) u-Plurals, shows remarkable similarities to as-Plurals, the only difference being in the NomAccPl. This is no accident: historically, this
class derives from Neuter a-stems, while as-Plurals continue Masculine a-stems, i.e., they represent but two (gender-dependent) variants of the same ancient inflectional class. As for the synchronic picture, the \( \text{wīte} \) type presents the same difficulty as \( \text{ende} \) in the as-class; we’ll return to it shortly. There is, however, a striking feature that this class exhibits: notably, the NomAccPl is sometimes realised as \(-u\), sometimes as zero. This requires explanation, especially because the variation is not random: instead, it is based on the phonological “weight” of the stem.

In Pre-OE, unstressed short high vowels (= \( i \) and \( u \)) in open syllables were subject to a process known as High Vowel Deletion (HVD), but the application of the process was restricted by prosodic structure. The phenomenon has received a fair amount of attention in recent decades, and a number of articles and chapters have been devoted to the question, several of them placing HVD in a wider context of prosodically conditioned sound changes in Pre-OE and Germanic, e.g., K&O 1985:4-12, Dresher & Lahiri 1991, Murray 1991, Minkova & Stockwell 1994, Lass 1994:95-102, Ritt 2004:289-306, Bermúdez-Otero 2004, etc. The details concerning the application of HVD in particular instances are complex and often controversial, just like its synchronic status in attested OE, especially in the late period of the language. Nevertheless, the original conditioning factors seem to be quite well understood in a broad outline. Notably, the process applies to unstressed high vowels in open syllables, subject to the following conditions:

1) In disyllabic forms, the first syllable must be heavy, cf. (87a) vs (87b);
2) In polysyllabic forms,
   a) HVD applies to a medial high vowel if the first syllable is heavy, cf. (87c,d,f,g);
   b) final high vowels in polysyllabics delete after a heavy medial syllable (87h) or if the first two syllables are both light, cf. (87e,f,g).

HVD applies to all available high vowels, regardless of morphological structure; indeed, the \( u-zero \) alternation in the NomSg of Feminine a-Plurals (= \( \text{lufu} \) vs. \( \text{lār} \), cf. (86c)) derives from the same process. In Pre-OE, all Neuter nouns had an overt marking in the NomAccPl,
notably, *-u. The same goes for the NomSg of the Feminines of the *lufu/lār* type. Let us see some examples for the sake of clarity.

<table>
<thead>
<tr>
<th></th>
<th>Pre-HVD</th>
<th>Post-HVD (OE)</th>
<th>Gloss</th>
</tr>
</thead>
<tbody>
<tr>
<td>(a)</td>
<td>*lufu</td>
<td>lufu</td>
<td>‘love-NomSg’</td>
</tr>
<tr>
<td></td>
<td>*sčipu</td>
<td>sčipu</td>
<td>‘ship-NomAccPl’</td>
</tr>
<tr>
<td>(b)</td>
<td>*lāru</td>
<td>lār</td>
<td>‘learning-NomSg’</td>
</tr>
<tr>
<td></td>
<td>*wordu</td>
<td>word</td>
<td>‘word-NomAccPl’</td>
</tr>
<tr>
<td></td>
<td>*searwu</td>
<td>*searw &gt; searu</td>
<td>‘device-NomAccPl’</td>
</tr>
<tr>
<td>(c)</td>
<td>*werudes</td>
<td>werudes/werodes</td>
<td>‘troop-GenSg’</td>
</tr>
<tr>
<td>(d)</td>
<td>*hēafudes</td>
<td>hēafdes</td>
<td>‘head-GenSg’</td>
</tr>
<tr>
<td>(e)</td>
<td>*færeldu</td>
<td>færeld</td>
<td>‘journey-NomAccPl’</td>
</tr>
<tr>
<td>(f)</td>
<td>*hēafudu</td>
<td>hēafdu</td>
<td>‘head-NomAccPl’</td>
</tr>
<tr>
<td></td>
<td>*wītiu</td>
<td>wītu</td>
<td>‘punishment-NomAccPl’</td>
</tr>
<tr>
<td>(g)</td>
<td>*werudu</td>
<td>werud/werod</td>
<td>‘troop-NomAccPl’</td>
</tr>
<tr>
<td>(h)</td>
<td>*wēstennu</td>
<td>wēstennu</td>
<td>‘desert-NomAccPl’</td>
</tr>
</tbody>
</table>

The examples given in (87) illustrate the operation of HVD. Note that those occurrences of Pre-OE i which escape HVD later lower to e (e.g., Pre-OE *wini ‘friend-NomSg’ > OE wine). As OE e can derive from other sources as well, most importantly from pre-OE æ in unstressed syllables, it becomes impossible to predict the presence vs. absence of an unstressed e by OE times\(^{198}\). The vowel u, on the other hand, only lowers regularly to o in medial syllables, and even there u is still found in OE texts, cf. the variant forms in (87c,g).

A final note: most recent analyses, starting with K&O 1985, have attempted to state the environments for HVD in prosodic terms, i.e., with reference to foot structures. The details are irrelevant here, and the reader is referred to the literature mentioned above. In any case, HVD clearly ceases to be an active phonological process by OE times, as testified by a number of variant forms such as *wæteru ~ wætru ~ wæter* ‘water-NomAccPl’, *tungolu ~ tunglu ~ tungol* ‘star-NomAccPl’, etc\(^{199}\). Crucially, monosyllabic stems are always well-behaved, so that “analogical” forms such as **wordu, **lāru, etc. are never found\(^{200}\). I follow

---

197 With vocalisation of -w to -u in word-final position.
198 The NomSg ende (vs. wine) is a good example, an e being present after a heavy root syllable — for the simple reason that it goes back to Pre-OE æ which, of course, does not delete at all.
199 See Bermúdez-Otero 2004:7.
200 See Bermúdez-Otero, ibid.
Bermúdez-Otero and assume that the \(-u\)-zero alternation is morphological in attested OE. We can therefore say that monosyllabic stems only take a suffix consisting of \(u\) if the stem is a light syllable\(^{201}\): if the stem is heavy, they choose a zero allomorph. In fact, Bermúdez-Otero (ibid.) points out that there is an interesting tendency observable in WS: Neuter nouns replace the historically motivated NomAccPl \(-u\) with \(-a\). This is not a sound change, and it only applies to Neuter NomAccPl forms, yielding variants such as \(sčipu \sim sčipa\), \(tunglu \sim tungla\), etc. The replacement only occurs in those cases, however, where \(-u\) is also possible, i.e., forms such as **worda** (NomAccPl) are not attested at all. Bermúdez-Otero concludes that the presence vs. absence of the suffix does not depend on the height of the vowel any more: instead, it is a morphological fact about monosyllabic heavy Neuter nouns that they take zero inflection in the NomAccPl, as opposed to light monosyllables (which have obligatory overt marking), as well as polysyllables (which show variation).

Let us now return to the \(u\)-Plurals (in the light of the abovesaid, it might be more appropriate to call them \(u/zero\)-Plurals, but I keep Hogg’s term for the sake of simplicity). We mentioned the problem of \(wīte\) (cf. (86b)), viz. that it may not be clear at first sight whether the \(-e\) is a suffix or part of the stem. The same problem arose in connection with as-class Masculines of the *ende* type. In fact, as far as \(wīte\) is concerned, its behaviour in the NomAccPl seems to suggest that the stem is the full free NomAccSg form, \(wīte\), rather than a bound stem \(wīt\)-. Specifically, the NomAccPl form is \(wītu\). Should the stem be \(wīt\)-, i.e., a heavy one, the NomAccPl ought to take zero, cf. \(bān ‘bone-NomAccPl’, word ‘word-NomAccPl’\). As monosyllabic stems exhibit a regular behaviour in the selection of \(-u\) vs. \(zero\), this is unexpected. If, however, one assumes that the stem is \(wīte\), the fact that \(-u\) is chosen is not surprising. K&O:42 argue along the same lines, proposing a Prevocalic Deletion Rule (PVD) to account for the absence of the stem-final \(-e\) in the inflected forms. Their derivation of the NomAccPl forms \(sčipu\), \(word\), and \(wītu\) runs as follows\(^{202}\):

---

\(^{201}\) This formulation, of course, requires traditional syllable-based analyses to assume that a word-final \(C\) is extrametrical, otherwise monosyllables such as \(sčip\) would be heavy, too. In CVCV, this move is not necessary, since final consonants are onsets which do not contribute to the weight of the preceding Rhyme.

\(^{202}\) K&O:ibid. Note that they assume rule ordering; furthermore, they take HVD to be an active rule of the phonology. Neither assumption is made here.
(88) K&O’s derivation of sčip, word, and wītu

<table>
<thead>
<tr>
<th></th>
<th>sčip</th>
<th>word</th>
<th>wīte</th>
</tr>
</thead>
<tbody>
<tr>
<td>UR</td>
<td>sčip</td>
<td>word</td>
<td>wīte</td>
</tr>
<tr>
<td>HVD</td>
<td>—</td>
<td>word</td>
<td>wīte</td>
</tr>
<tr>
<td>PVD</td>
<td>—</td>
<td>—</td>
<td>wīt</td>
</tr>
<tr>
<td>SR</td>
<td>sčipu</td>
<td>word</td>
<td>wītu</td>
</tr>
</tbody>
</table>

Crucially, they say, HVD must be ordered before PVD, and a stem wīte must be assumed, to achieve the correct results. I do not think, however, that it is inevitable to analyse the NomAccPl form wītu as derived from /wīte-u/; on the contrary, I will argue that this assumption is incorrect. I will come back to this problem shortly; first, however, let us return to the other nominal paradigms.

First, consider the Feminine a-Plurals in (86c). As I pointed out earlier, they show the same -u~zero allomorphy in the NomSg as the Neuter ones do in the NomAccPl. Besides, they have two variants for the GenPl, e.g., lufa ~ lufena. The latter form is the innovative one, taken from the n-Plurals (86d). The most important unique features of the a-Plurals, which set them apart from the previous classes (as- and u-Plurals) are:

1) Nouns of the lufu type seem to have a bound stem, e.g., luf-. The most important proof comes from compounding and derivation, cf. caru ‘care, anxiety, pain’ — carful ‘anxious’, carsēb ‘painful journey’; colu ‘disease’ — copliē ‘miserably’; stalu ‘stealing, theft’ — stalgong ‘stealthy going’, etc.\(^{203}\). It is not unrealistic to assume that compounds are formed using the stem rather than any inflected form\(^ {204}\). With the lār-type, this problem does not arise as the NomSg = the stem.

2) The a-Plurals exhibit multiple instances of syncretism: all three non-nominative cases are identical, and so are all cases in the Plural except for the Dative (and, when the noun takes -ena, in the Genitive). This property, coupled with the boundedness of the stem in the lufu-type, undoubtedly contributes to the fact that this class does not survive the OE period (cf. Hogg 1992b:133)\(^ {205}\).

Finally, let us consider the n-Plurals (86d). Here, too, the most striking facts are: (i) the stem appears to be bound, e.g., √nam-; indeed, this is the form used in derivatives and compounds, cf. nambōc ‘register of names’ (lit. ‘name-book’), namcēb ‘well-known’ (lit. ‘name-known’), etc.\(^ {206}\); (ii) the majority of forms exhibit syncretism (except the NomSg and the Gen/DatPl). I

\(^{203}\) Data from Hall & Meritt 1960.

\(^{204}\) We’ll refer to compounding later on, too.

\(^{205}\) The situation becomes even worse in LOE when unstressed vowels start to merge in schwa, resulting in an almost total loss of inflectional information.

\(^{206}\) Data from Hall & Meritt 1960.
will return to this point later on when discussing the post-ME development of inflectional patterns.

5.3. [Is the nominal morphology of OE fusional or agglutinating?]
Having examined the major classes of nouns in a broad outline, let us now turn our attention to the following questions:

1) How can we characterise OE nominal inflection in terms of agglutination vs. fusion?
2) What can we say about the status of nominal inflection with regard to analyticness vs. syntheticness?

In this section, I consider the first question, while the second one is the topic of Section 5.4.

In the previous chapter, I offered a detailed proposal regarding what should be considered a fusional concatenation. Let me remind the reader of the definition of fusional forms I suggested: a form is fusional if and only if it exhibits an instance of fusion, either radical or affixal (or both). Let us examine the OE data we encountered in this light.

The answer, as I alluded to it in the previous section, is rather straightforward: affixed forms, no matter in which declensional class, always exhibit affixal fusion. Let us take the most productive (and viable) class, that of the as-Plurals, as an example, whose declensional patterns, presented in (86a), are repeated here as (89) for convenience:

\[
\begin{array}{cccc}
\text{Sg} & \text{Nom/Acc} & \text{Gen} & \text{Dat} \\
\text{Pl} & \text{Nom/Acc} & \text{Gen} & \text{Dat} \\
\hline
\text{sān} & \text{sānes} & \text{sāne} & \text{sāne} \\
\text{ende} & \text{endes} & \text{ende} & \text{ende} \\
\text{bearu} & \text{bearwes} & \text{bearwe} & \text{bearwe} \\
\end{array}
\]

Even in the most unproblematic subclass, represented by \text{sān} (where there is no ambiguity whatsoever regarding segmentation into morphemes), each inflected (non-base) form exhibits affixal fusion, i.e.,

\[
\begin{array}{cccc}
\text{-es} & \text{-e} & \text{-as} & \text{-a} \\
\text{Gen} + \text{Sg} + \text{Masc/Neut} & \text{Dat} + \text{Sg} + \text{“non-n”} & \text{Nom/Acc} + \text{Pl} + \text{Masc} & \text{Gen} + \text{Pl} + \text{“non-n”} \\
\text{-um} & \text{Dat} + \text{Pl} & \end{array}
\]
Note that each suffix except the DatPl -um, which is common to all OE nouns, represents at least three morphosyntactic categories. I disregarded, of course, relic paradigms (if one takes them into consideration, the functional load of some endings will be even heavier).

The same is valid for all other noun classes. May I note that some OE nouns, not numerous but quite frequent, exhibit radical fusion, too. These are representatives of the classes traditionally subsumed under the term minor declensions (see Campbell 1959:251-60). They include, for example, fōt (M) ‘foot’, mann (M) ‘man’, mūs (F) ‘mouse’, bōc (F) ‘book’, etc. The NomAccPl of these forms are fēt, menn, mēs, bēc, respectively, clear instances of radical fusion. These nouns, however, have no effect on other declensions; indeed, they show a strong tendency to join the productive classes, and it is only a small number of them that survive into MoE207.

To conclude, the fusional nature of OE nominal morphology is beyond doubt. Nevertheless, it must be pointed out that radical fusion is very rare, and, furthermore, a great percentage of nouns belonging to the widespread declensional patterns have free stems. Let us now go on to examine a much more difficult question: is nominal morphology in OE synthetic or analytic?

5.4. [Analyticness and syntheticness]
As I mentioned, the decision whether the nominal morphology is synthetic or analytic is not an easy one. For one thing, a great number of nouns in the as- and the u-class clearly have free stems exhibiting no variation in phonetic shape, a property which makes them liable to an analytic interpretation — but not necessarily. Indeed, consider the paradigm of stān ‘stone’ (as-Plural), repeated below as (91), with morpheme boundaries shown with hyphens:

<table>
<thead>
<tr>
<th>(91)</th>
<th>Sg</th>
<th>Pl</th>
</tr>
</thead>
<tbody>
<tr>
<td>NomAcc</td>
<td>stān</td>
<td>stān-as</td>
</tr>
<tr>
<td>Gen</td>
<td>stān-es</td>
<td>stān-a</td>
</tr>
<tr>
<td>Dat</td>
<td>stā-ne</td>
<td>stān-um</td>
</tr>
</tbody>
</table>

It is difficult to come to a decisive conclusion. These forms can be analysed both as analytic and as synthetic, as shown in (92a) and (92b), respectively, below, taking the NomAccPl form as an illustrative example (I indicate morpheme boundaries with a hyphen for convenience, but please remember that they are not part of the phonological representations):

207 It goes without saying that MoE irregular Plurals such as mice, men, feet, etc. go back to this pattern.
(92) Two interpretations of *stānas*\(^{208}\)

(a) Analytic

```
        PG            Lic             EG
C  v  C  V  c  V  C  v  -  c  V  C  v
|    |    |    |    |    |    |
s  t  a  n  a  s
```

(b) Synthetic

```
        PG            Lic             EG
C  v  C  V  c  V  C  V  -  V  C  v
|    |    |    |    |    |    |
s  t  a  n  a  s
```

Both structures yield exactly the same result — much like in the case of MoE Present Participles such as *having, telling, feeding*: these MoE examples, too, are analysable either way, and further evidence is needed to prove that *-ing* is, in fact, an analytic suffix. I will now turn to nouns where the two analyses (i.e., synthetic vs. analytic) might yield different results.

First, I return to the problem represented by *ende* ‘end’, an as-Plural Masculine noun, whose paradigm is repeated here as (93):

```
(93)  Nom/Acc   Sg    Pl
      ende      endas
      endes      enda
      ende      endum
```

As I mentioned earlier, the status of the final *-e* in the NomAccSg is dubious: is it part of the stem, or is it a NomAccSg suffix? I delayed the discussion of this problem, but it is now time we came back to it.

First of all, consider the possibility of *-e* in the NomAccSg being a suffix. In that case, we arrive at the same conclusion as for *stān*: a synthetic as well as an analytic interpretation

\(^{208}\) As MoE, OE forbids word-initial #RT sequences, which is why we must assume that all skeletons are empty cv initial. As we are not interested in word-initial position here, I omit these cv’s to spare space, but note that they are there.
would yield the same result. There are, however, some arguments against this interpretation.

Firstly, this would require us to assume that nouns of the *ende* type have a bound stem throughout the paradigm (incl. the NomAccSg), which does not characterise other nouns in the as-Plural class. It is definitely more attractive to find a solution whereby one can uniformly treat as-Plurals as having unmarked free stems in the NomAccSg. Secondly, *ende*-type nouns would exhibit a syncretism of the NomSg and the DatSg, something that does not characterise any OE noun (aside from a negligible number of items, such as *fæder* ‘father’, which are highly irregular in other ways, too). Thirdly, and probably most importantly, *ende* &Co. appear with a final -e in compounds and productively suffixed forms, e.g., *ende-lēas* ‘endless’, *ende-dæg* ‘day of one’s death’ (lit. ‘end-day’), *ende-spēō* ‘epilogue’ (lit. ‘end-speech’); also *wine* ‘friend’: *wine-scīpe* ‘friendship’, *wine-lēas* ‘friendless’, *wine-dryhten* ‘friendly lord’; cf. *stān* ‘stone’: *stān-hol* ‘hole in the rock’ (lit. ‘stone-hole’), *stān-weġ* ‘paved road’ (lit. ‘stone-way’), etc. It is more than reasonable to assume that the stem is used in such compounds rather than a particular inflected form. I conclude, therefore, agreeing with K&O, that all nouns of the as-Plural type be analysed as having a free stem ending in -e plus zero inflection in the NomAccSg. This does not necessarily mean, however, that the same is true for inflected forms: one might as well say that the NomAccSg is the mere stem ending in -e, but in all other cases a bound allomorph √ is concatenated with the V-initial suffixes.

Let us now consider the details.

Remember that K&O assume a Pre-Vocalic Deletion Rule to derive *wīte*. They suppose that the same rule is at work in as-nouns such as *ende* or *wine*. A rule of this kind, however, is not available in a CVCV framework. There are two possible options for us.

First, inflected forms of *ende* &Co. are analysable as synthetic: the suffixes are added to a bound stem *end-*. Under this analysis, *endas*, for example, will have the representation in (94), cf. the synthetic version of *stānas* in (92b):

```
(94)  PG  EG

<table>
<thead>
<tr>
<th>c</th>
<th>V</th>
<th>C</th>
<th>v</th>
<th>C</th>
<th>V</th>
<th>C</th>
<th>v</th>
</tr>
</thead>
<tbody>
<tr>
<td>e</td>
<td>n</td>
<td>d</td>
<td>a</td>
<td>s</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
```

---

209 All examples cited here are taken from Hall & Meritt 1960.
210 Cf. also K&O:50f.
Nonetheless, it is not at all difficult to formalise the idea suggested by K&O that the full stem is present in the inflected forms in a CVCV framework. Recall that CVCV (and GP in general) is an autosegmental model, making use of a possibility which is present in all autosegmental theories: floating segments. Once we assume that *ende*-type nouns end lexically in a floating \( \epsilon \), everything falls into its place. Let us see how it can be done by representing *ende* (NomAccSg = the base form) in (95a) and *endas* (NomAccPl), i.e., *ende-as* in (95b):

\[(95)\]

\[(a)\]

\[
\begin{array}{cccc}
  c & V & C & V \\
  \mid & \mid & \mid & \mid \\
  \epsilon & n & d & \epsilon \\
\end{array}
\]

In (95a), the floating \( \epsilon \) cannot receive PG, since there is no available proper governor: as a result, it is phonetically realised, i.e., attached to its Nucleus. In (95b), on the other hand, it does receive PG from the following \( \alpha \) (the vowel of the suffix), so it remains unattached, hence silent. The reader may ask, nevertheless, how the position indicated as \( V^* \), itself governed, can properly govern the preceding empty Nucleus: after all, we assumed that governed Nuclei may not govern. Note, however, that the Scheerian version of CVCV we adopted here makes a difference between totally empty Nuclei and floating vowels. We can capitalise on this observation in our analysis. Notably, we can claim that a governed V position with a floating melody may govern a totally empty Nucleus, but nothing else (= it may not govern another floating segment). In fact, this assumption is required to explain forms such as MoE *handling* `handliŋ` as well. This form has clearly become lexicalised as a noun (much like *building*), cf. the compound *handling charge*. Accordingly, the I in it is
pronounced non-syllabic, i.e., the word is a synthetic domain. One must, however, assume a floating schwa between the d and the l even if it does not surface, for the following reasons. The sequence nd is separated by a literally empty Nucleus requiring PG. The string dl is not a possible IG domain in English: note that no word may begin with that string, and the ability of a string to occur word-initially in English is a good indicator of its being an IG domain. As dl is not an IG domain, the empty Nucleus between the d and the l requires PG. Being properly governed, this Nucleus could not possibly govern the preceding empty Nucleus, i.e., the one between inside the nd sequence. The only way out of the trap is to assume that there is a floating schwa between the d and the l: this schwa is always properly governed in the word handling, since this word is a synthetic (lexicalised) form: the floating schwa is followed by a full vowel, viz. i, on the Nuclear level. As it is not totally empty, it can govern a preceding totally empty Nucleus (but only that: it may not govern a full or floating V). Schematically (I omit the detailed representation of the final -ng, since it has no bearing on our discussion):

The same goes for monomorphemic forms such as Bentley, Handley, etc. We have no reason to assume that floating vowels are any different in OE. Moreover, this analysis is also supported by the fact that e in OE does take part in vowel ~ zero alternations in other cases as well (we’ll return to this when discussing syncopating nouns). Furthermore, e is never found prevocally in OE. There are, then, general phonological facts about OE which motivate this analysis. Nonetheless, it must be emphasised that no unambiguous decision can be made: (94) and (95b) are both possible representations. The importance of this fact lies in the

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211 Wells 1990 gives the variant 'handlyn as less common. I suspect that handling when used as a noun is hardly ever pronounced with a syllabic liquid nowadays.

212 Apart from the notorious sC sequences, of course. Note that although dl is an obstruent + sonorant sequence, it is not an IG domain, for reasons that need not concern us here.

213 Note that orthographic <eo, eo, ea, ea> represent diphthongs.
prediction it makes concerning language acquisition, i.e., that the Anglo-Saxon child, when confronted with as-nouns of the *stān* or *ende* type, can interpret them in either way, i.e., as synthetic or analytic. Which interpretation is chosen may also depend on what one considers: the paradigm of *ende* only, or the overall as-paradigm: in the latter case, the fact that *stān* &Co. have a free stem in all forms may support an analytic interpretation as in (95b).

The next problematic point is *wīte*. Recall that K&O argue for the stem being identical to the NomAccSg form throughout the paradigm because they cannot account for the appearance of -u in the NomAccPl: a monosyllabic heavy stem *wīt*- ought to take zero, but as *wīte* is disyllabic, the presence of the -u is explained. Yet, this assumption is not necessary; indeed, it makes a wrong prediction. There is a remarkable fact about Neuters of the *wīte* type: they all end in -e. No other Neuter noun does\(^{214}\). Interestingly, this simple fact has passed unnoticed in the literature (to the best of my knowledge). Furthermore, they invariably take -u in the NomAccPl (see Campbell 1959:229-46). Surface disyllabic stems, however, show a great deal of variation, cf. *tungol* ‘star’ \(\rightarrow\) *tungol ~ tungolu ~ tunglu*, *wæter* ‘water’ \(\rightarrow\) *weeter ~ weeteru ~ wætru*, *werod* ‘troop’ \(\rightarrow\) *werod ~ weredu*, etc. Note that the underlying structure *wīte*-u was posited precisely to account for the presence of -u: the stem is underlyingly disyllabic. Yet, treating *wīte*-type nouns on a par with other disyllables predicts the possibility of having a zero suffix in the NomAccPl. This prediction is not borne out: as I mentioned, *wīte* &Co. always take an -u.

The solution I propose is very simple: capitalising on the observation that *wīte*-nouns are e-final, but no other regular Neuter noun is, I simply claim that the morphological rule of NomAccPl formation for u-nouns is as follows:

1. Light monosyllables take an -u, e.g. *sċipu*.
2. Those ending in -e replace it with an -u, e.g., *wītu*.
3. Heavy monosyllables take zero, e.g., *word*.
4. Polysyllables (exc. those in -e) take -u or zero, e.g., *werod ~ weredu*.

The reader can see that the selection of -u vs. zero is crucially dependent on the form of the stem. This provides a very strong argument against an analytic interpretation of the NomAccPl inflection of Neuter u-Plurals, and I take the process to be synthetic, i.e., stem-based rather than word-based. This does not necessarily imply, however, that the other inflected forms of these nouns are also synthetic. Indeed, u-nouns have exactly the same endings in the rest of inflectional cases as Masculine as-nouns, where, as we have seen (and will see), the analytic

\(^{214}\) Except for the n-Plurals ēaġe ‘eye’ and ēare ‘ear’, which, as I mentioned earlier, are the only Neuter nouns of this class, and they are obvious relics.
vs. synthetic nature of inflection is ambiguous. In fact, Bermúdez-Otero 2004 explicitly claims that all inflection in the as- and the u-class is word-based, except the NomAccPl of u-nouns, which is stem-based. I take a less explicit stance, claiming that the rest of the paradigm may be word-based, or, technically speaking, analytic.

Let me now examine four noun types belonging to the as-/u-declensions, specifically the following ones: (i) the type exemplified by dæġ ‘day’ exhibiting a root vowel alternation, (ii) the type exemplified by mearh ‘horse’ and sċōh ‘shoe’, exhibiting a h-zero alternation coupled with compensatory lengthening and the loss of suffix-initial vowels, (iii) the bearu-type.

First, consider the paradigm of the as-class noun dæġ ‘day’ (M) and fæt ‘vessel’ (N). Nouns following this paradigm are monosyllabic ones with an æ in the root syllable (data from Campbell 1959:224f):

\[
\begin{array}{|c|c|c|}
\hline
 & \text{NomAcc} & \text{Gen} \\
\hline
\text{Sg} & dæġ & dæġes \\
\text{Dat} & dæġe & fæte \\
\text{Pl} & dagas & fatu \\
\text{NomAcc} & daga & fata \\
\text{Dat} & dagum & fatum \\
\hline
\end{array}
\]

The pattern, as generally accepted, is that æ replaces æ if the next syllable contains a back vowel. The alternation is not strictly phonologically conditioned: recall the same alternation in strong adjectival paradigms from Chapter 2, where it does not depend on the quality of the following vowel segment but, instead, on whether the following Nuclear position is empty or not. Therefore, the alternation is — both in nouns and in adjectives — statable in phonological terms, but it does have morphological conditioning, too. We must, as a result, assume that nouns of the type shown in (97) are synthetic.

The same goes for nouns like mearh ‘horse’ and sċōh; the paradigms are given in (98), taken from Campbell 1959:225:

---

See Campbell, ibid.
These paradigms do not easily lend themselves to an analytic interpretation, especially that of scōh, where the suffix-initial vowel is regularly dropped, and the GenPl is obviously transferred from the n-declension. The historical origin of these declensional patterns is a sound change deleting h between voiced segments, accompanied by the comensatory lengthening of the preceding short vowel (as shown by the oblique forms of mearh). At any rate, an analytic interpretation of these inflected forms runs into serious (if not unsurmountable) difficulties, just like in the case of dæg and feet. It is to be noted, however, that neither type survives the OE period.

Let us now consider nouns of the bearu (M) (and searu (N)) type, cf. (86ab). Notably, the alternation between u and w appears to require resyllabification, an option which is outlawed in GP. Yet, the version of CVCV I assume here makes it possible to analyse the alternation without resyllabification: again, all we need to suppose is that the u is a floating segment, as in (99) below:

(99) A representation of bearu

```
C  V  C  V
|  |  |  ;
b  æa  r  U
```

The u (i.e., the single melodic element U\(^{216}\)) is not properly governed, hence it surfaces. Consider now the NomAccPl bearwas:

---

\(^{216}\) The element U is assumed to be realised as u or w, depending on whether it is linked to a V or a C, respectively.
In (100), the a of the suffix acts as a suitable proper governor for the Nucleus marked as V*. As this position is governed, the U may not spread into it. It can spread, however, into the following empty c; being attached to a C position, it will be realised as w. Note that in order for this analysis to work in the model I adopt, I must assume that the concatenation is analytic, hence the suffix is c-initial217. The same goes for all inflected forms of bearu as well as the Neuter searu. Therefore, this type does not posit any particular difficulty; indeed, it provides an argument in favour of the view that the as- and u-declensions are analysable as analytic, aside from minor subcases such as the ones mentioned above.

The most interesting variation, however, is exhibited by disyllabic stems of either the as- or the u-declension ending in a single consonant such as tungol (N) ‘star’, hēafod (N) ‘head’, weder (N) ‘weather’, fugol (M) ‘bird’, finger (M) ‘finger’, ofen (M) ‘oven’, etc218. The majority of these nouns displays optional Syncope in the inflected forms, as shown in (100) below, using the Masculine as-declension fugol ‘bird’ and the Neuter u-declension weder ‘weather’ as examples (cf. Campbell 1959:226):

217 For CVCV models which claim that synthetic (as well as analytic) suffixes are C-initial, this assumption is, of course, not necessary. Such models appear to make no prediction as to the synthetic vs. analytic status of inflection in the case of bearu (or ende).

218 In fact, Feminine a-Plurals also exhibit optional Syncope, e.g., firen ‘crime’ — GenSg firen ~ firne, just like adjectives, e.g., yfel ‘evil’ — GenSgMasc (strong declension) yfles ~ yfeles.
Some notes are in order here. First, observe that the NomAccPl of _weder_, apart from the historically expected _weder_, shows “restoration” of -u, with or without Syncope. As we noted earlier, monosyllables are quite well-behaved with respect to the choice between -u and zero. Second, the quality of the alternating (syncopating) vowels requires some comment. The alternating vowel is e, or o in the majority of cases in WS texts, though i and u frequently occur, the former being usual before j, the latter frequently found before m, e.g., sārīg ‘sorry’ — sārīgé ‘id.-GenSgMasc (strong declension), māþum ‘treasure’ — māþumes ‘id.-GenSg’. The quality of the alternating vowel has been explained with reference to Vowel Harmony, i.e., that front alternating vowels are typical after front root vowels and back ones are found after back root vowels\(^{219}\), though there is great fluctuation, and (as we have just seen) the quality of the following consonant may also count. Hogg 1992a:237 suggests that the fluctuation is explainable by assuming that, at least by LOE, the alternating vowels became a schwa. Yet, _<em, en, er, el>_ spelling are frequent in eOE texts, too, even in cases where the phonetic environment would make us expect an _<o> or _<u>_, such as _fugelas, breahtem_ ‘noise’; both Hogg (1992a:237) and Campbell (1959:151) adopt the view that orthographic _<em, en, er, el>_ came to be applied as a possible way to represent syllabic consonants. Therefore, _fugelas_ may be interpreted as _fuyalas_ or _fuylas_ (or, possibly, even _fuyelas_, at least in early texts). The syllabic interpretation of consonants receives striking conformation by the fact that in a large number of instances, uninflected forms ending in a sonorant consonant are written without a vowel letter in the second syllable. For example, alongside _tācen_ ‘token’, _nægēl_ ‘nail’, _wolcen_ ‘cloud’, _māþum_ ‘treasure’, etc., one regularly finds _tācn, nēgl, wolcn, māþm_, etc., respectively. The general assumption is that OE displayed the same VC ~ C pattern as MoE, i.e., the variant spellings show that the alternating vowel could be dropped and the following sonorant became syllabic, cf. Hogg 1992a:237\(^{220}\). The same possibility is assumed in forms like _fugelas, heofenas_ ‘heavens’, etc., as mentioned above.


<table>
<thead>
<tr>
<th></th>
<th>NomAcc</th>
<th>Gen</th>
<th>Dat</th>
<th>Pl</th>
<th>NomAcc</th>
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<td>fugla</td>
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<tr>
<td></td>
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<td>wederes</td>
<td>wederes</td>
<td>weder ~ wedru ~ wederu</td>
<td>weder ~ wedru ~ wederu</td>
<td>wedra ~ wederara</td>
<td>wedrum ~ wederum</td>
</tr>
</tbody>
</table>

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(101) **Optional Syncope**

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Pilch 1970:98 does in fact claim that spellings of this kind are to be interpreted as representing syllabics, i.e., *fuylas and *hēovn̂us, respectively, and he assumes non-syllabics only where the spelling has no vowel letter before the sonorant, e.g., *fuglas, englas ‘angels’. It is a pity Pilch does not give detailed arguments.

The most striking fact about Syncope in OE is its highly optional nature. It is not entirely optional, though: it is more frequent after heavy initial syllables than after light ones. So, a form such as tungoles ‘star-GenSg’ is more likely to syncopate (yielding tungles) than wederes ‘weather-GenSg’. Recall our discussion of HVD and the data presented in (87). We saw that HVD applied to medial high vowels after heavy syllables but not after light ones, so that hēafodes ‘head-GenSg’ > hēafdes but werodes ‘troop-GenSg’ remains. As I pointed out there, this selectivity of HVD has been explained with reference to prosodic structure, notably, indicating light and heavy syllables with L and H, respectively, HVD applies in HLL but not in LLL forms\(^{221}\). A fairly consensual view\(^{222}\) is that HLL sequences are somehow less optimal feet than LLL ones, i.e., they are “overheavy” (cf. Lass 1994:100f and references therein), and HVD applies to optimise feet\(^{223}\).

However, there were a number of stems in pre-OE which were historically monosyllabic, such as *fugl- ‘bird’ and *tungl- ‘star’\(^{224}\). These developed epenthetic vowels in the uninflected NomAccSg, yielding fugol and tungol, respectively (the epenthetic vowels, of course, could drop and the final sonorant became syllabic). The inflected forms, however, did not (at an early stage, at least) develop epenthetic vowels, so that vowel ~ zero alternations arose, e.g., fugol ~ fuglas. This led to an unpredictable situation regarding the applicability of Syncope: LL stems in their suffixed forms (which < LLL) sometimes displayed Syncope, sometimes they didn’t. The language learner, having no access to historical information, had to assume that Syncope is a lexical property of certain nouns, rather than a predictable

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\(^{220}\) See Campbell 1959:151. Interestingly, I have not found Cr# spellings in the manuals among the examples. This is probably the result of an orthographic ban on such spellings rather than a reflection of the possibility that r could not be syllabic, since the vowel letter before the r is subject to variation, cf. pipor ~ piper ‘pepper’.

\(^{221}\) The word-final C, of course, does not make the last syllable heavy.

\(^{222}\) I use the term fairly concensual view with reference to the basic idea behind the different explanations which, however, differ in many ways in the details.

\(^{223}\) Allegedly, this also applies to disyllabic HL stems, which are, under this assumption, not optimal (see Lass 1994:100f, for example). I have never been able to understand why this should be so, especially because they are equal to LLL feet in weight (which are accepted to be okay), but also because HL seems to be a widespread configuration in Germanic. See Ritt 2004:289-306 for a slightly different analysis based on the idea that HVD and Syncope (as well as a number of later changes) work to make OE/earlyME feet as similar to trochees as possible. Note that HVD and Syncope are treated by him as separate changes, a view which is not unfounded; nonetheless, the difference is not important for the present discussion.

\(^{224}\) In PGmc, they, too, had a suffix, either *-az or *-a.
phonological rule. It comes as no surprise, then, that the vowel ~ zero alternation was soon extended to stems which historically lacked it, and, conversely, historically unjustified non-syncopating forms appeared as well; the result is the variation observed in OE texts, illustrated in (101). Even though it may be true that Syncope was more frequent in HLL forms than in LLL ones, this is but a mere tendency, and in no way a watertight, reliable rule. Consequently, the learner is faced with several options as to the interpretation of the optionality of Syncope. Let us see these options in a CVCV perspective.

One possibility is that items which do not display Syncope contain a full vowel rather than a floating one, but syncopating items contain a floating vowel (say, a schwa) in the alternation site. This is not unusual in the world’s languages. In Hungarian, for instance, the word terem ‘hall’ exhibits Syncope in, say, the Plural, yielding term-ek, but the near-homophonous perem ‘edge’ does not, its Plural being peremek. Lexically, terem can be analysed as having a floating e in the second syllable, subject to Proper Government in the Plural form, but perem has a lexically associated e, which cannot be silenced by PG. Such differences exist in a number of languages. Yet, in these languages, the particular items show remarkable stability in their behaviour: whether they syncopate or not is a lexical property, so, for example, in Hungarian, perem never exhibits Syncope but terem always does. In OE, for the same analysis to work, one should assume two different lexical representations for a large number of nouns (and adjectives), a solution I do not find quite attractive.

The other possibility is that OE behaves in the same way as MoE: Syncope is optional, cf. MoE hist[ə]ry ~ hist’ry, etc. This explanation (which I call the “phonological optionality solution”) is much more plausible than the former one, and it has two advantages. First, it predicts that some items may be more liable to Syncope than others. In MoE, it is observed that not all words which have a deletable schwa display Syncope; in general, the more frequent a word is, the more likely its is to syncopate. For example, while memory is very frequently pronounced mem’ry, summary is more often pronounced with a schwa than without it, and mammmary never occurs without a schwa.225 It is not unreasonable to assume that a similar situation held in OE, i.e., certain items were more frequently found with Syncope than others of a similar foot structure. Second, the fact that LLL forms are less liable to Syncope than HLL ones is not difficult to accommodate in this proposal, since optional PG may well depend on prosodic factors, even if not totally conditioned by them.226

225 Data from Bybee 2001:41, based on American English usage.
226 Cf. MoE, where Syncope is not possible before a stressed syllable, e.g. décorate is not found without a schwa.
The problem with this analysis is that optional Syncope seems to show a tendency to become generalised, i.e., applied more and more frequently to an increasing number of words. In MoE, for instance, Syncope affects many more words than it did but a century ago. The same phenomenon is observed in Vulgar Latin and early Romance, where an increasing number of items fell victim to Syncope. This is understandable: PG is a phonological mechanism, which by its very nature “aims” at general application independent of lexical identity. Unfortunately, OE texts do not seem to show such a tendency. It should not be forgotten, though, that the majority of OE texts is in the WS dialect, highly standardised and quite conservative, which may conceal the real language situation. Indeed, it is not at all impossible that Syncope was becoming more and more frequent in the spoken language but the conservativism of the orthography preserves a much earlier stage. Furthermore, as we will see, Syncope appears to be widespread in in eME texts which failed to be influenced by the conservativism of the standard (since there was none at the time). In sum, I find the phonological optionality solution plausible.

I would like to point out, however, that there is a third possibility made available by the theoretical framework used. Note that my aim is to offer possible versions of how the language learner may have interpreted the conflicting data; the phonological optionality solution is a possible, and indeed, viable, interpretation. Recall that we came to the conclusion that the analyticness vs. syntheticness of inflection in the as- and u-classes, especially with words like stān (i.e., monosyllabic C-final items)\(^{227}\), is an ambiguous matter, since either interpretation accounts for the attested forms. I would like to suggest that the optionality of Syncope may also be taken as an indication of this ambiguity.

Consider the alternative NomAccPl forms fuglas and fugelas ‘bird’. Let us try to represent the NomAccPl synthetically (102a) and analytically (102b); I will assume that the alternating vowel is a schwa, but note that this is of no special importance for us:

\[(102)\]

(a) 
\[
\begin{array}{ccccc} 
C & V & C & V & C \\
| & | & | & | & | \\
\text{f} & \text{u} & \text{y} & \text{a} & \text{l} & \text{a} & \text{s} \\
\end{array}
\]

\[227\] Disregarding, of course, some (recessive) subclasses which I treat as synthetic.
In (102a), a synthetic form is assumed. The suffix’s a acts as a Proper Governor for the floating schwa, rendering it silent. In (102b), on the other hand, the stem ends in an E-Governed FEN, which cannot act as a Proper Governor for the schwa which, accordingly, attaches to the available V position and receives phonetic realisation; alternatively, the l spreads into it, becoming syllabic (this is shown by the dashed line).

The language learner, then, when faced with the alternations, may also interpret the presence vs. absence of Syncope as a result of the possibility to assign different structures to the items in question. An obvious advantage of this solution is that it offers a parallel with the stān-type, where the analytic status of the inflected forms is also ambiguous, though there, the ambiguity is not reflected in the surface forms. Another favourable consequence of this assumption is that it bridges a gap between the stem-based system characterising Pre-OE and Germanic in general on the one hand and the word-based system characteristic of later English, including ME already. Morphological restructuring does not happen overnight, and it is more than resonable to think of OE as representing a transitional period which exhibits traces of the old system but, at the same time, it shows characteristic features of the later one. Finally, the possibility of analysing I must note, however, that this solution does not rule out the phonological optionality solution. Indeed, I believe that the two interpretations supplement each other and account for the observed variations. Indeed, the phonological optionality solution must clearly be used to explain the fact that a-Plurals (F), though probably synthetic rather than analytic in their inflected forms, also exhibit optional Syncope, cf. firēn ‘crime-NomSg’: GenSg firene ~ firne. Let us now revisit the a-Plurals.

The a-Plural nouns are all Feminine. As noted, they exhibit widespread syncretism, and, moreover, the lufu-type (= light stems) have bound stems, too. Heavy stems (such as lār), however, display a free stem in the NomSg. It might be tempting to claim that the two types are, in fact, distinct paradigmatic categories. Nonetheless, they have identical suffixes in all cases except the NomSg (where the -u ~ zero choice is predictable), and they are all Feminines as well. This is a strong argument for classifying them as one type. At any rate, the
paradigm is clearly recessive, and nothing but traces of it remain by ME times. An obvious reason for this is the high degree of syncretism a-Plurals display, further reinforced by the merger of unstressed vowels in schwa in LOE.

The same can be said about n-Plurals. As we saw, they have bound stems, as testified by their behaviour in derivation and compounding. Furthermore, they, too, exhibit multiple syncretism. In most dialects of English, this class disappears almost entirely by ME times, too, leaving but isolated relic forms, except in the South, where they enjoy wider currency for some time (more on this below).

The conclusions I can draw from this analysis of OE nominal paradigms are the following. First, of the widespread (major) paradigms, it is clearly the as-type which is most liable to a word-based (analytic) analysis. Within the u-Plurals, all forms except the NomAccPl are similar in their behaviour. Other major paradigms, viz. a- and n-Plurals, are likely to have been synthetic.

Second, even within the as-Plurals (and u-Plurals exc. the NomAccPl) the analyticness of inflected forms is not self-evident, although there are good reasons to believe that at least a substantial percentage of these nouns could lend itself to an analytic interpretation.

Third, the as-Plurals are, without doubt, the most transparent (iconic) class: apart from an easily identifiable subset (such as dæg and mearh), the inflected forms are easily derivable from a free base by the application of general rules: the C-final items (such as stān) present no problem, but the e-final ones (e.g., ende) are quite unproblematic, too, since the lack of the stem-final -e in the inflected forms is phonologically predictable. Note, furthermore, that the individual inflected forms are characterised by unique suffixes. The only exception is the Nom —Acc syncretism, which, however, is so widespread in OE that we may as well consider it to be the default case (cf. Hogg 1997, passim), i.e., we can actually talk about three forms in the productive paradigms: Base (= historically NomAcc), Genitive, Dative. In other words, the base form is understood as ‘Singular, Non-oblique’ in general: it is by now the form unmarked for case in the as-declension and the u-declension; Hogg (1997:103) suggests that by about 1000 A.D., the levelling of unstressed vowels in schwa led to the same situation in the case of lufu-type nouns (more on this below). To remain in the classical OE period, look at stān in this light:
Notice that all affixes are unambiguous markers of the given Number/Case, and this is especially true for -as and -es (the great survivors, as we’ll see), which can only indicate NomAccPl and GenSg, respectively, even if all nominal paradigms are considered\(^{228}\). In fact, it is possible to give symbolic formulae (cf. Ch4) along the following lines:

\[
\begin{align*}
(104) & \quad (a) & \text{[[X]nes]} & \text{‘GenSg’, where X = Masc/Neut} \\
& \quad (b) & \text{[[X]ne]} & \text{‘DatSg’} \\
& \quad (c) & \text{[[X]nas]} & \text{‘Non-obliquePl’, where X = Masc (as-class)} \\
& \quad (d) & \text{[[X]na]} & \text{‘GenPl’ (Note that n-nouns require special info)} \\
& \quad (e) & \text{[[X]num]} & \text{‘DatPl’}
\end{align*}
\]

The same rules apply to u-nouns except (104c). As I suggested, there is no corresponding rule in the Neuter class: I accept the view that the NomAccPl in that class is not liable to a word-based formulation, and must be referred to the lexicon.

The DatSg and the DatPl are uniformly expressed by -e and -um, resp., in all major paradigms (the DatPl -um occurs in all OE nouns, even the most irregular ones); the GenPl can be said to have a uniform marker, too, viz -a, with n-nouns (e.g., nama, sunne) requiring special marking that they take -ena, just like a-nouns (e.g., lufu, lār) which can optionally take -ena. Note that the suffix -ena fails to spread to as- and u-Plurals, even though — especially when unstressed vowels start to merge in schwa — it is a more conspicuous marker of the GenPl than -a; it was probably identified as a relic associated with non-productive paradigms.

To sum up, I assume that by IOE (10\(^{th}\) - 11\(^{th}\) centuries), but probably even earlier, the as-declension comes to be reinterpreted as primarily word-based, due to the highly transparent nature of inflected forms as well as the fact that the inflection operates with free stems. The same is valid for Neuter u-nouns except their NomAccPl. It is no wonder the suffix -as is extended to them as a Plural marker by the time of the earliest ME texts. I will discuss this in some detail later on.

This leads us to the end of our discussion of OE nominal inflection. I hope to have shown that analyticness is very much present in nominal paradigms. I will now turn my

\(^{228}\) And for -um, but the Dative as a case ceases to exist in eME, and so does the suffix as a result.
attention to the later development of nominal inflection and the rise of a system which is, apart from some details, virtually identical to the MoE situation. I will also consider the consequences of this restructuring in a particular theoretical sense. Specifically, I examine the implications of the restructuring of nominal morphology from the point of view of a central tenet characterising theories of grammaticalisation, viz. the Unidirectionality Hypothesis. I will present an outline of the development of Latin nominal inflection in West Romance, claiming that it undergoes a restructuring which in its very essence is identical to the English events.

5.5. [The restructuring of nominal morphology: the Middle English situation]
The end of the OE period is generally dated around 1100 A.D. The eleventh century, though still belonging to OE according to this dating, is characterised by an important phonological development: the merger of unstressed vowels in what was presumably schwa, generally represented in ME texts as &lt;e&gt;. In IOE manuscripts, as early as the late 10th century, a confusion of the letters &lt;a&gt;, &lt;e&gt;, &lt;o&gt; and &lt;u&gt; is observable to varying degrees. Furthermore, suffixal final -m becomes -n. This merger has far-reaching consequences for inflection, since a number of suffixes, distinguished in OE by their vowels, become homophonous. The situation is particularly disastrous for those inflectional suffixes which contained a vowel only. The resulting homophony is but one aspect of the consequences of reduction: another, equally important aspect is that it leads to morpholexical restructuring in several cases. Let us see the details.

To appreciate the effects of reduction on nominal declension, recall the paradigms in (86). First, look at the as-Plurals, repeated here as (105a); compare this to the post-reduction situation in (105b); recall that unstressed &lt;e&gt; = schwa. I omit the types represented by searu/bearu, since they undergo stem levelling by ME times and do not represent a separate subclass anymore.

---

229 As i never occurred in word-final position in OE, it can be left out of consideration in such cases.
230 This section is based on Mossé 1952:44-53 and Lass 1992:103-12. Note that, for the sake of simplicity, I neglect the fact that certain other sound changes may have affected the stressed vowels.
Classical OE as-Plurals: *stān* ‘stone’, *ende* ‘end’

<table>
<thead>
<tr>
<th>Case</th>
<th>Nom/Acc</th>
<th>Sg</th>
<th>Pl</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sg</td>
<td><em>stān</em></td>
<td><em>ende</em></td>
<td></td>
</tr>
<tr>
<td>Gen</td>
<td><em>stānes</em></td>
<td><em>endes</em></td>
<td></td>
</tr>
<tr>
<td>Dat</td>
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<td><em>ende</em></td>
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<tr>
<td>Sg</td>
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<td></td>
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</tr>
<tr>
<td>Gen</td>
<td><em>stāna</em></td>
<td><em>enda</em></td>
<td></td>
</tr>
<tr>
<td>Dat</td>
<td><em>stāne</em></td>
<td><em>ende</em></td>
<td></td>
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as-Plurals after Reduction (= 11th c.)

<table>
<thead>
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<th>Case</th>
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<th>Sg</th>
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<tr>
<td>Sg</td>
<td><em>stān</em></td>
<td><em>ende</em></td>
<td></td>
</tr>
<tr>
<td>Gen</td>
<td><em>stānes</em></td>
<td><em>endes</em></td>
<td></td>
</tr>
<tr>
<td>Dat</td>
<td><em>stāne</em></td>
<td><em>ende</em></td>
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<tr>
<td>Sg</td>
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<tr>
<td>Gen</td>
<td><em>stānes</em></td>
<td><em>endes</em></td>
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<tr>
<td>Dat</td>
<td><em>stāne</em></td>
<td><em>ende</em></td>
<td></td>
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</tbody>
</table>

The u-Plurals are similar, except, of course, in the NomAccPl, cf. (106a) and (106b):

Classical OE u-Plurals: *sēip* ‘ship’, *word* ‘word’, *wīte* ‘punishment’

<table>
<thead>
<tr>
<th>Case</th>
<th>Nom/Acc</th>
<th>Sg</th>
<th>Pl</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sg</td>
<td><em>sēip</em></td>
<td><em>word</em></td>
<td><em>wīte</em></td>
</tr>
<tr>
<td>Gen</td>
<td><em>sēipes</em></td>
<td><em>wordes</em></td>
<td><em>wītes</em></td>
</tr>
<tr>
<td>Dat</td>
<td><em>sēipe</em></td>
<td><em>worde</em></td>
<td><em>wīte</em></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Case</th>
<th>Nom/Acc</th>
<th>Sg</th>
<th>Pl</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sg</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Gen</td>
<td><em>sēipa</em></td>
<td><em>worda</em></td>
<td><em>wäta</em></td>
</tr>
<tr>
<td>Dat</td>
<td><em>sēipum</em></td>
<td><em>wordum</em></td>
<td><em>wītan</em></td>
</tr>
</tbody>
</table>

u-Plurals after Reduction (= 11th c.)

<table>
<thead>
<tr>
<th>Case</th>
<th>Nom/Acc</th>
<th>Sg</th>
<th>Pl</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sg</td>
<td><em>sēip</em></td>
<td><em>word</em></td>
<td><em>wīte</em></td>
</tr>
<tr>
<td>Gen</td>
<td><em>sēipes</em></td>
<td><em>wordes</em></td>
<td><em>wītes</em></td>
</tr>
<tr>
<td>Dat</td>
<td><em>sēipe</em></td>
<td><em>worde</em></td>
<td><em>wīte</em></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Case</th>
<th>Nom/Acc</th>
<th>Sg</th>
<th>Pl</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sg</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Gen</td>
<td><em>sēipe</em></td>
<td><em>worde</em></td>
<td><em>wīte</em></td>
</tr>
<tr>
<td>Dat</td>
<td><em>sēipen</em></td>
<td><em>worden</em></td>
<td><em>wīten</em></td>
</tr>
</tbody>
</table>

We can sum up the post-Reduction situation with regard to as- and u-Plurals as follows. The C-final stems are moderately affected compared to the V-final (more precisely: eOE e-final, iOE/eME schwa-final) ones. Reduction produces a syncretism of the DatSg and the GenPl (in the case of the *ship*-type, the NomAccPl as well) in -e, and the GenSg merges with the NomAccPl in the as-declension. The *wīte*-type undergoes the same syncretisms as the *sēip*-
type, but there is an additional, more far-reaching consequence which affects the ende-type, too. Notably, the morphological segmentation of these words into base + suffix alters.

Recall the analysis I proposed for ende and wīte. I claimed that it is possible to analyse inflected forms as analytic with suffixes added to the base ende/wīte which ends in a floating -e, doomed to silence if it is properly governed\textsuperscript{231}. Let us now check the post-Reduction situation. As the reader can verify, all forms contain the same vowel, viz. a schwa, in the final syllable. This fact calls for a reinterpretation of morphological divisions. Specifically, it is no longer necessary to assume a floating segment: instead, it is possible to assign the following structures to forms of ende and wīte:

\begin{equation}
\begin{array}{c|c|c}
\text{Sg} & \text{NomAcc} & \text{ende} \\
\text{Gen} & \text{ende-s} & \text{wīte-s} \\
\text{Dat} & \text{ende} & \text{wīte} \\
\text{Pl} & \text{NomAcc} & \text{ende-s} \\
\text{Gen} & \text{ende} & \text{wīte} \\
\text{Dat} & \text{ende-n} & \text{wīte-n} \\
\end{array}
\end{equation}

Note that Reduction has an important consequence for Neuters of the wīte type: the NomAccPl becomes identical with the NomAccSg. If one accepts the reinterpretation offered in (107), wīte loses its idiosyncratic (synthetic) NomAccPl: it joins the word type, where NomAccSg = NomAccPl = the free base.

An important result of this reinterpretation for all as- and u-nouns is that it introduces suffixal allomorphy: all suffixes under this interpretation will have two allomorphs, one consisting of a single -C and another one of the shape -eC. Compare, for instance, the NomAccPl of stān and ende: they will be assigned the structure stān-es and ende-s, respectively. The same goes for the GenSg and the DatPl.

As for the DatSg and the GenPl, this analysis claims that they lack an overt case-marking suffix in the ende/wīte type. Yet, the gradual and accelerating loss of these declensional cases, as well as the DatPl, is clearly observed already in eME. As Mossé 1952:49 points out, consonant-final stems without a DatSg -e appear as early as the earliest ME text, The Peterborough Chronicle (dating from the second half of the 12\textsuperscript{th} century), witness (in) hūs ‘(in) house’ for OE hūse. Indeed, this lends strong support for the claim that the DatSg was soon identified with the bare base form. As for the Plural, both the Genitive and the Dative are lost so early that Mossé 1952:47 posits but one common form for the Plural

\textsuperscript{231}Except, of course, for the NomAccPl of wīte, viz. wītu, which I analysed as synthetic.
at the start of ME: the original NomAccSg form, i.e., *endes*. Jones 1972:108 arrives at the same conclusion, citing examples from The Peterborough Chronicle. All in all, the assumption that the restructuring presented in (107) took place in IOE is not unfounded. We’ll soon come back to the overall reorganisation of nominal declension, but let us first see the other declensional classes, starting with a-Plurals in (108):

(108) (a) Classical OE a-Plurals: *lufu* ‘love’, *lār* ‘learning’

<table>
<thead>
<tr>
<th></th>
<th>Sg</th>
<th>Nom</th>
<th>lufu</th>
<th>lār</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Acc</td>
<td>lufe</td>
<td>lāre</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Gen</td>
<td>lufe</td>
<td>lāre</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Dat</td>
<td>lufe</td>
<td>lāre</td>
<td></td>
</tr>
<tr>
<td>Pl</td>
<td>Nom/Acc</td>
<td>lufa</td>
<td>lāra</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Gen</td>
<td>lufa/lufena</td>
<td>lāra/lārena</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Dat</td>
<td>lufum</td>
<td>lārum</td>
<td></td>
</tr>
</tbody>
</table>

(b) a-Plurals after Reduction (= 11th c.)

<table>
<thead>
<tr>
<th></th>
<th>Sg</th>
<th>Nom</th>
<th>lufe</th>
<th>lār</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Acc</td>
<td>lufe</td>
<td>lāre</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Gen</td>
<td>lufe</td>
<td>lāre</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Dat</td>
<td>lufe</td>
<td>lāre</td>
<td></td>
</tr>
<tr>
<td>Pl</td>
<td>Nom/Acc</td>
<td>lufe</td>
<td>lāre</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Gen</td>
<td>lufe/lufene</td>
<td>lāre/lārene</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Dat</td>
<td>lufen</td>
<td>lāren</td>
<td></td>
</tr>
</tbody>
</table>

This declensional class — which already shows a great amount of syncretism in OE — suffers a further reduction in the number of distinct forms, the most significant merger being that of the (now identical) NomAccSg form with the NomAccPl in the *lufu*-type. Note at the same time that this type gains a free base, too, found uncombined with a suffix throughout the Singular and in the NomAccPl: given that the Singular forms are all identical, it is plausible to assume that here, too, the form *lufe* became identified as a free base. This paradigm, as I noted earlier, does not survive the OE period: more on this shortly. Consider now the n-Plurals:
Classical OE n-Plurals: *nama* ‘name’ (M), *sunne* ‘sun’ (F)

<table>
<thead>
<tr>
<th></th>
<th>Sg</th>
<th>Nom</th>
<th><em>nama</em></th>
<th><em>sunne</em></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Acc</td>
<td><em>naman</em></td>
<td><em>sunnan</em></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Gen</td>
<td><em>naman</em></td>
<td><em>sunnan</em></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Dat</td>
<td><em>naman</em></td>
<td><em>sunnan</em></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Pl</td>
<td><em>naman</em></td>
<td><em>sunnan</em></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Nom/Acc</td>
<td><em>naman</em></td>
<td><em>sunnan</em></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Gen</td>
<td><em>namena</em></td>
<td><em>sunnena</em></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Dat</td>
<td><em>namum</em></td>
<td><em>sunnum</em></td>
<td></td>
</tr>
</tbody>
</table>

n-Plurals after Reduction (= 11th c.)

<table>
<thead>
<tr>
<th></th>
<th>Sg</th>
<th>Nom</th>
<th><em>name</em></th>
<th><em>sunne</em></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Acc</td>
<td><em>namen</em></td>
<td><em>sunnen</em></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Gen</td>
<td><em>namen</em></td>
<td><em>sunnen</em></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Dat</td>
<td><em>namen</em></td>
<td><em>sunnen</em></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Pl</td>
<td><em>namen</em></td>
<td><em>sunnen</em></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Nom/Acc</td>
<td><em>namen</em></td>
<td><em>sunnen</em></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Gen</td>
<td><em>namene</em></td>
<td><em>sunnene</em></td>
<td></td>
</tr>
<tr>
<td></td>
<td>Dat</td>
<td><em>namen</em></td>
<td><em>sunnen</em></td>
<td></td>
</tr>
</tbody>
</table>

The amount of syncretism displayed by this class hardly needs special comment. Note that the DatPl merges with the NomAccPl, and that the Masculine and Feminine paradigms become identical. At the same time, the NomSg can readily be reinterpreted as a free base. An important property of this class is that the NomSg and the NomAccPl are clearly distinguished, which is the obvious reason for why the n-Plurals enjoy some success in some (Southern) dialects in eME.

In this section, I give a brief overview of how the overall nominal declensional system was restructured. I will attempt to show that the system is in its essence of the same kind as that of MOE.

As I mentioned already, the case system of OE nouns undergoes simplification. Notably, the DativeSg is practically lost as an obligatory category by eME: it survives as an option for metrical purposes mainly, cf. Lass 1992:110, who gives examples such as *o lifffe* ‘in the air’ besides *o pe lifft* ‘id.’. He points out, furthermore, that it is no longer appropriate to call this case “Dative” since it is used as a post-prepositional marker. In the Plural, all case

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232 As the reader can see, the difference between Masculines and Feminines in (d) is minimal: they diverge only in the NomSg. Neuter nouns which belong here are *ēage* ‘eye’ and *ēare* ‘ear’; they go like Feminines except that their AccSg is identical to the NomSg.
distinctions are lost by the 12\textsuperscript{th} century. What remains, therefore, is a Non-Oblique case deriving from OE base forms, as well as a Genitive Singular in -(e)s from the OE -es GenSing marker of the as- and the u-declension. Note, however, that the loss of case distinctions is deeply rooted in OE: most nouns do not distinguish the Nominative and the Accusative, and the reduction of unstressed vowels further decreases the number of nouns which did show this distinction in classical OE. As for the Dative, it was frequently reinforced by the preposition tō ‘to’, even in instances where the Dative was unambiguously marked, including pronouns (e.g. (hē) cwæþ him tō ‘he said to him’, where him can only be a Dative; the use of tō after pronouns is usual in OE).

The most spectacular loss occurring by ME is the total elimination of grammatical gender distinctions; nonetheless, I am not going to discuss it since it is not relevant for us; see, for example, Lass 1992:105ff for an overview.

The early Middle English situation is summarised by Mossé 1952:47 in a tabular form, a slightly modified form of which is given in (110) below; I omit the Dative (which he does include) for reasons detailed above. I follow Mossé’s division of forms into stem + suffix.

<table>
<thead>
<tr>
<th></th>
<th>Sg</th>
<th>NomAcc</th>
<th>II</th>
<th>III</th>
<th>IV</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td><img src="image" alt="stōn" /></td>
<td><img src="image" alt="trē" /></td>
<td><img src="image" alt="soul-e" /></td>
</tr>
<tr>
<td>Gen</td>
<td><img src="image" alt="stōn-es" /></td>
<td><img src="image" alt="trē-s" /></td>
<td><img src="image" alt="soul-es" /></td>
<td><img src="image" alt="nām-e" /></td>
<td></td>
</tr>
<tr>
<td>Pl</td>
<td><img src="image" alt="stōn-es" /></td>
<td><img src="image" alt="trē-s" /></td>
<td><img src="image" alt="soul-es" /></td>
<td><img src="image" alt="nām-en" /></td>
<td></td>
</tr>
<tr>
<td>GLOSS</td>
<td>‘stone’</td>
<td>‘tree’</td>
<td>‘soul’</td>
<td>‘name’</td>
<td></td>
</tr>
</tbody>
</table>

The nouns in the table represent three different OE genders: ‘stone’ and ‘name’ are Masculine, ‘tree’ is Neuter, and ‘soul’ is Feminine in OE. This is not reflected in the inflected forms anymore. Type IV is only found in the dialects South of the Thames and even there, they are replaced by Type III gradually; in the dialects of the Midlands and the North it does not exist except in some forms. As standard MoE descends from a Midland dialect, I will neglect Type IV here altogether, since nāme &Co. do not differ from Type III in these dialects; the rest of the discussion is about Types I-III.

\[233\] See Appendix II for the pronunciation of ME data, cited here according to the usual historical linguistic practice.
The following generalisations can be made. First, the GenSg is identical to the Plural: we have reached the MoE situation. Second, note that Mossé treats Type III as having a bound stem, to which endings identical to those in Type I are added. I argued earlier, however, that this treatment is not necessary — indeed, it is highly suspect: why should we assume that *soule* is a suffixed form, if the -e is always present? Furthermore, this treatment requires one to posit a bound stem, which is not found elsewhere. It is more reasonable to think that the learner, when faced with these data, would assume suffixal allomorphy rather than posit bound stems, especially because the suffixal allomorphy -s ~ -es must be recognised anyway due to the existence of Type II. I propose, therefore, a simple two-way distinction, based on K&O:61:

(111) Declensional types in eME: 2nd version

<table>
<thead>
<tr>
<th></th>
<th>I</th>
<th>II</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sg</td>
<td><em>stōn</em></td>
<td></td>
</tr>
<tr>
<td>Gen</td>
<td><em>stōn-es</em></td>
<td></td>
</tr>
<tr>
<td>Pl</td>
<td><em>stōn-es</em></td>
<td></td>
</tr>
</tbody>
</table>

Note that I included two nouns in Type II because they differ in that *trē* ends in a stressed vowel while *soule* ends in an unstressed one (schwa), but the endings they take are identical.

Let us now try to represent these forms in a CVCV framework. A form like ‘tree’ seems to present no problem: a suffixal -s is added to the base in what can without any particular difficulty be analysed as an analytic concatenation, cf. (112a):

(112) (a)

Yet, a synthetic concatenation would produce the same result:
Note that the second V position of the long vowel requires licence to receive spreading melody. In (112a), it is given to it by being domain-final (via EG); in (112b), it is licensed by the E-Governed FEN. The same representation goes for all vowel-final items, cf. (113), representing *luve* ‘love’ — here, no formal difference can be effected since *luve* ends in a full V, i.e., it is not expected to show any phonological speciality word-finally:

(113)

Let us now turn to the ‘stones’ type; a possible analytic representation is given in (114a), and a synthetic one is shown in (114b):

(114)
This is the same situation as in the case of OE stānas (or GenSg stānes; both sound the same in ME) ancestral to the ME form (cf. (92a,b)). Yet, the ME situation is different, because we must posit two lexically distinct allomorphs of the Plural/Genitive morpheme, /s/ and /ôs/, in order for the analysis to work. The choice of allomorphs is phonologically conditioned; yet, the difference in their phonetic form cannot be derived phonologically. If we assume a single lexical form /s/, we must account for the appearance of the schwa, which, of course, cannot appear out of nowhere. We might, of course, say that the schwa is stem-final, but that would be even worse: for all C-final nouns, a schwa-final allomorph should be assigned to serve as a bound stem to which the Plural/Genitive /s/ is added. This implies, however, that the Plural (and the homophonous Genitive) is synthetic in the case of C-final nouns; yet, if the schwa is final in the stem, it contradicts the proposal I put forward in Chapter 4, viz. that stems are universally C-final in synthetic forms. It would also fare quite badly in the sense that this allomorphy would have to be posited for a huge number of lexical items, whereas a solution according to which it is the suffix which displays allomorphy only involves two suffixal morphemes: the Genitive and the Plural.

Suppose we claim that the lexical form of the Plural/Gen allomorphs is /ôs/, from which both surface alternants are derived. In this case, however, the problem is to account for why the schwa disappears after V-final stems, especially after ones such as ‘tree’ which ends in a long vowel. A sequence of a long Nucleus + schwa is perfectly well-formed in ME, cf. crīen 'kriían ‘to cry’. Schwa-final nouns are less problematic: a concatenation such as luve-en 'luvô-an would yield a sequence of two schwas, which can be rendered illegal by the Obligatory Contour Principle, resulting in the deletion of one of them; as schwa can never be long in ME, the remaining schwa could not spread into the vacated Nucleus. Yet, even so, the fundamental problem remains: we must still posit /s/ for trê &Co.

A possible way out is to make use of floating segments, which we did employ in our analysis of OE. Specifically, one could suppose that the suffix begins with a floating schwa melodically, but with the C position associated with the s on the skeletal level. This option is represented in (115) below:
This option is difficult to accommodate in our theory: as the question mark in (115b) indicates, the encircled V position is the problematic point. Notably, it is lexically unassociated with any melody, and there are two melodic expressions aiming at attaching to it, as shown in the diagram. What decides which of them will find its way there? The only way I can see is to stipulate that the e has precedence, but, alas, that would be none other than forcing extrinsic ordering on the phonetic interpretation. This is strictly forbidden in GP. Alternatively, we could propose that EG “closes” the gates from any process coming from the right, an assumption that does not sound unfounded. In that case, however, (115a) is illicit.

To sum up, it appears that all we can do is to assume posit two lexically given (= phonologically unrelatable) allomorphs for the Plural suffix (and the homophonous Genitive), notably /s/ and /as/. If, however, this is the case, we must make the inevitable step and conclude that Plural/Genitive forms are synthetic\textsuperscript{234}. This conclusion is not particularly attractive: the Plural suffix had been spreading since OE times, becoming the only productive Plural marker by eME. The same is true for the Genitive. Furthermore, they both attach to free word forms rather than stems. In Pinker’s terms, they have become default rules, which can be formulated as follows:

\textsuperscript{234} At least in the model I am using here. In a Classical GP model combined with Harris’ and Harris & Lindsey’s view that the schwa is the manifestation of the identity element, the problem is not impossible to tackle.
The ME Plural Rule

[[X]N-(e)s] ‘Plural’

The ME Genitive Rule

[[X]N-(e)s] ‘Genitive’

It would be definitely better to be able to say that these suffixes are analytic. In fact, I will now argue that the situation is by far not as disheartening as it may seem. Note that we posited (being forced by the theory) two, lexically distinct allomorphs for both suffixes: /s/ and /æs/. I would like to propose that one of them, viz. /s/, is indeed analytic and it is the “default” case, the regular, “blind” variant in Pinker’s sense. I suggest a revision of (116), shown below as (117):

(117) The ME Plural/Genitive Rules: Final version

(a) The ME Plural Rule

[[X]N-s] ‘Plural’

(b) The ME Genitive Rule

[[X]N-s] ‘Genitive’

That is, the default Plural rule attaches /s/ to nouns. This means that all C-final nouns, such as stōn, concatenate synthetically with /æs/. Let us see the arguments.

Argument # 1: Syncope. Remember that in OE, Syncope was by and large optional. In eME, however, it becomes much more extensive, cf. the ME Sg ~ Pl/Gen pairs fader — fadres ‘father’, heven — hevnes ‘heaven’, engel — engles ‘angel’, sadel — sadles ‘saddle’, etc. Nouns which are liable to Syncope due to suffixation invariably end in a consonant; I predict, therefore, that they will behave synthetically. This prediction is borne out by the generality of Syncope in eME. Indeed, full forms such as hevenes are rare, and, as Wright & Wright 1928:140 point out, they may well be but written forms not reflecting actual pronunciation.

Argument # 2: Anglo-Norman Plurals. The adoption of AN loans lends strong support for the claim that /s/ was taken to be the default Plural/Genitive marker. K&O:61-71 offer a detailed discussion of the evolution of the English Plural rule, in which they devote considerable attention to the Plural formation of AN loans in ME, based on early13th century data from the West Midlands dialect known as AB. K&O point out that C-final French loans regularly take a -s in the Plural, and variably -s or -es in the Genitive. (The only exception is when the

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235 Mossé 1952:36, Wright & Wright 1928:139.
French word ends in \( \text{\textit{ts}} \) or \( s \): in the former case, \(-es\) is used, in the latter, the Plural is not marked. The latter practice is still observed in Chaucer\(^{236}\).) Examples include Plural/Genitive forms such as \textit{prelats, liuns, parlurs, lesceuns, confessurs, martyrs} ‘prelate, lion, parlour, lesson, confessor, martyr’. K&O explain this with reference to French influence, pointing out that \(-s\) is the regular Plural suffix in Old French: the pattern is, therefore, an imitation of French usage. K&O assume that the underlying form of the English Plural marker was \( /es/ \), which dropped the e after vowel-final stems but surfaced unchanged when added to C-final ones (cf. \textit{luve-s} vs. \textit{st\( \textit{\textit{n}}\)-es}); hence, they cannot explain the pluralisation of French loans unless they claim it to be a French pattern. Recall that the Genitive marker appears variably as \(-s\) or \(-es\), so that in a Genitive function, one finds both \textit{parlurs} and \textit{parlures}, for example. Here, the vowelless form can hardly be explained with French influence, since there is no Genitive in OF. K&O assume a reanalysis of the GenSg suffix as \( /s/ \) based on French nouns ending in a schwa in the Nominative, e.g., \textit{silence}, Gen \textit{silences}. For this, however, they are forced to make a stipulation: notably, that forms such as \textit{parlures} are earlier than \textit{parlurs}. Yet, both forms are found in the same dialect. Furthermore, they fail to explain why the same reanalysis does not happen in the Plural: schwa-final French loans ended in \(-es\) \textit{as} in the Plural, too, not only in the Genitive.

Now, I do not wish to claim that French loans could not be adopted together with their Plural form. They certainly could, as testified by Plurals such as \textit{caas} ‘cases’\(^{237}\). Yet, what I find unconvincing in K&O’s proposal is that one would expect to find at least some instances of nativisation. Specifically, if the domestic regular Plural ending is \( /es/ \), how is it possible that no attempt was made to extend it to loans? This is clearly not in accordance with empirical observations about loanword adaptation. I believe that the French Plural rule could be so easily accommodated because \( /s/ \) was already available as a default rule for Plural formation in English, as formulated in (117a). As we have seen, the Plural allomorph \( /s/ \) showed (in native words) a uniform analytic behaviour; \( /\text{\textit{\textit{a}}}s/ \) did not. The analysis predicts that \( /s/ \) will be a more likely candidate for being the default rule. In other words, the adoption of French words in their original Plural form was made possible by this fact; and, conversely, the French loan Plurals probably reinforced the already existing situation. The use of \( /s/ \) in C-final AN loans is but the application of the default rule to newcomers.

\(^{236}\) E.g., \textit{caas} ‘case’ can be both Sg or Pl, see Obst & Schleburg 1999:121.

\(^{237}\) Nouns in \(-s\) had zero Plural in OF.
Note that French loans in the Genitive may also appear with an /œs/. This may be used as an argument against my interpretation. Nonetheless, as we saw, /s/ is also found and it was, in fact, becoming more widespread. The fact that Genitives could be formed with /œs/ can indeed be attributed to the fact that here, French patterns did not reinforce the default pattern, as there was no Genitive in OF. Therefore, the existing native surface pattern (which is invariably -C#œs) could more easily compete with the symbolic rule. This is not surprising: existing forms, even though possibly not generated by a symbolic rule, can certainly have an effect on new ones, especially if they are numerous. I suppose that the analogical influence of existing (native) C-final words, which invariably took /œs/, competed with the symbolic rule until the latter gained control. In the Plural, the existence of the OF pattern, which happened to yield identical outputs to the default symbolic rule in the majority of cases, provided the necessary support for the symbolic rule to be the sure winner against the analogical influence of existing but non-default formations.

Argument # 3: “Inorganic e”. From an early date in the ME period, one can observe the appearance of schwa in word-final position in a number of nouns which historically ended in a consonant, e.g., tīde ‘time’, brīde ‘bride’, cōle ‘coal’, going back to OE tīd, brīd, col, respectively. This etymologically unjustified schwa is usually called “Inorganic e” in the literature. The traditional explanation for its appearance is with reference to inflected forms, where there was a schwa, i.e., tīdes, brīdes, cōles, etc. The schwa was, then, analogically extended to the base forms. The source of analogy is clearly the set of words which end in a schwa in their base form, e.g., nāme — nāmes. This is, then, proportional analogy in the Neogrammarian sense: nāme : nāmes = X : tīdes. The problem with this explanation is the usual one with all analogical explanations: it does not really explain why it happened and neither does it give an answer to an even more important question: why didn’t it happen the other way round? Specifically, why did nāme not change to **nām? Yet, such instances are not attested. The proposal made here gives a straightforward answer: the appearance of “Inorganic e” is but a way of regularising the items in question. It is a case of lexical restructuring which results in that the words in question conform to the default rule, so tīde — tīdes is as fine with regard to the default rule as luve — luves or trē — três.

238 Cf. Wright & Wright 1928:52.
239 Schwa-final words do lose their schwa, so that nāme does become nām, but this is much later and affects all word-final schwas.
The default Plural/Genitive rule of ME gains ground relatively fast. Its extension to C-final words is observable in *Amys and Amiloun*, dating from about 1300. The poetic meter clearly shows that forms such as *bësts*, *wôrds*, *gods* existed alongside the older *bëstes*, *wôrdes*, *godes*. By Chaucer’s time, adding *-s* in the Plural/Genitive is probably a widespread option in spoken usage — at least it is in poetry, and it is safe to assume that poetry would hardly have used an innovative device which was impossible in natural speech. Yet, it takes considerable time until the Plural/Genitive formations stabilise in their modern form.

Polysyllabic nouns exhibiting Syncope in eME follow suit, too. By Chaucer’s time, forms like *hevens*, *lovers*, etc. are widespread, and by MoE, syncopated forms (*hevnes*) disappear altogether. The shift to analyticness, therefore, takes several centuries; but this is not surprising, since language is inherently conservative, and old forms die hard.

In fact, there are still nouns in MoE which preserve the ME *-es* suffix: those ending in a sibilant. It is interesting to note that in the model I adopt, they must be considered synthetic — for the same reason why the Plural/Genitive of all C-final nouns in eME. The fact that this suffix allomorph (Modern RP *iz*) attaches productively to new sibilant-final words seems to contradict this conclusion. It is nevertheless not unparallelled that fully regular suffixes may behave synthetically in relation to a phonologically defined class of words. In Hungarian, for instance, the Inessive suffix *-ban/-ben* (the choice between the two variants depends on vowel harmony) is fully analytic except when the noun ends in *-a* or *-e*: these stem-final vowels undergo lengthening, cf. *pajta* ‘barn’ — *pajtában* ‘id.-Inessive’, *pince* ‘cellar’ — *pincében* ‘id.-Inessive’, etc.; and the lengthening happens with foreign items (including proper names), too, cf. *Atlanta* — *Atlantában*, etc.

As far as the status of the Plural suffix with regard to fusion is concerned, it is undoubtedly not fusional any more in ME. The reason for this is simple: case distinctions were eliminated in the Plural by the ME period. As a result, the functional load of the suffix decreased until it came to signify ‘Plural’ only. As (regular) nouns do not display radical fusion in the Plural, concatenations of this kind are agglutinating. The same goes for the Genitive: the loss of case distinctions in the Plural caused it to become the exponent of the category ‘Possessive’. Both suffixes, then, have become both analytic and agglutinating, though they derive from suffixes which were originally fusional and to a large extent synthetic, too.

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240 Lass 1992:81 says that this stabilisation is datable to around 16th century the earliest.
The story has not ended, though, because the Genitive suffix underwent a further change: it has become a clitic which attaches to NP’s rather than nouns, witness examples such as *the king of England’s crown, the girl we met’s knickers*, etc. The detailed analysis of this shift from affix to clitic is beyond the scope of the present thesis. Suffice it to say that examples for the cliticisation of the Genitive marker are observable as early as in Chaucer’s texts, cf. [*the god of sleep*es heyr ‘the god of sleep’s heir’ (c. 1368)]. Interestingly, the same shift has occurred in some other Germanic languages as well, such as Afrikaans and Danish. The reader is referred to Carstairs 1987 or Norde 1998 for details.

5.6. [A consequence]

The term *grammaticalisation* was first used by Meillet 1912 (see Hopper & Traugott 2003:21), and it has been in use ever since. It is usually defined as the “development of grammatical morphemes out of earlier lexical formatives” (Hopper & Traugott, ibid.). Examples are abundant: for example, the MoE Future auxiliary *will* derives from OE *willan* ‘to want’, a full lexical verb; the French adverb-forming suffix *-ment* goes back to the Ablative case of the Latin noun *mēns* ‘mind’, viz. *mente*, etc.

It has been observed that lexical items first become non-content (grammatical) words such as Auxiliaries, Adpositions, etc., while preserving their syntactic autonomy. At a later stage, they may gradually lose their independence, becoming first clitics, then agglutinating affixes, and thereafter, fusional affixes. Finally, they may disappear altogether, sometimes leaving but a residue behind themselves in the form of radical fusion manifested in the original host. Altogether, the following “grammaticalisation cline” is posited:

(118)  *The Grammaticalisation Cline*

\[
\text{content item} > \text{grammatical word} > \text{clitic} > \text{agglutinating affix} > \text{fusional affix} > \text{zero}
\]

The general observation that content items tend to go in this direction, i.e., from left to right rather than from right to left, has led to the proposal that there is a universal linguistic principle which “forces” formatives to behave that way. This assumption is known as the *Unidirectionality Hypothesis* (*UH*). Hopper & Traugott (2003:Ch5) offer a detailed presentation; they admit that UH is a strong hypothesis indeed, but conclude that “the counterexamples to unidirectionality that have been adduced so far are sporadic, whereas the evidence for unidirectionality is systematic and cross-linguistically replicated” (2003:99).
One of the counterexamples they mention is the rise of the English Possessive clitic ‘s from the ME affix -s/-es. This development has received some attention in the literature. Carstairs 1987:156 mentions an interpretation by Janda 1980, according to whom the clitic does not derive directly from the ME suffix but results from a construction known as the his-Genitive, widespread in eMoE, e.g., the king of England his crown. The suffix, Janda argues, was easily reinterpretable as the unstressed pronunciation of his, i.e., iz. Carstairs argues, to my mind convincingly, that evidence from English as well as parallel constructions in Dutch and Afrikaans suggests that Janda’s interpretation is not plausible. He concludes that the modern Possessive clitic does indeed derive from the ME suffix. Interestingly, Hopper & Traugott do not present any arguments for or against the view that the development of this MoE clitic is a counterexample to UH. In sum, I take it to be a real piece of counterevidence.

There is, however, something even more striking about discussions of the MoE clitic: the fact that it derives from an affix has received a fair amount of attention, but the fact that the affix itself underwent a path of development which contradicts UH has passed unnoticed. Notably, the affix itself is agglutinating, but it derives from an OE fusional affix. That is, it went along the path fusional affix > agglutinating affix > clitic. It goes without saying that the Plural suffix shares the first stage with the Possessive, but it stops there, i.e., it goes along the cline fusional affix > agglutinating affix. Furthermore, not only do both suffixes gain morphological independence, but phonologically, too, they become analytic from an earlier synthetic stage. The history of English nominal inflections seems to offer more evidence against UH than it has been assumed so far.

I now offer a brief analysis of the origins of Plural marking in the West Romance languages. The discussion is based on Elcock 1960:39-68 and Penny 2002:57-117. The declensional system of Classical Latin (CL) distinguished two numbers, six cases, five declensional classes and three grammatical genders. Of the five declensional classes, two (the 4th and the 5th of traditional grammars) were recessive and do not survive at all into the Romance languages. Furthermore, consonantal stems in the 3rd declension often have nominative forms that are highly different from the rest, e.g., mīles ‘soldier-NomSg’ vs. mīlitem ‘id.-AccSg’. There is an increasing tendency in spoken Latin from the earliest times to replace these nominatives with regularised forms such as *mīlitis, according to the pattern provided by 3rd declension nouns like cīvis ‘citizen’, AccSg cīvem, which had no stem allomorphy. I give a table of the productive declensional types in (119); I omit the Vocative since it does not survive:
The affixes are clearly fusional, and, since all nouns have a bound stem, the inflection is synthetic as well. However, the system outlined above undergoes a wholesale restructuring in Vulgar Latin (VL): case distinctions are reduced, and the Neuter ceases to exist, most of its members joining the Masculines. Furthermore, a number of phonological changes occur, too. By Late Latin, the spoken language (= VL) possesses a simplified system (data given in IPA):

\[(120) \text{VL paradigms}\]

<table>
<thead>
<tr>
<th></th>
<th>1st (F/M)</th>
<th>2nd (M)</th>
<th>3rd (M/F)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Sg</strong></td>
<td><strong>NOM</strong></td>
<td><strong>Masc</strong></td>
<td><strong>Neut</strong></td>
</tr>
<tr>
<td><strong>Fem</strong></td>
<td><strong>Masc</strong></td>
<td><strong>Neut</strong></td>
<td><strong>(M/F/N)</strong></td>
</tr>
<tr>
<td><strong>SG NOM</strong></td>
<td>capra</td>
<td>mūrus</td>
<td>vīnum [ū]</td>
</tr>
<tr>
<td><strong>ACC</strong></td>
<td>capram [ā]</td>
<td>mūrum [ū]</td>
<td>vīnum [ū]</td>
</tr>
<tr>
<td><strong>GEN</strong></td>
<td>caprae [āi]</td>
<td>mūrī</td>
<td>vīni</td>
</tr>
<tr>
<td><strong>DAT</strong></td>
<td>caprae [āi]</td>
<td>mūrō</td>
<td>vīnō</td>
</tr>
<tr>
<td><strong>ABL</strong></td>
<td>caprā</td>
<td>mūrō</td>
<td>vīnō</td>
</tr>
<tr>
<td><strong>PL NOM</strong></td>
<td>caprae [āi]</td>
<td>mūrī</td>
<td>vīna</td>
</tr>
<tr>
<td><strong>ACC</strong></td>
<td>caprās</td>
<td>mūrōs</td>
<td>vīna</td>
</tr>
<tr>
<td><strong>GEN</strong></td>
<td>caprārum [ū]</td>
<td>mūrōrum [ū]</td>
<td>vīnōrum [ū]</td>
</tr>
<tr>
<td><strong>DAT</strong></td>
<td>caprēs</td>
<td>mūřēs</td>
<td>vīnēs</td>
</tr>
<tr>
<td><strong>ABL</strong></td>
<td>caprēs</td>
<td>mūřēs</td>
<td>vīnēs</td>
</tr>
<tr>
<td><strong>Gloss</strong></td>
<td>‘goat’</td>
<td>‘wall’</td>
<td>‘wine’</td>
</tr>
</tbody>
</table>

The pronunciation of italicised parts is indicated for the sake of clarity.
Note that in the 1<sup>st</sup> and 3<sup>rd</sup> declensions, the AccPl is iconically marked relative to the AccSg. The decisive step on the way to the general West Romance system is what can be called the High-mid Merger, characterising all of Central and Western Romance except Sardinian: i and o merge with e and o, respectively, yielding e and o. The consequences are remarkable:

(121) *Proto-West-Romance nominal paradigms*

<table>
<thead>
<tr>
<th></th>
<th>1&lt;sup&gt;st&lt;/sup&gt; (F (M))</th>
<th>2&lt;sup&gt;nd&lt;/sup&gt; (M)</th>
<th>3&lt;sup&gt;rd&lt;/sup&gt; (M/F)</th>
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<td><strong>SG</strong></td>
<td>kapra</td>
<td>muros</td>
<td>kanes</td>
</tr>
<tr>
<td><strong>ACC</strong></td>
<td>kapra</td>
<td>muro</td>
<td>kane</td>
</tr>
<tr>
<td><strong>PL</strong></td>
<td>kapre</td>
<td>muri</td>
<td>kanes</td>
</tr>
<tr>
<td><strong>ACC</strong></td>
<td>kapras</td>
<td>muros</td>
<td>kanes</td>
</tr>
</tbody>
</table>

**Gloss** ‘goat’ ‘wall’ ‘dog’

The Accusative (italicised in the table) clearly becomes the transparent and iconic case: in both numbers, AccPl = (AccSg + -s). In relation to this, the NomSg, when marked, is identical to the AccPl. It is hardly surprising that the Romance languages adopt the Accusative Singular as a common Singular form, ousting the Nominative<sup>242</sup>. This change is complete by the time of the earliest attestation of Italian and Ibero-Romance; the two-case system survives longer in Gallo-Romance (OF still has it). In the Plural, the Accusative, being wholly iconic in relation to the common Singular form, is generalised in the West (the Nominative endings -i and -e are generalised in Italian and Romanian, but this is due to an independent reason: a ban on word-final consonants).

To sum up, the input to West Romance (non-Gallian) systems is as follows:

(122) **Sing** kapra muro kane

**Pl** kapras muros kanes

<sup>242</sup> There are other reasons, too. Already in CL, the Accusative is used in a wider range of syntactic contexts than the other structural case, viz. the Nominative. Klausenburger 2000 offers a detailed discussion of the origins of Romance case systems.
This system survives almost intact into present-day Spanish. The most important points are as follows:

1. The system is clearly agglutinating. A unitary Plural marker \(-s\) is added to all nouns; there is no fusion of either kind.

2. The concatenations are analytic: the Plural suffix is added to the free Singular form, causing no alternations, including stress (which remains on the same syllable where it is in the base).

As a result of the restructuring, the evolution of Romance Plurals goes along the same path as in English. It must be emphasised that the Romance and English developments are independent of each other, and both seem to present clear violations of the Unidirectionality Hypothesis.

I do not think, however, that UH should be entirely given up, for the following reasons. Note that the term grammaticalisation is in fact used as a cover term for two different processes:

a) A lexical word loses its lexical content and assumes a grammatical function.

b) A word (formative) with a purely grammatical function fuses with a main category.

It is not at all obvious, in my view, that the two processes must inevitably be treated as stages along the path of a single process. Notably, a) is grammaticalisation proper, which, however, does not imply b). English auxiliaries, for example, show no tendency at all to land on the main verb as clitics or affixes; b), on the other hand, states the process whereby a function word loses its syntactic independence: b) is a path of synthesis. I find it difficult to accept why a clitic or an affix is more “grammatical” than an auxiliary, for example. English will is just as “grammatical” as the preterite -\(ed\): both realise Inflection.

It appears to me that the treatment of a) and b) as a unitary process derives from the earliest formulations of grammaticalisation. Meillet, for example, defines a grammaticalisation path as “lexical item > morphology” (see Hopper & Traugott 2003:99), omitting syntactic stages. That is, a lexical item becomes an inflectional item. At that time, inflection was understood as morphology. Yet, nowadays probably no one would take that view: inflection is generally regarded as being realised syntactically or morphologically, but both options are functions of the grammar.

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243 The only important difference is that final -\(e\) > zero after a single coronal consonant in Spanish, so, for example Proto-West-Romance \(\ast le\text{\'one} \text{‘lion’} > \text{Sp le\text{\'on}}\) (but Plural le\(\text{\'ones}\)); the acute accent indicates stress. The item cane does not survive in Sp, being replaced by perro.
To sum up, if one separates a) from b), the hypothesis that grammaticalisation in the sense of a) is unidirectional can probably maintained: all serious objections against UH come from the domain of b). In other words, the path from a grammatical word to an affix may not be unidirectional, but this does not necessarily imply that grammatical items can happily become lexical ones.
The restructuring of verbal inflection

This chapter discusses the origins and the formation of ME verbal inflection. As with nouns, I will be interested in productive inflections. As a result, strong verbs, which represent a closed class already in OE (indeed, in PGmc) and are clearly synthetically affixed, are not given detailed treatment; I only mention them when it is necessary for some reason. The present tense forms of verbs will also be discussed in a limited way, concentrating on Class II weak verbs. Detailed discussion will be devoted to the Preterite and Past Participle of both Class I and Class II weak verbs, since this aspect is highly relevant for later developments. I will conclude that analytic structures are clearly present in the verbal conjugation of OE. In the second half of this chapter, I examine the change known as Pre-Cluster Shortening, providing arguments that it is better analysed as a consequence of morphological restructuring rather than a phonological change. Some implications of this proposal for cognateness will be considered.

6.1. [An outline of OE weak verbs]

The traditional classification of verbs in OE grammars distinguishes two main categories: strong and weak verbs, the former divided into seven classes, the latter into three\(^{244}\). Apart from these major categories, OE grammars distinguish two minor ones, preterite-present and irregular verbs. Yet, the following points must be pointed out\(^ {245}\):

1. The Preterite-Presents and the Irregulars are only distinguishable from a historical perspective: synchronically speaking, both are irregular.

2. The same is valid for some weak verbs, viz. four which are traditionally classified as W3 (they include habban ‘have’, seeğan ‘say’, libban ‘live’ and hyêğan ‘think’), as well as several members assigned conventionally to W1, such as brenğan ‘bring’, tâčan ‘teach’, tellan ‘tell’, sêčan ‘seek’, and some others. These verbs are all highly irregular.

3. Verbs belonging to S7 (such as feallan ‘fall’, gangan ‘go’, lêtan ‘let’, etc.) have unpredictable forms as well. Indeed, as Krygier 1998:126 points out, the only reason for

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\(^{244}\) I will use the abbreviations S and W, resp., for strong and weak verbs; if class membership within these categories needs to be indicated, I will use Arabic numerals, so, for example S1 = Class I strong verbs, W2 = Class II weak verbs, etc.

\(^{245}\) Data from Campbell 1959:296-342.
assigning a separate strong class to them is that they cannot be squeezed in anywhere else. Class S7, therefore, is but a litter bin class.

I will regard all of the above mentioned verbs as totally irregular right at the outset. Furthermore, strong verbs, though displaying more or less predictable patterns, are a closed class, continuously losing members and hardly ever gaining new ones. They have up to four different stems, which can hardly be related to each other via phonological rules. During the later history of the language, too, they continue to reduce in number, and their patterns become less and less transparent. As a result, they cannot be said to play a part in the formation of the regular conjugational patterns in later English. Nevertheless, their importance in OE is undoubtable, for two reasons: first, the strong category contains hundreds of items; second, many of these are among the most frequent verbs in the language. Due to this, I will refer to them whenever their properties appear to be relevant for regular weak verbs.

This section gives an overview of the conjugation of weak verbs. As I said, I limit the presentation to W1 and W2. First, I present the WS paradigms, found in traditional manuals. Yet, the majority of English speakers in OE times did not speak WS as a native dialect: indeed, WS is geographically limited to the very South-West of England, more precisely, to areas South of the Thames excepting Kent. The reason why WS is taken as a model in OE grammars is that Wessex was the cultural and political centre of England during the late OE period, and its dialect naturally became elevated to the level of a pan-English standard. Therefore, the overwhelming majority of OE texts is written in WS. For the later history of English, however, WS is of limited importance: after the Norman Conquest, it loses its cultural-political significance, the new centre being London. Furthermore, the new Norman nobility spoke French (AN, more precisely), English being degraded to the status of being the language of the “low” classes. It is only during the second half of the ME period that English becomes the dominant language and, accordingly, a new standard emerges. This standard, however, has its dialectal base in the East Midlands, whose dialects do not derive from WS but the Mercian dialect of OE. The dialectal differences within OE were neglected in the previous chapter because they were not particularly relevant for the discussion of nominal inflection. As for verbs, however, there is a significant difference between WS and the rest of England from our point of view, with regard to the Preterite of W2. I will therefore present the relevant differences but I base my analysis on Anglian forms. However, since WS is taken as a model in manuals, I start with the WS paradigms. The presentation is based on Campbell 1959:321,332. I omit some forms, notably the Inflected Infinitive and the Present Participle.
(neither of them are relevant for the later morphological developments) as well as all forms of the Subjunctive (which are formed from stems mentioned anyway, and they become, formally speaking, indistinguishable from the base or the plural in ME, so their inclusion would unnecessarily complicate the discussion\textsuperscript{246}).

(123) The paradigm of Class I weak verbs

<table>
<thead>
<tr>
<th></th>
<th>1a</th>
<th>1b</th>
<th>2</th>
<th>3a</th>
<th>3/b</th>
</tr>
</thead>
<tbody>
<tr>
<td>INF</td>
<td>fremman</td>
<td>lettan</td>
<td>nerian</td>
<td>dēman</td>
<td>cēpan</td>
</tr>
<tr>
<td>PRESINDSG1</td>
<td>fremme</td>
<td>lette</td>
<td>nerie</td>
<td>dēme</td>
<td>cēpe</td>
</tr>
<tr>
<td>SG2</td>
<td>frem(e)st</td>
<td>letest/lest</td>
<td>nerest</td>
<td>dēmst</td>
<td>cēpst</td>
</tr>
<tr>
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<td>nereþ</td>
<td>dēmþ</td>
<td>cēþþ</td>
</tr>
<tr>
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<td>neriaþ</td>
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<tr>
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<td>lete</td>
<td>nere</td>
<td>dēm</td>
<td>cēp</td>
</tr>
<tr>
<td>PL</td>
<td>fremmaþ</td>
<td>lettaþ</td>
<td>neriaþ</td>
<td>dēmaþ</td>
<td>cēpaþ</td>
</tr>
<tr>
<td>PRETPART</td>
<td>fremed</td>
<td>lett/leted</td>
<td>nered</td>
<td>dēmed/dēmd</td>
<td>cēped/cēpt</td>
</tr>
</tbody>
</table>

\textsuperscript{246} The Subjunctive, both in the Pres and the Pret, has but two forms: a common Sg and a common Pl one. The Sg one is always identical to the Sg1Ind of the same tense, and the Plural can be formed from the Sg by adding \textit{-n}, so, for example, \textit{fremme(n)} (Pres) — \textit{feremed(e)n} (Pret) for \textit{fremman}.  

(124) The typical WS paradigm of Class II weak verbs: *lufian* ‘love’

<table>
<thead>
<tr>
<th></th>
<th>PRES</th>
<th>INFINITIVE</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sg1</td>
<td>lufie/lufiġe</td>
<td>lufian/lufiġan</td>
</tr>
<tr>
<td>2</td>
<td>lufast</td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>lufaþ</td>
<td></td>
</tr>
<tr>
<td>Pl</td>
<td>lufiaþ/lufiġaþ</td>
<td></td>
</tr>
</tbody>
</table>

(125) The typical Anglian/Kentish paradigm of Class II weak verbs: *lufian* ‘love’

<table>
<thead>
<tr>
<th></th>
<th>PRES</th>
<th>INFINITIVE</th>
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</thead>
<tbody>
<tr>
<td>Sg1</td>
<td>lufie/lufiġe</td>
<td>lufian/lufiģan</td>
</tr>
<tr>
<td>2</td>
<td>lufast</td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>lufaþ</td>
<td></td>
</tr>
<tr>
<td>Pl</td>
<td>lufiaþ/lufiģaþ</td>
<td></td>
</tr>
</tbody>
</table>

As it is clear from the tables, W1 displays a wide variety of patterns; as opposed to this, W2 is uniform in the sense that all verbs of the class conjugate according to a single paradigm. Furthermore, Class W1/1 displays a phonologically unpredictable alternations of stem-final geminates with short consonants, cf. Sg1Pres *fremme* vs. Pret *fremede*, as well as variable deletion of -e- in the 2/3SgPres, cf. *frem(e)st*. It is no wonder that W2, a class which is highly productive from PGmc times on, becomes even more successful in OE in attracting new members. Indeed, new verbs, including loans, are regularly assigned to W2. The reasons are easy to see: W2 is uniform, it does not display unpredictable variation (though see more on
this below), i.e., the paradigm is insensitive to the phonological makeup of the stem, which (as we’ll see later) is not true for W1. Furthermore, W1 verbs are subject to two restrictions: (i) their root vowel can only be front, (ii) there appears to be a templatic restriction on the skeletal shape of their base; both restrictions will be discussed shortly.

Let us now see an overview of the morphophonology of W1. I will argue that the Preterite and the PretPart of most of these verbs can be analysed as analytic, and so is the Present of many items. I will propose, furthermore, that the ImpSg form be taken as a base to which analytic suffixes are added.

Generally speaking, the members of W1 are organised into subclasses according to the weight of the historical root. All verbs in this class are historically C-final roots, to which a thematic element -i- is added (realised as -j- before vowel-initial suffixes), so, for example, fremman has the historical root √frem-, which, extended with the thematic element, yields the stem alternants *fremi- and *fremj-. In the case of light (-VC) roots, the final C undergoes a process referred to as West Germanic Gemination before the thematic variant -j-, so Infinitive *frem-j-an > *fremm-j-an; later on, the yod is dropped, hence the attested situation, viz. Inf. fremman: this historical sequence yields the patterns observed in Class 1a/b. (See below for the Pret forms of lettan.) Note that the thematic variant -i- does not cause gemination, and unstressed i > e by OE times, so PretSg *frem-i-de 247 > OE fremede, ImpSg *frem-i > OE freme, etc. The gemination (and the subsequent loss of yod) fails to apply to r; here, the remaining yod is vocalised in OE to i, so *ner-j-an > neri; this sequence of events yields Class W2. Gemination also fails if the root is heavy, i.e., -VVC or -VCC, but the yod drops nonetheless, so *dēm-j-an > dēman, also *send-j-an > sendan. The thematic variant i regularly drops via HVD, so PretSg13 *dēm-i-de > OE dēmde, ImpSg *dēmi > OE dēm. This story is behind Class 3 of W1. If the root ends in a voiceless C, the Pret/PretPart suffix -d- assimilates to it in voice, hence subclass 3b.

In OE, however, it hardly makes sense any more, in a synchronic sense, to talk about W1 being divided into subclasses according to the weight of the root. Since the conditions causing Gemination have become obscure, variants within Class 1, such as frem- and fremm- are not relatable to each other phonologically (at least not in our model: in a classical generative theory, they present no particular problem). Second, there is no particular reason to assume that the final -e in nere, freme, etc. is a thematic element in OE. This stance is taken

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247 More precisely, *frem-i-de, with unstressed æ > e in OE. I simplify reconstructions if the original details are irrelevant and would introduce unnecessary complexities in the presentation.
by Kastovsky 1998:140, who suggests that the -e be analysed as part of the individual
suffixes, so ImpSg frem-e = root + ImpSg suffix, and frem-ed-e = root + Preterite marker +
Sg1/3 suffix. In this analysis, then, the ImpSg and the Pret suffixes display allomorphy, since
the respective forms of dēman, for instance, are dēm and dēm-de. I will suggest an alternative
below, claiming that the -e in nere(de) etc. is part of the root.

As noted above, the alternants of the frem- and fremm- type (Class 1) are no longer
derivable from a common underlier. I suggest, therefore, that this allomorphy be lexically
encoded. The same view is expressed by Kastovsky (ibid.). A result of this analysis is that the
Present forms in Class 1a/b are clearly synthetic, not only because of the C ~ CC variation but
also because in Sg1 and Pl suffixes attach to a bound stem, cf. fremman: fremm- is clearly not
a free stem. (According to Kastovsky, all other forms exhibit a bound stem as well, viz.
frem-.) This contrasts sharply with Class 3a/b, where all suffixes are attached to a free stem,
e.g., dēm, cēp: that these stems are free is evident from the fact that they appear unsuffixed in
the ImpSg. I take this as a basis for the following suggestion: all verbs of W1 have a free stem,
occurring in the ImpSg. This free stem appears in:
1) all forms of Classes 2 and 3 (see below on the e ~ i alternation in Class 2);
2) the Preterite and PretPart of all W1 weak verbs except Class 1/b;
3) the Sg23Pres of Class 1.
Let us now see some support for this proposal.

First, Classes 2 and 3 become regular and unified. Suffixes are transparent and
identical both classes; stem allomorphy is virtually eliminated. The only problem is the e ~ i
alternation in Class 2, cf. neri-an vs. nere-de. Yet, this is easily derivable phonologically,
even in the restrictive framework I use. Recall that mid front vowels in GP are represented as
combinations of the elements A and I. Note, furthermore, that e does not appear
prevocally in OE. I capitalise on this observation and claim that prevocalic e is unlicensed
in this language; if a vowel follows, it is deprived of its A element. Note that A.I is a complex
segment, whereas I is simplex. This is, then, an example of elemental decomposition, i.e.,
vowel reduction. Furthermore, such a process is not at all unique: the raising of mid vowels to
high ones prevocally is amply attested cross-linguistically, both synchronically and
diachronically. Schematically (where a = any vocalic expression):

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248 For example, in VL prevocalic e > i.
Such an analysis accounts for the facts in a coherent fashion and achieves a remarkable unification of Classes 2 and 3. The question is why the delinking of \( A \) takes place formally speaking: what is the phonological relation causing it? Note that V’s are inherent governors: they want to govern. In the majority of cases, they either govern the preceding empty Nucleus, or, if the preceding Nucleus is empty, they govern their Onset. The essence of hiatus is, however, that there is no filled Onset. Now, empty \( c \) positions, we can assume, are not potential targets for Government. Therefore, the only remaining target is the first V position. As Government is a “silencing” force if its target is a Nucleus (e.g., PG dooms its target to muteness), its effect manifests itself as the reduction of the complex expression to a simplex one. (I surmise that lexically full complex Nuclei are not liable to being silenced totally.)

Second, the assumption that the ImpSg is taken as a base form is cross-linguistically motivated. Bybee 1985:60ff shows that the base of regular verbs in Latin is realised with zero inflection in the ImpSg. The same is valid for Spanish. In fact, this is not at all unexpected. The Imperative can stand on its own as a sentence without an overt subject. Indeed, in English, which is otherwise non-Pro-Drop, the Imperative behaves that way.

Third, the same analysis, taking the ImpSg as a base, works nicely with non-WS W2 verbs too, as I will show shortly. Furthermore, the ImpSg of strong verbs is also the pure (present) stem. While strong verbs are not a productive class, they are numerous and include many items of high frequency, so their possible effect on the analysis of weak verbs cannot be denied. My proposal, therefore, achieves a generalisation about all OE verbs: the ImpSg is the stem.

Under this analysis, the only real irregularities are found in Class 1 of W1. We have already talked about the C ~ CC alternation. Yet, there is another irregularity: in Class 1b, the stem-final -e fails to appear in the Preterite, something that is unexpected both in the light of

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249 Note that the analysis I proposed for inflected forms of ende &Co. is different. There, however, I proposed a floating vowel stem-finally. Here, the final e in nere &Co. is a lexically attached, full Nucleus.

250 In fact, I make another stipulation: only operator (non-head elements) may be delinked. As e is I-headed, A may be delinked. This may be derivable from Structure Preservation: headedness must be preserved. I do not know of any detailed treatment of hiatus in the GP literature.

251 Strong verbs have several stems, the least marked being the present stem, which is equal to the ImpSg.
the aforesaid and in a historical sense, too: as light roots, verbs like *lettan, PretSg13 *let-i-de, should not show HVD (cf. 1a and 2). Yet, all verbs in Class 2 end in a coronal plosive, viz. d or t, cf. also *treddan ‘investigate’, PretSg13 tredde. In the Pret forms, such verbs uniformly behave in this way. The deletion of the vowel is probably a haplology effect, and I assume that such preterites had become lexicalised by the time of the attestation of OE. In the PretPart, WS shows the same deletion (lett, tredd), but this is not assumed for Anglian dialects (Campbell 1959:326), hence Anglian leted, treded. These PretPart forms are frequently written with a single <C>, i.e., let, tred in WS, due to the fact that geminates were most probably disallowed in word-final position in OE, being actually restricted to intervocalic position after a stressed vowel. I also assume that PretParticiples of this type are lexicalised, i.e., synthetic. Indeed, some of these verbs survive as irregulars up to the present, e.g., set, knit.

A final note: the consonant cluster resulting from the addition of the 2/3Pres suffixes -st and -þ to C-final bases often results in simplification, frequently shown in spelling, cf. the forms lett, lest in (123); the same can happen to other C-final weak verbs, too, e.g., mētan ‘meet’: 2Sg mēst, 3Sg mētt; fēdan ‘feed’: 2Sg fēst, 3Sg fētt, etc. These simplifications always involve clusters of plosives and fricatives (or affricates), and it is fair to assume that they reflect casual pronunciations, some of which may have entered more formal styles, too; at any rate, such simplifications are probably low-level phonetic processes rather than manifestations of some deep structural principle, so I will neglect them here.

To sum up, I assume that within W1, Classes 2 and 3 are analytic throughout the paradigm. Class 1 is partly analytic, partly synthetic. The analyticness of several forms is self-evident from the presence of long vowels in surface closed syllables (e.g., dēmde, kēpte), and often by the monomorphemically illicit clusters straddling the morpheme boundary (eg., cēpst, dēmde, also lēfde [vd]) See Kurath 1956 [1969]:142f on this. McCalla 1980 provides a list of two-member clusters occurring only across morpheme boundaries.
Analytic and synthetic forms in Classes of W1

<table>
<thead>
<tr>
<th></th>
<th>1a</th>
<th>1b</th>
<th>2</th>
<th>3/a</th>
<th>3/b</th>
</tr>
</thead>
<tbody>
<tr>
<td>INF</td>
<td>‘do, perform’</td>
<td>‘hinder’</td>
<td>‘save’</td>
<td>‘judge’</td>
<td>‘keep’</td>
</tr>
<tr>
<td>ResIndSG1</td>
<td>fremm-an</td>
<td>lett-an</td>
<td>neri-an</td>
<td>dēm-an</td>
<td>cēp-an</td>
</tr>
<tr>
<td>SG2</td>
<td>frem(e)-st</td>
<td>lete-st/lest</td>
<td>nere-st</td>
<td>dēm-st</td>
<td>cēp-st</td>
</tr>
<tr>
<td>SG3</td>
<td>frem(e)-þ</td>
<td>lete-þ/lett</td>
<td>nere-þ</td>
<td>dēm-þ</td>
<td>cēp-þ</td>
</tr>
<tr>
<td>Pl</td>
<td>fremm-aþ</td>
<td>lett-aþ</td>
<td>neri-aþ</td>
<td>dēm-aþ</td>
<td>cēp-aþ</td>
</tr>
<tr>
<td>Pl</td>
<td>fremm-aþ</td>
<td>lett-aþ</td>
<td>neri-aþ</td>
<td>dēm-aþ</td>
<td>cēp-aþ</td>
</tr>
<tr>
<td>RetIndSG13</td>
<td>freme-de</td>
<td>let-te</td>
<td>nere-de</td>
<td>dēm-de</td>
<td>cēp-te</td>
</tr>
<tr>
<td>SG2</td>
<td>freme-de-st</td>
<td>let-test</td>
<td>nere-de-st</td>
<td>dēm-de-st</td>
<td>cēp-te-st</td>
</tr>
<tr>
<td>Pl</td>
<td>freme-d-on</td>
<td>let-t-on</td>
<td>nere-d-on</td>
<td>dēm-d-on</td>
<td>cēp-t-on</td>
</tr>
<tr>
<td>ImpSG</td>
<td>freme</td>
<td>lete</td>
<td>nere</td>
<td>dēm</td>
<td>cēp</td>
</tr>
<tr>
<td>Pl</td>
<td>freme-d</td>
<td>(a) let-t</td>
<td>nere-d</td>
<td>(a) dēm-ed</td>
<td>(a) cēp-ed</td>
</tr>
<tr>
<td>(b) lete-d</td>
<td>(b) dēm-d</td>
<td>(b) cēp-t</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

I shaded those boxes which contain synthetic forms, indicating morpheme boundaries for the sake of clarity, although it must be borne in mind that phonologically speaking, these morpheme boundaries have no relevance. Note that several forms, notably V-initial ones, taken to be analytic here, can also be analysed as synthetic. However, given the abundance of forms which are clearly analytic, I will assume that the entire Class 2 and 3 paradigms were analysed as word-based, hence analytic, by OE speakers. Historians of English will have noticed that I departed from the usual segmentation of the suffixes in the Preterite into Tense marker + person/number marker; more on this shortly.

Let us first see the CVCV representations of some forms. In (128), I provide a representation of dēman, neri-an, nere-de and dēmde, to illustrate both fully analytic classes before a V-initial as well as before a C-initial suffix.

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254 Recall that, in the nominal inflection, this was not the case: the overwhelming majority of forms could be analysed either way.
One problem, to which I alluded above, must be still clarified. Notably, the Preterite forms contain two suffixes: the Preterite marker and a Person/Number marker. As I noted, I depart from classical analyses in the way I draw the boundary between the affixes. Let us see in what sense and what my reasons are.

The traditional analysis is presented in (129a) (cf. Kastovsky 1998:140), whereas (129b) shows my version.
The two segmentations into morphemes differ in two ways. First, I assume that verbs of the nerian type have a free stem; we have discussed this already. Note that this move unifies the Preterite marker, too, analysable as an analytic suffix attaching to free stems, uniformly beginning with a d. Second, I claim that the Preterite suffix is -de, rather than -d. In other words, conventional analyses take the e to be part of the Person/Number suffix. This view, however, is difficult to maintain except from a historical point of view (historically, the Preterite suffix is indeed -d-). On the other hand, there is an important fact which has passed unnoticed in the literature. Specifically, the Sg1 and Sg3 forms are homonymous. They always are: no OE verb makes a distinction. There is no exception to this rule: even the most irregular verbs are well-behaved in this sense. It is reasonable to assume, therefore, that the Sg1 and the Sg3 are simply unmarked. There are two arguments I put forward in defence of this claim.

First, let us take strong verbs. As I mentioned, they have up to four distinct roots; beran ‘bear’, for example, has the following root allomorphs: ber, bær, √bær-, √bor-. Note that the last two are bound roots. The first root, that of the Present, occurs without a suffix in the ImpSg. The second one, bær, serves as the Sg1 and Sg3 form of the Preterite, without any suffix. It is more likely, viewed in this light, that OE speakers analysed nerede as a form unmarked for Person/Number.

Second, the homonymy of the Sg1 and Sg3 form in the Preterite is not at all unusual. In several Romance languages, for example, this is the rule in the Imperfect, cf. the following Imperfect Sg1/3 forms: Po fala-va ‘speak’, diz-ia ‘say’, Sp toma-ba ‘take’, ten-ia ‘have’, OF chante-ve ‘sing’, etc. Therefore, to assume the same for OE is cross-linguistically supportable, too.

The only reasonable objection against this analysis is that the PretPl has the suffix -on, attached directly to -d-. Yet, we have already made use of floating segments in our analyses of nominal inflection: there is no particular reason not to apply the same to the Preterite of verbs. Specifically, I propose that the Preterite marker contains a floating e, which attaches to the

---

available nuclear position if its final, but, if it is followed by a full Nucleus, it is silenced by
Proper Government. A representation of -d-on is given in (130):

(130) PG EG
\[ C \quad V - c \quad V \quad C \quad v \]
\[ d \quad e \quad o \quad n \]

As we’ll see, this analysis anticipates the verbal morphology of ME.

The presentation of W1 verbs is not yet complete: there is a problematic class,
including items such as timbrian ‘build’, to which I will come back; however, in order to
understand their peculiarity, we must first take a look at W2 verbs.

The most important difference between WS and other dialects from our point of view relates
to W2 verbs. I repeat the paradigms in (124) and (125) as (131) and (132) for convenience,
highlighting the relevant parts by shading them:

(131) The typical WS paradigm of Class II weak verbs: lufian ‘love’

<table>
<thead>
<tr>
<th>PRE</th>
<th>IND</th>
<th>Sg1</th>
<th>lufie/lufiġe</th>
</tr>
</thead>
<tbody>
<tr>
<td>2</td>
<td>lufast</td>
<td></td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>lufaḥ</td>
<td></td>
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</tr>
<tr>
<td>Pl</td>
<td>lufiḥaḥ/lufiġaḥ</td>
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<table>
<thead>
<tr>
<th>IMPERATIVE</th>
<th>INF</th>
<th>FINITIVE</th>
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</tr>
</thead>
<tbody>
<tr>
<td>Sg</td>
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<td>PRE</td>
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</tr>
<tr>
<td>Pl</td>
<td>lufiḥaḥ/lufiġaḥ</td>
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</tr>
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</table>
The typical Anglian/Kentish paradigm of Class II weak verbs: *lufian* ‘love’

<table>
<thead>
<tr>
<th></th>
<th>PRE</th>
<th>IND</th>
<th>PRE</th>
<th>IND</th>
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<tbody>
<tr>
<td>SG1</td>
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<td>SG13</td>
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</tr>
<tr>
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<td></td>
<td>2</td>
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<td>lufaþ</td>
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<td>P</td>
<td>lufadon</td>
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<td>Pl</td>
<td>lufiaþ/lufiġaþ</td>
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**Imperative**

<table>
<thead>
<tr>
<th></th>
<th>INF</th>
<th>INFINITIVE</th>
<th>lufian/lufiġan</th>
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<tr>
<td>SG</td>
<td>lufa</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pl</td>
<td>lufiaþ/lufiġaþ</td>
<td></td>
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</tr>
</tbody>
</table>
(cf. the Words-and-Rules Theory), is in direct relation to productivity. Specifically, it appears that symbolic rules can be happily given to derive all inflected forms of a large number of W1 verbs, an unproductive class, but the same option is unavailable for the only productive class, W2. Yet, as often, appearances are deceptive.

First, the majority of dialects (all Anglian ones) give up the -i-final stems by ME times, regularising the paradigms. We will see the ME conjugation later on. This is expected: irregularities in a productive class are not infrequently levelled out. The situation in OE is, then, similar to the nominal declensions: OE represents a transitional period on the way from a fully synthetic to a fully analytic system. It is hardly surprising that some inconsistencies are retained.

Second, though many W1 verbs are indeed analytic, I believe that there are good reasons for why are unproductive, as I mentioned earlier: notably, there appear to be lexical restrictions on the shape of their roots as well as the shape of their stems (bases), which is uncharacteristic of W2 verbs.

Let us first consider the restriction on their root, more precisely, their root vowel. Recall that W1 was originally characterised by a thematic element -i-/j-. In Pre-OE, a process referred to as I-Umlaut (or I-Mutation) operated, whereby stressed vowels were affected by a high front segment (= i or j) in the following syllable. The details of Umlaut are irrelevant here; suffice it to say that it involved the fronting of back root vowels and the raising of non-high front ones. For example, the OE verb trymman ‘strengthen’, originally formed from the adjective trum ‘strong’, underwent the following development: *trum-j-an > (West Gmc Gemination) *trumm-j-an > (Umlaut) *trymm-j-an > (Yod-dropping) trymman257, also in the Sg13Pret *trum-i-da > (Umlaut) trym-i-da > (other changes) trymede. The result of Umlaut was devastating for W1: back root vowels were eliminated from all forms of W1 paradigms. After the environment for Umlaut was destroyed by later changes (Yod-dropping and the lowering of i to e), the fact that no W1 verb contained a back root vowel became a lexically given speciality of the class, which no general rule could account for. This fact undoubtedly contributed to the loss of productivity in this class. In the case of W2 verbs, no such restriction was operative.

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256 In early Gmc, stressed V = root V, since stress falls on the root.
257 Pre-OE o/œ was first fronted to œ/œ, but, except in very early texts, it becomes unrounded to e/ë, cf. dēman ‘deem’ vs. dōm ‘doom’.
The second restriction concerns the overall shape of the base (stem) of W1 verbs. Let me give a formal representation of the stems of W1 verbs we have discussed plus that of sendan ‘send’, which, having a heavy root, goes exactly like dēman:

(133) **W1 verbs: stem structures**

(a) nere CVCV  
(b) dēm CVcVCv  
(c) send CVCvCv

Based on these structures, I make the following claim (see below for explanations):

(134) **W1 stem structure template**

The stem of W1 verbs contains, minimally and maximally, two CV units.

The proposal may seem to be false: after all, dēm and send have 3 CV pairs each. Yet, the last V is a FEN. It is a widely observed property of FENs that they fail to count for prosodic purposes (this manifests itself, in traditional terms, as the “extrametricality” of surface final consonants). Examples are abundant. We saw in Chapter 3, for instance, that Classical Latin stress is assigned to the third-last Nucleus. I omitted C-final words for the sake of simplicity, but it is now time to deal with them. Take the word dōminus ‘lord’ for example. It has a beautiful CVCVCVCv skeleton. We would expect the stress to fall on the penult, i.e., *dominus. Yet, it falls on the antepenult. Traditional analyses propose that final syllables are extrametrical. In CVCV terms, we can say that the E-Governed FEN is invisible for higher-level (prosodic) purposes. The same phenomenon can be observed in Modern English verbs: édit, for instance, is stressed on the penult, since disyllabic verbs are only final-stressed if the ult is heavy. The final -t does not seem to count.

I propose, therefore, that (134) is a principle of OE prosodic organisation, which disregards Cv units containing an E-Governed Nucleus, i.e., a FEN. Therefore, the prosodically “real” skeletons of the stems in (133) is as follows:

(134) **W1 verbs: stem structures — Near-final version**

(a) nere CVCV  
(b) dēm CVcV (Cv)  
(c) send CVCv (Cv)
Note that word-internal empty positions do count, just like for purposes of stress.

Nonetheless, there are still problems: verbs beginning with a consonant cluster, such as spildan ‘destroy’ or clyppan ‘embrace’. Note, however, that these invariably involve #sC or #TR clusters. As for the latter, we saw in Chapter 3 that the empty nucleus enclosed between the T and the R is “invisible” for higher levels: it’s circumscribed by being enclosed in an IG domain. IG domains behave like single consonants for prosodic purposes: for example, they do not make the preceding syllable heavy. We also noted the problematic nature of #sC clusters, and made a proposal that they, too, probably constitute a kind of Melodic Government domain. I conclude, therefore, based on independent claims of the theory, that verbs exhibiting initial clusters do not contradict the principle. Melodically “handled” empty Nuclei do not count for higher levels — much like FENs, which are handled from outside the phonology. It seems, therefore, that the prosody disregards everything that is not handled by the phonology. Let us sum up the aforesaid in a diagram:

\[(135)\] **W1 verbs: stem structures —Final version**

(a) nere | CVCV
(b) dēm | CVcV (Cv)
(c) send | CVCv (Cv)
(d) clypp | (Cv) CVCv (Cv)

The essence of the proposal is that W1 verb stems are subject to a templatic restriction. Striking confirmation is provided by several weak verbs which showed a particularly strong tendency from the earliest times to go over to W2, though they were originally W1. Some of these are given in (136).

\[(136)\] hyngran ‘hunger’ > hyngrian
bīecnan ‘make a sign’ > bīecnian
timbran ‘build’ > timbrian
symblan ‘feast’ > symblian
efnan ‘level’ > efnian
frēfran ‘comfort’ > frēfrian
segłan ‘sail’ > segłian
It is true that several W1 verbs join W2. The type *nerian*, for example, is particularly affected: but here, the obvious reason is formal similarity, i.e., the ending -*ian* in the Infinitive and several forms of the paradigm. Even so, *nerian*-type verbs are very frequently found with W1 inflections, too. Yet, the verbs of the type in (136) abandon W1 with amazing consistency, as if something were “driving” them out of the class. To the best of my knowledge, there is no existing satisfactory explanation for this. Note that these verbs do not exhibit formal similarities with W2 verbs (unlike *nerian* &Co.). The proposal I make here is that they flee from W1 because, unlike the overwhelming majority of W1 verbs, they are just “too big”: they all have an ImpSg (= base) in -*e*, e.g., *timbre*, *symble*, *efne*, *seğle*, etc. Not counting the empty V sitting inside the clusters *br* and *bl* in *timbre* and *symble* (assuming that these clusters are IG domains), they have the skeleton CVCVCV.

To sum up, W2 is the winner in the long run against W1 due to the following reasons:

1. Already in PGmc, it is becoming the productive class. W1 hardly adds new members.
2. W2 verbs have a single paradigm whereas W1 has various subclasses, one of which (1) exhibits irregularities.
3. W1 verbal roots are restricted inasmuch they may not contain a back vowel.
4. W1 verbs are subject to a templatic restriction, whereas W2 verbs are not.

It must be pointed out that the unproductivity of W1 soon manifests itself in that inflected forms of such verbs, especially the Pret and the PretPart, start to become lexicalised, and by the beginning of the ME period W1 becomes virtually irregular. This, however, is the topic of the following sections. Let us now turn our attention to the emergence of the ME system.

6.2. [The weak verbs in ME]

As I have just mentioned, W2 verbs come out as the “winners” by the time of the earliest attestations of ME. W1 items are either lexicalised or join W2. At the same time, W2 verbs undergo a full-scale regularisation: the bound allomorphs of W2 stems in -*i* disappear (except in the South). The following discussion provides the details.

Verbs of W1 are clearly lexicalised by the beginning of ME, if not earlier. The late 12th century Ormulum, one of the earliest ME texts, provides striking evidence. Its author, Ormm, has a strange habit of indicating the shortness of vowels (stressed and unstressed) by doubling the consonant graph after the vowel letter. So, for example, he writes, *patt* ‘that’, *Ennglisssh*
‘English’, *iss* ‘is’, etc\textsuperscript{258}. Consider now the following W1 forms (taken from Bermúdez-Otero 1998a):

\begin{table}[h!]
\begin{tabular}{lll}
\hline
\textbf{Orrm} & \textbf{OE} & \textbf{Gloss} \\
\hline
cwemmde & cwēmde & ‘please-PretSg13’ \\
demmde & dēmde & ‘deem-PretSg13’ \\
keppte & cēpte & ‘keep-PretSg13’ \\
spredd & sprēdde & ‘spread-PretPart’ \\
wepptenn & (Strong in OE) & ‘weep-PretPl’ \\
ledde & lǣdde & ‘lead-PretSg13’ \\
hidd & hīdd & ‘hide-PretPart’ \\
\hline
\end{tabular}
\end{table}

The spellings of Orrm unambiguously show that the root vowel was short. The generally accepted view is that this is due to a sound law, known as Pre-Cluster Shortening, which operated in eME and shortened long vowels before certain two-member clusters. We will have occasion to question the validity of this statement; in fact, I will deny the existence of such a sound law. This is the topic of the next section; for the time being, let me simply point out that W1 preterites must, in our framework, analysed as synthetic — otherwise, we would have no reason for the shortening. Note that when the stem-final C is followed by a vowel, there is no shortening.

The present tense of the class is not particularly interesting, simply because it does not survive: W1 verbs assume the present tense suffixes of W2 verbs by ME. These will be dealt with below.

The most remarkable thing about verbs of this class is the very early extension of the voiceless allomorph of the Preterite/PretPart marker, viz, *-t*, to stems ending in a sonorant or \textbf{Cd}. While Orrm has *d* in the forms demmde, cwemmde, the usual pattern is with *-t*, e.g., (all forms are PretSg13) senden — sente ‘send’ (OE sende), mēnen — mente ‘mean’, fēlen — felte ‘feel’, etc\textsuperscript{259}. The only exception is hēren — herde ‘hear’ (up to the present day, this is the only irregular weak verb which shortens its vowel and adds *-d*)\textsuperscript{260}. Several items, such as dēmen, went over to W2, hence PretSg13 dēmede > MoE deemed. The point is that this “devoicing” is

\textsuperscript{258} Data from Lass 1997:54.
\textsuperscript{259} Data from Mossé 1952:75.
\textsuperscript{260} It must be added, though, that \textbf{rd} was a lengthening cluster in IOE/eME (see Homorganic Lengthening in 6.3.), so that the Pret form was most probably hērde in eME; as there was no shortening, the *-d* remained.
totally unmotivated phonologically speaking. It appears that the suffixal allomorph -t (which was used in OE only after voiceless stem-final consonants) came to be lexically associated with all irregulars (except ‘hear’) which exhibited a VV ~ V alternation. Again, a certain degree of templaticness can be observed: a ME irregular weak verb displays the pattern in (138):

(138) | Infinitive/Present | Pret | Examples |
--- | --- | --- | --- |
-VVC | -VCt(e) | *kepte, felte, grette, fedde* |
-VC{t, d} | -VCt(e) | *sente, caste ‘cast’* |

Notes:
1. Forms like *fedde* are easily explained with reference to progressive voicing assimilation.
2. The infinitives in the second row are *senden* and *casten*, resp.
3. The verbs *sell* (*sold*) and *tell* (*told*) are already irregular in OE; note also that they have a long vowel in the Preterite, too, as opposed to a short one in the Infinitive.

This restructuring is the clearest proof that W1 Preterites became lexicalised. As I mentioned, their Present forms were completely remodelled after W2. Let us now discuss a typical Anglian W2 paradigm, such as the one found in Chaucer, i.e., the London dialect of the 14th century.

The lexicalisation of W1 verbs left the former W2 class as the only regular, and (as we’ll see) by now fully analytic paradigm. This means that by eME, if not in IOE, the old strong/weak dichotomy broke down, and the “regular/irregular” division, characterising MoE, came into existence. It is true that in ME, strong verbs still differed from irregular weak ones in more ways than in MoE (e.g., they had two stems in the Preterite), which suggests that the strong/weak division was probably more strongly felt than now. Yet, the fact that the modern “regular/irregular” dichotomy is rooted deep in the past is undeniable.

The following paradigm can be said to be typical for ME, though it must be added that by the 14th century, it had become somewhat archaic; more on this below. It represents the verb *loven* ‘love’:

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261 The final -e was, of course, lacking in the PretPart
262 See Kastovsky 1998 for detailed arguments.
263 Based on Obst & Schleburg 1999:137
(139) *The ME regular paradigm* (note: unstressed <e>, of course, = schwa; stress is initial)

<table>
<thead>
<tr>
<th></th>
<th>PRESENIND</th>
<th>PRETIND</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Sg1</strong></td>
<td><em>luve</em></td>
<td></td>
</tr>
<tr>
<td>2</td>
<td><em>luve-st</em></td>
<td></td>
</tr>
<tr>
<td>3</td>
<td><em>luve-þ</em></td>
<td></td>
</tr>
<tr>
<td><strong>Pl</strong></td>
<td><em>luve-n</em></td>
<td></td>
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<th></th>
<th>IMPERATIVE</th>
<th>INFINITIVE</th>
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<tr>
<td><strong>Sg</strong></td>
<td><em>luve</em></td>
<td><em>luve-n</em></td>
</tr>
<tr>
<td><strong>Pl</strong></td>
<td><em>luve-þ</em></td>
<td><em>lvey-inge</em></td>
</tr>
</tbody>
</table>

Note: Subjunctive forms are identical to the Sg13 and the Pl of the Indicative in the respective tenses.

As with nouns, the merger of unstressed vowels in schwa has a serious consequence: the OE differences have been completely levelled out. Furthermore, the problematic *-i* (cf. OE *luvie* ‘I love’, etc.) has disappeared.

The segmentation into morphemes is practically unproblematic. The form *luve* can be taken as a free base, to which inflections are added. In traditional grammars, the *-e* is taken to be part of the suffixes; yet, there is no synchronic reason for it. Note that the PresSg1 has become suffixless, the vowel readily interpretable as part of a unified stem. Let us now see the details.

The PresPart is included in the table because it is the obvious ancestor of MoE *-ing*. In OE, the PresPart was formed differently (with a suffix *-nd*, which did not survive the ME period\(^\text{264}\)). The immediate problem with it is that it appears to join a bound stem. Yet, prevocalic schwa is not found in ME (just like in MOE). I assume, therefore, that the schwa is delinked in prevocalic position, as shown in (140):

\(^\text{264}\) It does occur in ME texts, but it is not found in the London dialect, which is in this period on the way to serve as the basis for a national standard.
The schwa is represented melodically by the element @, which I assume is never a head in ME, at least it can never be a head on its own. This stance is supported by the following facts: (i) schwa never appears in stressed syllables, (ii) it is the only short vowel occurring in word-final position. All other short vowels are banned finally. As it is not a head, it can be deleted.

In OE, I analysed the Preterite suffix as -de, with a floating e, to account for the fact that it deletes before the Pl suffix -on. Since OE o > ME a, the schwa can without any difficulty be assigned to the Preterite suffix in ME. Whether it is a floating melody or it is lexically attached to its Nucleus is not clear from the data: it always surfaces, and either interpretation could account for this. Note that the stem-final schwa cannot be interpreted as a floating segment, since if it were, it ought to be silenced by PG in the Preterite forms by the schwa of the Preterite suffix.

The presentation above suggests that all regular verbs have a schwa-final base in ME. This may sound like a radical claim: are there no consonant-final or long vowel final stems? In fact, there aren’t, at least not in this class: stems ending in a long vowel, such as dō ‘do’, gā ‘go’, bē ‘be’, are all highly irregular verbs; consonant-final ones are former W1 verbs or strong verbs, neither representing a regular pattern in ME.

The claim that all regulars have a schwa-final base is supported by the adoption of French loans: they are, as newcomers, usually assigned to the regular class, and they are adopted with a stem-final schwa, even those whose stem in French ends in a long vowel (incl. diphthongs), cf. stems like crē ‘cry’, preie ‘ask, pray’, but also chaunge ‘change’, move ‘move’, etc265.

As I pointed out earlier, the paradigm given in (139) had become somewhat archaic by Chaucer’s time, for the following reasons:

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265 Cf. Mossé 1952:76.
Firstly, the forms taking the suffix -n dropped this suffix, becoming identical to the base. In Chaucer’s texts, one finds both sēken and sēke for ‘to seek’, for example. The deletion of this suffix started early in the North, gradually spreading southwards. According to Mossé 1952:79, this process became widespread in London in the 14th century; this is in accordance with the variation shown in Chaucer. We can assume that by the end of the century, -n-less forms were probably the norm in natural spoken usage.

Secondly, final schwas were gradually being deleted throughout the ME period, again, starting from the North. By the 14th century, they were at best optional in spoken usage; Chaucer employs them for purposes of rhyme and metre, and the final schwa is regularly dropped in his poetry before a following vowel-initial word. Final schwas certainly disappeared completely by the end of the ME period.

Thirdly, not only did schwas disappear in word-final position: they tended to be deleted domain-finally, too, inside words. Chaucer frequently has forms such as luvars ‘lovers’ instead of earlier luvars, or kumθ for earlier kumθ ‘come-Sg3Pres’. This, however, was probably not as widespread as the deletion of schwas in absolute final position, but it was an option nonetheless; and, if it was used in poetry, we can hardly doubt that it existed in the spoken language, too. It takes considerable time until the deletion of schwa before suffixal consonants establishes itself; in the 3rd Sg of the present tense (as well as in the Plural/Genitive of nouns) the present-day distribution of schwa vs. zero in inflectional endings is only generalised in the 16th century, and in the Preterite of verbs still later (Lass 1992:81). Nevertheless, the pattern did exist in ME, and by the end of the period the conjugation of verbs is basically identical to the modern system, as shown in (141):

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266 Obst & Schleburg 1999:37.
The differences between this system and the modern one are minor, and relate to individual suffixes rather than the overall morphophonological structure. Later changes include (i) the loss of a distinct ImpPl form, (ii) the loss of 2Sg (still found in Shakespeare’s time), (iii) the replacement of 3SgPres -þ by -s, the latter generally used in the North from eME times, but slowly spreading southwards; in the 14th century, it is not found in the London dialect yet (Chaucer, for example, only uses it to characterise Northern speakers), but by the early 17th century it is widespread (Shakespeare uses it regularly), and it soon replaces the old suffix.

This concludes our discussion of ME morphology. In the final section, I devote some attention to the alleged sound law known as Pre-Cluster Shortening. I will argue that such a sound change never existed: the forms which display shortening are not the victims of a sound law but of lexicalisation.

### 6.3. [“Pre-Cluster Shortening” revisited]

The change challenged here has been called “Pre-Cluster Shortening” or “Shortening Before Consonant Clusters” (henceforth: PCS) in the literature. The change was first described and analysed in detail by Luick (see below). The traditional formulation has been modified during the past hundred years or so, especially in recent decades, and, following Luick, most analyses treat PCS as part of a more general conspiracy to optimise feet (see, e.g., Ritt 1994, 2004, Bermúdez-Otero 19998a,b) but one claim has never been questioned: PCS was a phonological change. I will argue that this view is incorrect.
In most accounts, the change is called Pre-Cluster Shortening (e.g., Lass 1992:72, 1994:249) or Shortening Before Consonant Clusters (Ritt 1994, passim). The two names are identical in their content and implications; I use the former merely because the latter is more lengthy. In traditional grammars, the change is not named anyhow, but is simply listed as an instance of ME quantity changes (more below on PCS and other changes in length).

Luick (1914/21:324) describes the change as follows:

(...) trat Verkürzung in allen Fällen ein, in denen ein Vokal vor zwei oder mehr Konsonanten stand

(...) shortening took place in all cases where a vowel stood before two or more consonants


Luick places the change in die “Übergangszeit”, i.e., the transitional period, between OE and ME. There is widespread consensus among scholars that the change must have been complete by around the twelfth century, because the ME text traditionally called Oermulum, dating from the late 1100’s from Lincolnshire (Lass 1997:54), clearly shows its effects. This, however, does not say anything about when the change operated: it only proves that it must have been completed by that time. Historians seem to agree on a relatively early start: Fisiak (1968:30) dates it “before 1000”, and Brunner (1963:9) also writes, “in the 10th century, and later in similar conditions, long vowels were shortened”. Lass (1992:72) describes it as “beginning about the eleventh century”. Luick himself (1914/21:327f) places the change “am wahrscheinlichsten in den Jahrzenten vor und nach 1000”, i.e., “probably in the decades before and after 1000”. It can be safely stated, therefore, that PCS was operative as early as about the turn of the millennium, if not earlier. As a result, it is hardly justified to call it a Middle English change, as it is often done. Historians of English seem to agree that the beginning of Middle English is not earlier than 1100, which clearly means that PCS is a late OE change, or possibly late OE — early ME. Of course, it must be borne in mind that “Old English” and “Middle English” are labels one puts on texts belonging to certain periods, and

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267 Translations are mine — L.K.
268 The word mādd is, in fact, a mistaken example, as I will point out below: here, the shortening is earlier, as shown by the different MoE reflex of the vowel than that of lädde (i.e., MoE æ vs. e).
269 We mentioned this earlier.
in reality language changes gradually, so that whether we call it an OE or ME change is more a matter of terminology than of theoretical status, but I find it useful to remind the reader that PCS is, in fact, earlier than what its traditional description as a ME change would suggest. Indeed, we’ll see that several items traditionally assumed to undergo this change shortened their vowel much earlier.

PCS is but one of the changes in vowel quantity that is usually assumed to be operative in late OE/early ME. The others are:

(a) Trisyllabic Shortening (TSS): Long vowels shorten in stressed antepenults, e.g., OE sūþerne (> ME sutherne) ‘southern’, hālıgđeġ (> ME holidei) ‘holiday’.

(b) Pre-Cluster Lengthening or Homorganic Lengthening (HOL): Short vowels lengthen before a homorganic cluster of a sonorant + a voiced stop; if the sonorant was r, the second C could be a voiced fricative or n, too, e.g., OE ċild (> ME child) ‘child’, climban (> ME clīmben) ‘climb’, murnan (ME mournen) ‘mourn’, etc. A third C blocked HOL, so OE ċildru > MoE childre(n).

(c) Open Syllable Lengthening (OSL): Short vowels lengthen in open penults, e.g., OE nama (> ME nāme) ‘name’, brecan (> ME brēken) ‘break’, etc.

Of these changes, (b) is early, dated “by about the tenth century (if not earlier)” (Lass 1994:249), while (a) and (c) are late ones, dated around 1200 (Lass 1992:73). The fact that HOL is much earlier than OSL is proved by the different qualitative outcomes: for example, OE a, lengthened by HOL yields a:, which merges with original OE a; in ME (except in the North), both appear as ā, cf. OE (Anglian273) haldan ‘hold’, stān ‘stone’ > ME hōlden, stōn (cf. the identical vowels in MoE, too). As opposed to this, OE ā, when it undergoes OSL, yields ME ā:, as in OE nama ‘name’, bacan ‘bake’ > ME nāme, bāken: that is, the ā produced by OSL fails to undergo the shift of OE ā to ā. In other words, the chronology is (1) HOL, (2) ā: > ā, (3) OSL.

Some notes are in order here. First, I would like to draw the reader’s attention to the wide temporal span — at least two hundred years, but possibly more — defined by these

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271 The former term is confusing (cf. Pre-Cluster Shortening!), hence I will use Homorganic Lengthening.

272 There is a great deal of dialectal variation, too, and lengthening before some clusters (e.g., ꞣ/) is later “reversed”, cf. ME sŏng > MoE song.

273 In West Saxon, which is the “standard” dialect of OE, ‘hold’ appears as heald, but the MoE standard is of Anglian origin.
quantitative changes. Second, HOL seems to contradict PCS in terms of their structural description, but this is only apparent: the homorganic clusters that cause (early) lengthening fail to cause (later) shortening. This fact, of course, has not passed unnoticed, cf. the formulation of Brunner (1963:9), who writes about PCS, “long vowels were shortened before groups of two consonants, except those […] causing lengthening”.

A brief note is necessary concerning terminology: the sound change in question, as noted already, is usually referred to as Pre-Cluster Shortening. The term is misleading: only those clusters cause shortening which close the preceding syllable, i.e., Coda-Onset Clusters. Before Syncope sites, there is no shortening, cf. Orm’s deecness ‘deacons’, cf. OE. dēacnas, Sg. dēacon²⁷⁴: “bogus” clusters, i.e., ones which exhibit vowel ~ zero alternations, are not shortening clusters. It is doubtful if OE has non-initial TR clusters which are clearly IG domains: in the overwhelming majority of instances, foot-internal TR clusters appear to be bogus ones. I leave this question open, since it would require a thorough study of OE phonotactics to answer this question, and, unfortunately, OE phonotactics has not been described at all (apart from some pages in a couple of books and articles). All in all, it would is better to call the change “Closed Syllable Shortening”, as it has been done by some authors²⁷⁵.

The obvious reason for the term “Pre-Cluster Shortening” is that no shortening occurs in syllables “closed” by a single final C. Yet, final consonants, not being Codas, obviously do not matter. I will therefore use the term “Closed Syllable Shortening” (CSS) from now on.

The uncertainties mentioned earlier regarding the dating of CSS are due to a very simple fact: OE orthography, just like that of Classical Latin, does not denote vowel length. The length indications given in edited texts, dictionaries and grammars are mostly based on etymology: it is assumed that OE preserves the inherited length of vowels. Crucially for us, the theory of CSS itself relies mostly on such etymological considerations. This makes the whole argumentation intricately circular. Consider the following train of thought characterising theoreticians of CSS. OE had a long vowel in, say, softe ‘soft’ (= it was sōfte). How do we know? Because it derives from a VN sequence, cf. its German cognate sanft, and the vowel lengthened as a result of compensatory lengthening. We know this, because you have items such as goose which have a long vowel up to the present day, so they must have had a long

²⁷⁴ From Bermúdez-Otero 1998a
²⁷⁵ E.g., Bermúdez-Otero 1998b
vowel in OE: gōs, cognate with German Gans. Conclusion: ‘soft’ must have been sōfte. ME, as well as MoE, have a short vowel, so the vowel shortened in eME. The problem is this: how do we know when the vowel shortened? We don’t. It’s pure conjecture. It might never have been long: the loss of the nasal might not have resulted in compensatory lengthening in a closed syllable, as opposed to an open one (note that final C’s are Onsets). ME soft may well go back to OE softe, too: an OE original o, just like an o shortened in lOE/eME, yields the same ME vowel. Length isn’t indicated in OE texts. We haven’t proved anything about when the shortening happened.

The major problem with vowel length in OE is that the majority of etymological long vowels which shorten at some point prior to eME merge with their originally short counterparts, as in the case of o/o. There are but three long vowels which give us a relatively reliable clue: the long low vowels and the low diphthong of OE, viz. æ, a, æa. The diphthong merges with æ, yielding the same result, so I will not treat it separately. The important point is that in lOE, the long low vowels raise to mid-low position (I will call this simply Raising here), so æ > e, a > ø.276 Now, OE “etymological” short æ and a merge in ME a. Therefore, if the ME word has a short vowel deriving from an OE long low one, we have the following possibilities:

1. The ME word contains a. In this case, the shortening is early, chronologically preceding Raising: æ/a > (Early Shortening) æ/a > a.

2. The ME word contains e or o. In this case, the shortening is late, following Raising (the shortened mid-low vowels fall together with OE short e/o): æ/a > (Raising) ɛ/ɔ > (Late Shortening) e/o.

Examples:

1. OE mēdd ‘mad’ > ME mad must have undergone early shortening, otherwise it would undergo Raising > * * mēdd first, then yielding ME ** med.

2. OE āscian ‘ask’ > ME asken must have also undergone early shortening, otherwise the ā would raise to ō, yielding * * oun > * * osken.

3. OE lēdde ‘led’ > ME ledde is late shortening: the vowel first raises, then it shortens. Early shortening yields ME ladde (in fact, such a form is amply attested).

4. I have found no clear example in the literature for OE ā undergoing late shortening.

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276 The latter change does not take place in the North, but this is not relevant for us. Note that ɑ undergoes rounding, too, but this is not unexpected.
The abovesaid leads to an important conclusion: the only instance we know when the long vowel was shortened is if it contained a low vowel in OE. In all other cases, everything is but a conjecture.

The second problem with CSS is that it assumes that OE and ME differ in a significant sense: the former tolerated superheavy rhymes, but the latter did not. This appears to be unfounded, but it is widely assumed and even stated explicitly in the literature. Yet, the hard fact is that in non-analytic domains, one hardly finds sure examples. All instances where the length of the vowel in OE can be safely assumed is the Preterite and PretPart of W1 verbs with radical æ, such as lædan (see the previous section). Based on this, the shortening of long vowels in W1 Preterites is early Middle English. As we saw, however, such forms were analytic in OE (which is shown by the monomorphemically illicit clusters straddling the morpheme boundary, cf. læfde ‘left’. I assume, therefore, the following:

CSS was not a sound change. Instead, morphologically complex analytic forms which became lexicalised, hence synthetic, conformed to the existing patterns of phonotactics, including the ban on long vowels in closed syllables.

Let’s see some specific examples.

1) A number of items which display shortening are obscured compounds or derived words, often formed with suffixes that are not productive even in OE; several of them also exhibit cluster simplification. All instances I have found with an OE low vowel point to early shortening or have two variants, one implying early shortening (ES), the other late shortening (LS). This supports the assumption that CSS was a result of lexicalisation which went on for centuries, and affects lexical items at different points in time, often producing variation: put differently, it spreads by lexical diffusion; and if there is one thing we expect to spread that way, it is lexicalisation. I will give the etymological length in the OE data, but note that

---


278 Plus the W2 verb clænsian klaenzian, whence MoE cleanse. Note, however, that homorganic sonorant + obstruent sequences are lengthening ones. In fact, the surprising thing about this verb is that it undergoes shortening.

279 The cluster vd is consistently eliminated in ME, which is in accordance with my claim: not only do long vowels shorten to conform to existing phonotactic patterns, but so do monomorphemically illicit clusters. The cluster in question is either simplified to dt, as in OE hæfde > ME hadde, had ‘had’, or is replaced by ft, as in left < OE læfde.
several of these items may have undergone shortening even quite early in the OE period. Examples:

OE wræþ-{æ}þu ‘wrath’ > ME wræþ (ES), wreppe (LS)
OE hlæfdi{æ}ge ‘lady’ > ME lafdi, lādy (ES), levdi (LS)
OE wifmann ‘woman’ > ME wimon, womman (???)
OE mǣdd ‘mad’ > ME mad (ES, and very early at that, just like fiëtt ‘fat’ > ME fæt)280

2) The only examples where shortening is claimed to occur inside a non-analytic domain are before the clusters xt and ft281. However, the examples with a low vowel all point to early, hence OE shortening with the former cluster; with the latter, there are no surely datable examples (??? = we cannot tell, as the vowel betrays nothing about the time the V shortened):

OE tāhte ‘taught’ > eME tahte (ES)
OE āhte ‘property’ > ME (Orrm) ahhte (ES)
OE fīfți{æ}g ‘50’ > ME fifty (???)
OE söfte > ME softe (???)

3) Preterite/PretPart of W1 verbs. As I said, verbs with æ are found both with e and a in ME, although in MoE, the former versions survive; as pointed out in the previous section, there is no W1 verb with a back root vowel, hence there are no examples with OE a:

OE lǣdde ‘led’ > ME ledde (LS), ladde (ES)
OE lāfde ‘left’ > ME lefte (LS), lafte (ES)
OE cēpte ‘kept’ > ME kepte (???)

The assumption that eOE did tolerate superheavy rhymes is also contradicted by West Germanic Gemination (WGG). Recall that WGG affected all consonants exc. r, but only after a short vowel. If the gemination would have resulted in a superheavy rhyme, it was blocked, cf. OE fremman < *fram-j-an, but OE dēman < *dōm-j-an. The prehistory of OE, therefore, shows that the language, at an early stage, did not legalise superheavy rhymes, and neither did ME. It seems thus that CSS is none other but a rescue operation to mend forms which produced superheavy rhymes by historical accident. But this is exactly what my position says: concatenations which became synthetic via lexicalisation fell under the ban on superheavy rhymes. No sound law is needed to account for the shortenings. Furthermore, the fact that shortening seems to characterise the language from eOE to eME, gradually devouring forms

---

280 Both are lexicalised PretParticiples already in OE. In Classical OE, geminates were not licit finally. The shortening, therefore, must have occurred at a very early date, before the geminates shortened, possibly before the first attested OE texts appeared.

281 Excepting sC clusters, to be discussed below.
as they are becoming lexicalised, lends strong support to my claim. Let me now discuss two remaining issues.

First, a precursor of CSS is assumed in OE: shortening of long vowels before a three-member cluster (I will call this shortening 3S for the sake of brevity). There are, however, only two examples given in the literature:

- bræmblas ‘brambles’ > bræmlas;
- gōdspell ‘godspell’ > godspell.

Now, to establish a sound law on the basis of two examples is suspicious at best: such a law is nearly unique. Furthermore, the items themselves are problematic, as pointed out by Murray 1988, who questions the validity of 3S as a genuine sound change. The first one goes back to PGmc *brāmilōz, subject to HVD, yielding Pre-OE brāmlas, cf. NomSg brāmef. The point is that the form bræmblas, the alleged input to 3S, shows lexicalisation in terms of consonantal makeup, too: as long as the syncope is transparent, the cluster ml can happily exist, the two consonants being separated by a floating segment, i.e., bræmlas. The appearance of b is, however, expected, if the form was reinterpreted as non-syncopating, hence the cluster ml no longer straddles a floating V; this cluster, however, is not quite optimal in this case, and the stop is inserted to create a monomorphemically licit Coda-Onset cluster mbl. Such insertions are far from being unique, cf. MoE fa[mbl]y, or the regular insertion of a stop inside a cluster of nasal + liquid in West Romance, e.g., F chambre < *kamra < CL cameram ‘chamber’, nombre ‘number’ < *nomro < CL numerum, Sp hombre ‘man’ < *omre < *omne < CL hōminem, etc. The appearance of a new NomSg bræmbel shows that relexicalisation did in fact take place. As a result, we can safely assume that the shortening of the vowel is also a consequence of relexicalisation.

As for godspell, an alternative explanation is available (see Murray 1988:99, referring to the OED): as vowel length was not indicated in writing, the word was easily reanalysed as containing the noun god, rather than the adjective gōd ‘good’ (of course, gōdspell is a calque of evangelion ‘good news’), which is not at all unexpected since the text was more easily associated with God than with good. At any rate, even if shortening is assumed before reinterpretation, this is the only example for 3S, which is then a sound law accounting for a single item. The model I propose, viz. that shortening in closed syllables occurred as a result

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282 Forms with ē are also found, being characteristic of Anglian dialects, where WGmc *ā regularly shows up as ē, as opposed to WS, where it > ā, see also WS strēt = Anglian strēt, cf. G Strasse, Dutch straat.
of lexicalisation from early times on, removes the need to refer to the number of consonants and can easily do away with 3S.

The second issue is shortening before sC clusters: it is by far not consistent, as it is generally pointed out. There is shortening in, e.g., dust < OE dúst, fist < OE fýst, but no shortening in, e.g., east < OE ēast, priest < OE prēost. Traditional explanations blame analogy from inflected forms, where the sC cluster is assumed to be a complex Onset, hence there is no CSS. Yet, the status of such clusters as Onsets is quite dubious. Note, however, that sC clusters are special anyway, inasmuch as they can occur initially. I assume, therefore, that the variation in such cases is simply due to the ambivalent behaviour of sC clusters, i.e., this is a melodically, rather than structurally, governed phenomenon. Note furthermore that OE and later English do not differ in whether they allow long vowels before such clusters: up to the present day, English happily tolerates them in this position. Therefore, the occasional shortenings can hardly be interpreted as the result of a phonotactic change between OE and ME: I conclude that they are simply not appropriate examples for CSS.

To conclude, the wide time span and the fact that all forms with demonstrable late shortening are suspiciously analytic in OE lead me to claim that CSS was not a sound change at all, especially not an eME one. The forms which are undoubtedly non-analytic in OE clearly point to early shortening, possibly pre-OE. The verbs of W1 may be the only regular group which exhibits IOE/eME shortening (but even there, variation exists); this is perfectly in line with the assumption I made, i.e., that they lost their analyticness in precisely this period. Finally, I would like to point out that those suffixes which are highly productive — up to the present day, in fact — quite consistently fail to cause shortening. Again, this supports the argument presented here. (These suffixes include -ness, -ly, etc., OE -nesse, -liċ, cf. Orrm’s godnesse ‘goodness’, gastliċ ‘ghostly’; note that Orrm would double the following <C> if the vowel were short.)

The last question to be answered is why the Neogrammarians, Luick in particular, invented this sound law as well as 3S. The reason for this is simple: he was forced to do so by the theoretical framework he used. Alterations in sound shape, according to Neogrammarian theory, must preferably be explained with reference to regular sound changes. As there are, undoubtedly, instances where shortening before two consonants (= CSS) is clearly IOE or

---

283 Interestingly, Bermúdez-Otero 1998a observes the morphological effects on CSS, but he comes to the conclusion that CSS was very early morphologised. I believe it was not: it had never been phonological.
eME, the interpretation is that there was a sound law operating in this period. As for 3S, it is clearly needed as a separate law to account for the two examples of early shortening we discussed. As I pointed out in the Introduction, history is heavily theory-laden. It is interesting to note that some other sound laws established in the late 19th or the early 20th centuries, such as Trisyllabic Shortening or Open Syllable Lengthening, have been partially or totally questioned in recent decades (see, e.g., Minkova 1982, Ritt 1994:103ff). For some reason, the same has not happened with CSS. I do not, of course, wish to see all sound laws being falsified, but it must be borne in mind that they are not there in the same sense as African elephants are: they are inventions. I hope to have shown that the anti-theory bias often shown by historians is thus unfounded: we do rely on theory, even if the basic (Neogrammarian) model is taken for granted to such an extent that we tend to forget that it is a theory after all. As such, it makes predictions and forces one to interpret data in a particular way. The overall model I adopted in this dissertation suggests something else.

The fact that CSS didn’t exist as a sound law has an interesting implication regarding cognateness. Recall from Chapter 1 that we determined cognateness as derivability from a common ancestral form via sound laws. If CSS is not a sound law, the root alternants in MoE pairs such as *keep ~ kept* are, technically speaking, not cognates — “counterintuitive non-cognates”, as it were: they are no more related than, say, *choose* and *chosen*.

Yet, the situation is not quite serious, because we do have a “rescue strategy”: lexicalisation itself. The relatedness of these MoE pairs can be proved by historical investigation, and they do have a common ancestral form. But the scientific proof of cognateness in the technical sense is derivability via sound laws. This is the essence of Neogrammarian theory, and the most fundamental theoretical tenet of historical linguistics. Ironically, the Neogrammarian quest for laws (wherever possible) led to the formulation of a regularity which does not exist. History is, as we know, full of surprises.
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APPENDIX I

This appendix is a guide to how OE forms cited according to traditional orthography should be interpreted phonetically. I follow the recent authoritative descriptions of OE. Note that in OE manuscripts, vowel length is not indicated, and <c> and <g> are not used with diacritic marks, either. All these markings are editorial. I depart from the usual practice in indicating light (contour, i.e., monomoric) vs. heavy (real, i.e., bimoric) diphthongs: the tradition is, for example, to render a light <ea> as [æə] while a heavy <ēa> as [æːə]. I believe that this practice is misleading. Instead, I choose to represent light vs. heavy diphthongs differently in phonetic transcriptions, e.g., <ea> (light) = [æə], vs. <ēa> (heavy) = [æːə], in a parallel fashion with affricates. NOTE: * After nasals and initially; ** between voiced segments.

<table>
<thead>
<tr>
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<td>iːy</td>
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# APPENDIX II

## MIDDLE ENGLISH VOWEL SOUNDS AND SPELLINGS

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<th>IPA</th>
<th>Spelling</th>
<th>Examples (with ModE equivalents)</th>
</tr>
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<tbody>
<tr>
<td>a</td>
<td>[a:]</td>
<td>a</td>
<td>a</td>
<td>sak (sack), shal (shall)</td>
</tr>
<tr>
<td>ā</td>
<td>[a:]</td>
<td>a</td>
<td>a (aa)</td>
<td>tale (tale), aker (acre), caas (case)</td>
</tr>
<tr>
<td>e</td>
<td>[e:]</td>
<td>e</td>
<td>e</td>
<td>bed (bed), seven (seven), herte (heart)</td>
</tr>
<tr>
<td>ē</td>
<td>[e:]</td>
<td>e, ee</td>
<td>clene (clean), deel (deal), breeth (breath)</td>
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</tr>
<tr>
<td>ē</td>
<td>[e:]</td>
<td>e, ee</td>
<td>gees (geese), seke (seek)</td>
<td></td>
</tr>
<tr>
<td>i</td>
<td>[i]</td>
<td>i, y</td>
<td>hiden (hide), mys (mice)</td>
<td></td>
</tr>
<tr>
<td>ī</td>
<td>[i]</td>
<td>i, y</td>
<td>hiden (hide), mys (mice)</td>
<td></td>
</tr>
<tr>
<td>o</td>
<td>[o]</td>
<td>o</td>
<td>o</td>
<td>frogge (frog), cok (cock)</td>
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<tr>
<td>ō</td>
<td>[ɔ:]</td>
<td>o, oo</td>
<td>hoom (home), boot (boat), throte (throat)</td>
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<tr>
<td>ō</td>
<td>[ɔ:]</td>
<td>o, oo</td>
<td>goos (goose), mone (moon), good (good)</td>
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<tr>
<td>u</td>
<td>[u]</td>
<td>u, o</td>
<td>ful (full), bukke (buck), sonne (sun), comen (come)</td>
<td></td>
</tr>
<tr>
<td>ū</td>
<td>[u]</td>
<td>ou, ow</td>
<td>mous (mouse), brow (brow)</td>
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<tr>
<td>(e)</td>
<td>[ə]</td>
<td>e</td>
<td>talę (tale), rootę (root), alle (all)</td>
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<tr>
<td>ei</td>
<td>[e:i]</td>
<td>ai, ay, ei, ey</td>
<td>hail (hail), day (day), seil (sail)</td>
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<tr>
<td>oi</td>
<td>[oi]</td>
<td>oi, oy</td>
<td>joy (joy), cloistre (cloister)</td>
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<tr>
<td>ui</td>
<td>[ui]</td>
<td>oi, oy</td>
<td>joinen (join), poysen (poison)</td>
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<tr>
<td>au</td>
<td>[au]</td>
<td>au, aw</td>
<td>lawe (law), aught (aught)</td>
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<tr>
<td>ēu</td>
<td>[ɛu]</td>
<td>eu, ew</td>
<td>fewe (few), knew (knew), beautee (beauty)</td>
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<td>iu</td>
<td>[iu]</td>
<td>eu, ew, u</td>
<td>newe (new), vertu (vertue)</td>
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<tr>
<td>ōu</td>
<td>[ou]</td>
<td>ou, ow</td>
<td>snow (snow), dough (dough), broughte (brought)</td>
<td></td>
</tr>
</tbody>
</table>

The representation of consonants is generally the same as in MoE; <gh> = [x].
KRISTÓ LÁSZLÓ

The restructuring of Early English morphology: Theoretical foundations and some consequences

A korai angol alaktani rendszer átalakulásának elméleti alapjai és néhány következménye

Nyelvtudományi Doktori Iskola, Angol Nyelvészet Program

Témavezető: Nádasdy Ádám CSc., egy. doc.

Budapest, 2005.
I hereby certify that this dissertation is entirely the result of my own work, and that no material is included for which a degree has previously been conferred. I have faithfully, exactly and properly cited all sources made use of in the dissertation.

12/03/2005

Témavezető aláírása/Supervisors’s signature:
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