

The Structure of Phonological Theory

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Abstract

This dissertation takes a Minimalist approach to phonology, treating the phonological module as a system of abstract symbolic computation, divorced from phonetic content. I investigate the position of the phonological module within the architecture of grammar and the evolutionary scenario developed by Hauser et al. (2002a) and Fitch et al. (2005).

Chapters 1& 2 introduce Minimalism, the substance-free approach to phonology, and Evolutionary Phonology, the tripartite foundation upon which the dissertation rests. I argue that the role of diachrony must be factored out from synchronic phonological theory: what is diachronically possible must be separated from what is computationally possible and from what is learnable.

Chapter 3 seeks to define the nature of phonological representations. This chapter addresses issues such as whether phonological features are innate or emergent, how much underspecification is allowed in lexical representations, and how segmental and suprasegmental material is organized into strings. I argue that phonological representations are ‘flat’ or ‘linearly hierarchical.’

Chapter 4 establishes the formalisms for the repertoire of primitive operations, SEARCH, COPY, and DELETE, which account for all (morpho)phonological processes. I illustrate the application of these operations with analyses of data from domains

such as vowel harmony, reduplication, affixation, and subtractive morphology, then extend ‘generalized SEARCH and COPY’ to the rest of phonology.

Chapter 5 moves from the representations and operations developed in the previous chapters to the syntax-phonology interface. This chapter argues for maintaining a direct reference conception of the syntax-phonology interface, based on the notion that phonology and syntax operate on synchronized cycles.

Chapter 6 focuses on the broader implications of the theory presented in the earlier chapters. I demonstrate on the basis of behavioral and physiological studies on a variety of species that all the cognitive abilities necessary for human phonological representations and operations are present in creatures other than *Homo sapiens* and in domains other than phonology.

Chapter 7 summarizes the dissertation and suggests directions for future research.

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Chapter 1

Introduction

2009 marks the fiftieth anniversary of the publication of Noam Chomsky’s review of B.F. Skinner’s *Verbal Behavior* (Chomsky 1959), universally considered to be one of the most important papers in the cognitive revolution. The arguments for nativism put forth in the review laid the groundwork for a science of the ‘language organ’ or ‘faculty of language,’ a research program pursued for the past fifty years by generative linguists. Much of the work during this period has focused on describing the properties of Universal Grammar (UG), the initial state of the faculty of language. Over the years, more and more structure has been attributed to UG, with the goal of reducing language acquisition to a manageable parameter-setting task for a child learner (i.e., taming what has come to be called Plato’s Problem). However, with the advent of the Minimalist Program (Chomsky 1995b), and particularly in more recent Minimalist works, (e.g., Chomsky 2004, 2005, 2007, Boeckx 2006, *inter alia*), this perspective has shifted, as Chomsky (2007:3) notes:

“Throughout the modern history of generative grammar, the problem of determining the character of [the faculty of language] has been approached ‘from top down’: how much must be attributed to UG to account for language acquisition? The [Minimalist Program] seeks to approach the problem ‘from bottom up’: How little can be attributed to UG while still

accounting for the variety of I-languages attained[...]?”

This dissertation takes a bottom-up approach to phonology, made possible by treating the phonological module as a system of abstract symbolic computation, divorced from phonetic content. In short, I pursue the research agenda laid out by Hale & Reiss (2000a,b), investigating the universal core of formal properties that underlie all human phonological systems, regardless of the phonetic substance or indeed of the modality by which they are expressed. I place emphasis on the position of the phonological module within the Minimalist conception of the architecture of grammar, and also the evolutionary scenario developed by Hauser et al. (2002a) and Fitch et al. (2005), especially the distinction between the ‘Faculty of Language - Broad Sense’ (FLB) and ‘Faculty of Language - Narrow Sense’ (FLN).

The work undertaken here opens the door for future research into questions which are independently raised given another consequence of the Minimalist Program (see Boeckx 2008): genuine variation within narrow syntax has been eliminated, being relegated instead to the lexicon and to morphology. As a result, there can be no more study of comparative *narrow* syntax, but careful investigation of phonological representations and processes can provide complementary data that is bound to inform our knowledge of syntax, both narrowly and broadly construed. Investigating phonology from this perspective lays the groundwork for testing and refining the arguments made by Bromberger & Halle (1989) in support of the view that phonology is fundamentally different from syntax (contra van der Hulst 2005, Anderson 2006). Such work allows us to focus not on the question of *whether* phonology is different, but rather *how* it is different and *why* this is the case.

Chapters 2 & 3 are less data-oriented than what follows; they lay the foundation upon which the theory developed in Chapters 4 & 5 rests. Chapter 2 establishes the methodology to be used in the remainder of the work. It provides an introduction to linguistic minimalism, distinguishing this ontological commitment from methodological minimalism (i.e., Ockham's Razor) and characterizing a Galilean research program for phonology that strives to go 'beyond explanatory adequacy' (Chomsky 2004). We discuss the implications for phonology that stem from the Strong Minimalist Thesis (Chomsky 2000)—the notion that language is the optimal solution to linking the Sensory-Motor (SM) and Conceptual-Intentional (C-I) systems—and from the idea that phonology is an 'ancillary' module "doing the best it can to satisfy the problem it faces: to map to the SM interface syntactic objects generated by computations that are 'well-designed' to satisfy C-I conditions" but unsuited to communicative purposes (Chomsky 2008). I also introduce the hypothesis, to be supported throughout the ensuing chapters and particularly in Chapter 6, that phonology provides a domain-general solution to a domain-specific problem, namely the mapping of syntax to the SM interface, and that nothing in phonology is part of FLN.

Also in Chapter 2, I seek to circumscribe the domain to be covered by a synchronic theory of phonology by clarifying assumptions about the nature of language acquisition and sound change. Following Ohala (1981), Blevins (2004), and others, I emphasize the role of the listener in propagating sound change and maintain that the cross-linguistic distribution of sound patterns correlates, at least in large part, with the frequency of the diachronic changes that give rise to those patterns. I argue that, if the goal of synchronic phonological theory is to characterize the properties of

possible phonological systems, the role of diachrony must be factored out: what is *diachronically* possible must be separated from what is *computationally* possible, which is still different from what is *learnable*. This has serious consequences for phonological theory: on this view, markedness, naturalness, ease of production, ease of perception, and other functionalist principles should be eliminated from phonology proper; these are ‘E-language’ phenomena outside of the realm of linguistic competence, and thus not demanding of explanation in the synchronic grammar. Eliminating these functional considerations in the grammar has the immediate advantage of reducing the size of Universal Grammar and simplifying synchronic phonological theory.

In Chapter 3 we turn to the task of defining the phonological representations which provide a workspace for the operations to be defined in the next chapter. We begin with discussion of phonological features, in which I present arguments from spoken and signed languages for the emergent feature hypothesis (Mielke 2008) and provide evidence that emergent phonological categories can be learned via a self-supervised learning involving cross-modal perceptual grounding (Coen 2006).

At this stage I argue for a theory of ‘archiphonemic’ underspecification along the lines of Inkelas (1995), as opposed to ‘radical’ or ‘contrastive’ underspecification. I also distinguish between a perseverant form of underspecification that persists at all stages of the phonological and phonetic representations (see Keating 1988, Hale et al. 2007) and a resolvable type that is repaired by application of the SEARCH and COPY operations specified in the next chapter.

Zooming out, we next discuss the organization of segmental and suprasegmental material into strings and the idea that phonological representations are ‘flat’ or

‘linearly hierarchical’ (Neeleman & van de Koot 2006). We compare phonological syllables and syntactic phrases, which have been equated by Levin (1985) and many others, with some even claiming that phrase structure was exapted from syllable structure (Carstairs-McCarthy 1999). I argue with Tallerman (2006) that these analogies are false, and provide evidence that many of the properties commonly attributed to syllabic structure can be explained as well or better without positing the existence of innate structure supporting discrete syllables in the grammar.

Chapters 4 & 5 form the empirically-oriented core of the thesis. In Chapter 4, we establish the formalisms for the repertoire of primitive operations that will be employed throughout the remaining chapters. The operations developed in this chapter suggest two future avenues of research: first, comparing these operations to their counterparts in narrow syntax, and second, exploring the possibility that they are used in other species and/or cognitive domains as well. We take the first steps toward such a goal in Chapter 6.

Three operations are formalized in Chapter 4:

- **SEARCH** provides a means by which two elements in a phonological string may establish a probe-goal relation. The **SEARCH** algorithm we adopt is a modified version of the one formulated by Mailhot & Reiss (2007), itself a formalization of the system of simultaneous rule application found in Chomsky & Halle (1968:344): “to apply a rule, the entire string is first scanned for segments that satisfy the environmental constraints of the rule. After all such segments have been identified in the string, the changes required by the rule are applied simultaneously.”

- COPY takes a single feature value or bundle of feature values from the goal of a SEARCH application and creates a copy of these feature values to the probe. We establish a typology of possible conditions on COPY independent from the parameters of SEARCH, a program begun but not fully explored by Mailhot & Reiss (2007).
- DELETE removes an element from the derivation. We discuss the difference between deletion and ‘jump links’ (Raimy 2000a, Gagnon 2008) that mimic deletion of segments but are in fact a predicted result of SEARCH and COPY.

I illustrate the application of these operations with analyses of data from domains such as vowel harmony, reduplication, affixation, and tone spread. I argue that these three parameterized operations yield a restricted typology of possible phonological processes that can achieve the necessary empirical coverage without positing autosegmental tiers or constraints on representations such as the No Line-Crossing Constraint.

Chapter 5 takes us from the representations and operations developed in the previous chapters to the syntax-phonology interface. I argue that “phonological derivation by phase” makes it possible to combine the best parts of Lexical Phonology (Kiparsky 1982), Distributed Morphology (Halle & Marantz 1993), and Derivation by Phase (Chomsky 2001). This is in spirit a defense of the direct reference conception of the syntax-phonology interface (Kaisse 1985, Odden 1990, Cinque 1993). The basis for this theory is the notion that phonology is cyclic and therefore inescapably derivational. I further argue, following Marvin (2002), that this is the *direct consequence* of cyclicity (i.e., phasality) in syntax. This is the “best-case scenario” according to

Chomsky (2004:107):

“Assume that all three components [syntax, semantics, & phonology] are cyclic, a very natural optimality requirement and fairly conventional. [...] In the best case, there is a single cycle only. [Phonology] is greatly simplified if it can ‘forget about’ what has been transferred to it at earlier phases; otherwise, the advantages of cyclic computation are lost”

Not only may this solution be computationally efficient, it also allows us to recognize the important contributions of cyclic models of phonology such as those proposed by Chomsky et al. (1956), Kean (1974), Mascaró (1976), Kiparsky (1982), and Mohanan (1982), *inter alia*.

With this model in place, I argue that the phonological operations described in the previous chapter are triggered by applications of Merge in the narrow syntax and can be viewed as interface-driven repair strategies necessitated by ill-formed objects being transferred to phonology. I present arguments that the domains of phonological rule application, both above and below the word level, come for free when we assume Distributed Morphology and a phasal syntax. Specifically, phonological processes and operations such as linearization of looped structures get the chance to apply at each application of Spell-Out, and phonological rule application is restricted by the Phase Impenetrability Condition (Chomsky 2001). I claim that morpheme-level phases can replace Lexical Phonology’s hierarchy of strata, and that clause-level phases can replace the prosodic hierarchy. These arguments are supported with analyses of segmental and suprasegmental processes including detailed case studies from Basque and Korean.

This chapter also includes discussion of the status of various phonologically null syntactic objects with respect to the domains of phonological rule application in an at-

tempt to reconcile the findings of Truckenbrodt (1999), Seidl (2001), and An (2007b). We also discuss the distribution of pronounced items that seem to be sensitive to prosodic factors, such as the ‘second-position clitics’ found in the Slavic languages and elsewhere.

Chapter 6, the final body chapter, takes the comprehensive model of human phonological systems developed throughout the earlier portion of the dissertation as its starting point and discusses the broader implications of such a model. I demonstrate on the basis of behavioral and physiological studies on animal cognition conducted by other researchers on primates, songbirds, and a wide variety of other species that all the cognitive abilities necessary for human phonological representations and operations are present in creatures other than *Homo sapiens* (even if not to the same degree) and in domains other than phonology or, indeed, language proper; this implies that nothing required by phonology is part of FLN.

Contrary to the statements by Anderson (2004) and Yip (2006a,b) to the effect that phonology has not been tested in animals, we see that virtually all the abilities that underlie phonological competence have been shown in other species. The phonological properties for which we discuss animal counterparts include phonemic category building, natural classes, arbitrary rules/distributions, computations of identity, linear and hierarchical grouping, and rhythmicity, in addition to the SEARCH, COPY and DELETE mechanisms developed in Chapter 4. I reject the claim made by Pinker & Jackendoff (2005:212) that “major characteristics of phonology are specific to language (or to language & music), [and] uniquely human,” and their conclusion that “phonology represents a major counterexample” to the hypothesis proposed by

Hauser et al. (2002a), namely that FLN consists of only recursion and the mapping from narrow syntax to the interfaces.

Chapter 7 summarizes the dissertation and suggests directions for future research.

Chapter 2

A Minimalist Program for Phonology

2.1 Introduction

Before delving into the matters of representations and operations which will occupy us for the remainder of this work, I would first like to define the object of the present study: synchronic phonology. This chapter seeks to circumscribe the domain by laying out what I take to be the primary aims of a phonological theory that is both ‘substance-free’ and Minimalist in character. Since these terms mean a lot of different things to a lot of different people, I want to be clear at the outset about the usage employed here. We will discuss the implications for phonology that stem from the Strong Minimalist Thesis (Chomsky 2000), the notion that language is the optimal solution to linking the Sensory-Motor and Conceptual-Intentional systems, and from the idea that there is an asymmetry between these two interfaces, with the latter enjoying a privileged position. I will also justify why I do *not* attempt in this thesis to explain the ‘naturalness’ or prevalence of phonological processes, and introduce Evolutionary Phonology (Blevins 2004) as a theory of diachronic phonology

which provides a natural companion to the theory of synchronic phonology for which I argue.

2.2 Linguistic minimalism

Since phonology does not exist in a vacuum, neither can phonological theory. The theory presented here is designed to integrate with a particular architecture of grammar, and a particular way of thinking about language as an object of scientific study, which since the mid-1990's has been known as the Minimalist Program. I want to introduce this way of thinking first, and then discuss its connection to phonological theory in particular. In this chapter I will not stray too far from historical and conceptual concerns, but in later chapters, and particularly in Chapter 5, we will delve into the particulars of the particular instantiation of Minimalism within which I situate my theory of phonology.

The discussion in this section relies heavily on works by Cedric Boeckx (Boeckx 2006, To appear), which provide a much more in-depth discussion of the historical origins and theoretical underpinnings of Minimalist thinking than space considerations allow me to provide here. For those who are interested in reading more about particular syntactic topics, Bošković & Lasnik (2007) contains excerpts from many of the foundational works in Minimalism, arranged thematically. There are also several good textbooks on how to “do” Minimalist syntax, to which I refer the interested reader, including Lasnik et al. (2005) and Hornstein et al. (2005). I hope that this dissertation will provide a guide for how (and why) to do Minimalist phonology.

For fifty years now, linguists in the generative tradition have sought the answer to

what has been called Plato's Problem: how is it that children, barring pathology, all acquire the language(s) of their surroundings—whatever those may be—in a way that seems both uniform across individuals and essentially effortless (at least compared to the difficulties adults face in acquiring a foreign language)? Noam Chomsky's extremely influential review of B.F. Skinner's *Verbal Behavior* (Chomsky 1959) put forth numerous compelling arguments that the answer to Plato's Problem lies in our human biological endowment; our species is genetically programmed to grow a language, just as we are programmed to grow arms or lungs. This laid the groundwork for a science of the 'language organ' or 'faculty of language' which makes growing a language possible. Naturally, much of the work during the past half-century of inquiry into Plato's Problem has focused on describing the properties of Universal Grammar (UG), the initial state of the faculty of language. Over the years, more and more structure has been attributed to UG, with the goal of reducing grammar acquisition to a manageable parameter-setting task for a child learner. Chomsky (2007) calls this the 'top-down' approach to characterizing UG.

Perhaps the single most important facet of Minimalism is that it turns the top-down approach on its head. As Chomsky (2007:3) describes it, the Minimalist Program "seeks to approach the problem 'from bottom up': How little can be attributed to UG while still accounting for the variety of I-languages attained[. . .]?" This shift in perspective is particularly apparent in more recent Minimalist works (e.g., Chomsky 2004, 2005, 2007, Boeckx 2006, *inter alia*), but it is implicit in the Strong Minimalist Thesis, which dates back to the early 1990's. The Strong Minimalist Thesis is, as Boeckx (To appear) puts it, "a challenge to the linguistic community: Can it be shown

that the computational system at the core of [the faculty of language] is optimally or perfectly designed to meet the demands on the systems of the mind/brain it interacts with?" This is a hard-line gamble, and one that even by Chomsky's admission is likely to be wrong. Even so, it is important to push the Strong Minimalist Thesis as far as it can go (and it has taken us quite far already), because it encourages us to *make sense* of the language faculty's properties, not in isolation, but rather within the larger picture of cognition. What's more, pursuing this line of inquiry is bound to yield new understanding of the Conceptual-Intentional and Sensory-Motor systems, because it forces us to think about the "legibility conditions" imposed on the language faculty by those other modules.

It is critical to distinguish this ontological commitment from methodological minimalism (i.e., Ockham's Razor). What I mean by this is that the point of linguistic Minimalism is not to make the linguist's calculations more economical (a methodological minimalism); rather, we're making a bet that *the object of our study* is simple (an ontological or metaphysical minimalism). To be sure, ontological minimalism demands methodological minimalism. It requires that we eliminate redundancy from our theories and adopt the simplest solutions possible, *ceteris paribus*.¹ This is just good science, and good philosophy, as has been recognized for centuries. But it goes deeper than that, imploring us to seek to go "beyond explanatory adequacy" (Chomsky 2004). The Strong Minimalist Thesis asks us to posit only what is a 'virtual

¹Although, as Boeckx (2006) points out, economy considerations have been demoted from axiomatic to theorematic status in more recent Minimalist works; the emphasis has turned from economy *principles* to economy *effects*.

conceptual necessity' or empirically unavoidable, and to seek explanations for the latter category in the systems with which the language faculty must interface.

As is often stressed, and rightfully so, Minimalism is a *research program* in the sense of Lakatos (1970), not a theory (see discussion in Boeckx (2006), §3.3). The Strong Minimalist Thesis does not imply that any one specific view of UG is correct; it is better thought of as a question than an answer. In practice, Minimalist theories are largely built on the foundations of Principles & Parameters, and on Government & Binding Theory more specifically. And there are properties which characterize Minimalist theories of the architecture of grammar to the exclusion of earlier ones, like the elimination of levels of representation (DS, SS, LF, PF). This abandonment of the Y-/T-model inevitably leads to a new understanding of the syntax-phonology interface, which I discuss at length in Chapter 5. Despite the way Minimalism has radically reshaped our view of the interfaces, it seems odd that, as van Oostendorp & van de Weijer (2005:3) remark, the Minimalist Program “has not been applied to phonology;” similarly, Pinker & Jackendoff (2005:220) state that “The Minimalist Program, in Chomsky’s original conception, chooses to ignore... all the phenomena of phonology.” But there is no reason why this should be. The following quote summarizes one of the primary motivations behind the present work:

“For decades, generative linguists have viewed the internal grammar in terms of the interplay of two types of factors: genetic endowment, generally referred to as Universal Grammar (UG), and experience—that is, exposure to e-language. In recent years this picture has been augmented by a third type of factor: general principles of biological/physical design. This new focus tends to worry those who had been hoping for a rich and articulate UG (see Pinker & Jackendoff [2005]), but on the other hand it is fully in line with minimalist thinking. A particularly welcome effect produced by this shift of focus is that we may now reassess the issue of formal similarities and dissimilarities between syntax and phonology. For

many years, the dominant view has been that syntax and phonology are fundamentally different. [...] But general principles of design may very well be active in syntax and phonology in similar ways.” (van Riemsdijk 2008:227)

Given what we now understand about syntactic architecture in light of Minimalism, investigating phonology from this perspective lays the groundwork for testing and refining the arguments made by Bromberger & Halle (1989) in support of the view that phonology is fundamentally different from syntax (contra van der Hulst 2005, Anderson 2006). Such work allows us to focus not on the question of *whether* phonology is different, but rather *how* it is different and *why* this is the case. If Minimalists are correct to emphasize the role of the ‘Third Factor’ (general principles of good design) in the architecture of grammar (Chomsky 2005), then this should be a fruitful endeavor.

I also want to explore the idea advanced in many recent Minimalist writings that phonology is an ‘ancillary’ module, and that phonological systems are “doing the best they can to satisfy the problem they face: to map to the [Sensory-Motor system] interface syntactic objects generated by computations that are ‘well-designed’ to satisfy [Conceptual-Intentional system] conditions” but unsuited to communicative purposes (Chomsky 2008:136). Phonology is on this view an afterthought, an externalization system applied to an already fully-functional internal language system. While some (e.g., Mobbs (2008)) have taken this to suggest that phonology might be messy, and that we shouldn’t expect to find evidence of ‘good design’ in it, there is another perspective which suggests instead that the opposite conclusion is warranted: phonology might be much *simpler* (less domain-specific) than has previously been thought, mak-

ing use of only abilities that already found applications in other cognitive domains at the time externalized language emerged. Throughout the rest of this dissertation, I establish what I take to be the formal properties of these operations and representations, with the aim of showing that they are all present elsewhere in the animal kingdom and, plausibly, in our first speaking ancestors.

This view accords with the evolutionary scenario developed by Hauser et al. (2002a) and Fitch et al. (2005), who suggest that language may have emerged suddenly as a result of minimal genetic changes with far-reaching consequences (cf. Pinker & Jackendoff (2005) and Jackendoff & Pinker (2005), who see language as manifesting complex design).² Particularly relevant is the distinction that Hauser et al. (2002a) make between the ‘Faculty of Language - Broad Sense’ (FLB), including all the systems that are recruited for language but need not be unique to language, or to humans, and the ‘Faculty of Language - Narrow Sense’ (FLN), which is the subset of FLB that is unique to our species and to language. At present, the leading hypothesis among proponents of this view is that FLN is very small, perhaps consisting only of some type of recursion (i.e., Merge) and the mappings from narrow syntax to the interfaces. Pinker & Jackendoff claim that phonology constitutes a major counterexample to this hypothesis. I argue that if the theory advanced in this thesis is even close to correct, then this criticism of Hauser et al. is unfounded.

²The relation of Hauser et al.’s claims to the Minimalist Program is somewhat controversial, and the authors themselves claim that the two are independent. At least from my personal perspective, they are two sides of the same coin.

2.3 Galilean phonology

The Minimalist Program, like its predecessors in generative grammar, is Galilean in character.³ Chomsky describes the Galilean style as follows:

“[T]he Galilean style... is the recognition that it is the abstract systems that you are constructing that are really the truth; the array of phenomena is some distortion of the truth because of too many factors, all sorts of things. And so, it often makes good sense to disregard phenomena and search for principles that really seem to give some deep insight into why some of them are that way, recognizing that there are others you can’t pay attention to.” (Chomsky 2002:98)

While the Galilean style does not entail the Strong Minimalist Thesis *per se*, and indeed Chomsky wrote of a Galilean-style linguistics in *Rules & Representations* (1980), it does strongly suggest ontological minimalism, and it certainly encourages the quest to go beyond explanatory adequacy. It also foregrounds the study of linguistic competence, as disentangled from interfering factors of performance (memory limitations, muscle strength, mood, barometric pressure, and so on *ad infinitum*). That is not to say the latter cannot or should not be studied—quite the opposite. They are just outside the domain of the linguist.

I stress that this is not meant to be a free pass to ignore difficult data, and I do not intend to use it as such. Rather, it lies behind my rejection of the “knee-jerk reaction to recalcitrant data: expansion of the computational power of the phonology” (Hale & Reiss 2008:257). I agree strongly with the continuation of the above passage: “The best science, in our view, results when, rather than bloat theories with machinery which makes possible highly accurate data-matching, we adopt a critical attitude

³Or Copernican, or Keplerian; see Boeckx (2006), §4.1.

towards the alleged data itself. Does it truly fall within the purview of phonology as computation?" Embick (2008) calls the common criticism of this view the 'Putative Loss of Generalization' argument: the type of theory for which I argue (a 'Localist' theory as opposed to a 'Globalist' one, in Embick's terms) cannot *explain why*, for example, a particular allomorph appears in one context as opposed to another. But Embick argues on the basis of extensive morphological evidence that Localism is vindicated from an empirical standpoint—Chapter 5 of this dissertation supports and builds on this conclusion—and makes the more theoretical argument that

“The idea that Localist theories have nothing to say about patterns of distribution is slightly misleading. A more accurate way of making the point is that the Localist theory cannot state *within the grammar* that the distribution of allomorphs is the way it is *because* surface phonological properties is (sic) optimized. There is an important point here that is often overlooked. It is not true that a Localist theory cannot be connected with any explanation of allomorph distributions; it can. However, it would assign the explanation of the putative generalizations about distribution to another part of the theory of language in the broad sense; after all, not every generalization about language is a generalization about the grammar. The net result of this line of reasoning is that the Localist view does not assert that there are *no* generalizations about how allomorphs are distributed in surface forms; rather, it holds that if there is something to be said about why some distributions (and not others) are found, these generalizations fall under the purview of diachrony, acquisition, phonetics, processing, etc., in some combination perhaps. Analyzing a generalization in these terms does not exclude it from principled explanation.” (Embick 2008:82)

Substituting the more general heading '(morpho)phonological alternations' for 'allomorph distributions,' the above quote is essentially a summary of the view Hale & Reiss express in many works, that the object of phonologists' study should be (or more to the point, *is*) a system of abstract symbolic computation, divorced from phonetic content. This has come to be known as the 'substance-free' approach. I

advocate for substance-free phonology as a partner to Minimalism; the substance-free view makes it possible to approach phonology ‘from bottom-up’ in Chomsky’s sense. In the sections that follow, I introduce substance-free phonology, which is itself more a program than a theory, and the third member of the triad on which this dissertation is based: Evolutionary Phonology (Blevins 2004).

2.3.1 Substance-free phonology

The substance-free approach gets its name from Hale & Reiss (2000a,b), in which it is argued that phonologists must stop the practice of “substance abuse,” or misguidedly mixing the study of phonological form with the properties of phonetic content.⁴

As summarized by Reiss (2008a:258-259),

“[t]hese works conclude that the best way to gain an understanding of the computational system of phonology is to assume that the phonetic substance (say, the spectral properties of sound waves, or the physiology of articulation) that leads to the construction of phonological entities (say, feature matrices) *never* directly determines how the phonological entities are treated by the computational system. The computational system treats features as arbitrary symbols. What this means is that many of the so-called *phonological universals* (often discussed under the rubric of markedness) are in fact epiphenomena deriving from the interaction of extragrammatical factors like acoustic salience and the nature of language change. Phonology is not and should not be grounded in phonetics since the facts which phonetic grounding is meant to explain can be derived without reference to *phonology*.”

In short, the goal of substance-free phonology is to determine the nature of the universal core of formal properties that underlie all human phonological systems,

⁴It is interesting to note that other cognitive scientists, such as Kaplan (1987 [1995]) and Pylyshyn (2003), also caution against “the seduction of substance” in their fields (computational linguistics and vision, respectively).

regardless of the phonetic substance or indeed of the modality by which they are expressed.⁵ This stands in stark contrast to the practice of ‘phonetically-grounded phonology,’ proponents of which maintain exactly the opposite: that phonological patterns result from articulatory and perceptual phonetic factors which should be directly encoded into the grammar (in recent years, most often as teleological constraints; see, e.g., the contributions in Hayes et al. (2004)).

The debate between substance-free and grounded approaches to phonology stretches back almost a full century, with the former originating in Ferdinand de Saussure and Louis Hjelmslev’s insistence on the arbitrariness of linguistic signs, and the latter in the ideas of Nikolai Trubetzkoy.⁶ The history of these divergent approaches up to the present day is chronicled in Morén (2007a); I will review some of it within the larger context of the formalism vs. functionalism debate later in this chapter. Within the present-day substance-free program, too, multiple different theories are being explored. As Blaho (2008) notes, there are (at least) five variations on substance-free phonology currently practiced. I refer the reader to §1.2 of Blaho’s dissertation for a list of representative publications in these various approaches and discussion of how they differ. What all these theories, and the one presented in this dissertation, share is the following set of assumptions:

⁵I readily admit, though, that ‘substance-free’ also has its limits. That is to say, human physiology bounds the possible forms which linguistic expression can take. It is difficult to see, for example, how one might produce and perceive language using the olfactory senses.

⁶Nevertheless, Trubetzkoy (1939) famously wrote that “phonetics is to phonology as numismatics is to economics.”

- (1) The common basis of substance-free phonology (from Blaho 2008:2)
- Phonology refers to the *symbolic computational system* governing the *signifiant*, i.e., the non-meaningful level of linguistic competence. Phonology is taken to be *universal*— common to all (natural human) languages and all modalities —, and *innate*. Phonological knowledge is part of UG, but phonetics is not.
 - Phonological primes are substance-free, in that their phonetic interpretation is invisible to phonology, and thus does not play a role in phonological computation.
 - Markedness and typological tendencies (in the sense of Greenberg (1957, 1978)) are not part of phonological competence, but rather an epiphenomenon of how extra-phonological systems such as perception and articulation work.

The theory to be presented here most closely follows arguments expressed in Hale & Reiss (2000a,b) and in subsequent individual and collaborative work by these authors. Hale & Reiss (2008) provide an excellent book-length introduction to ‘the phonological enterprise’ as they (and I) see it. I briefly summarize some arguments made in these various works below.

One of the most salient points to my mind concerns the nature of what a theory of UG, and of phonological UG in particular, should seek to explain. Hale & Reiss (2008:3) set up the following hierarchy:

- (2) ATTESTED \subset ATTESTABLE \subset HUMANLY COMPUTABLE \subset STATABLE
- a. Attested: Cree-type grammars, English-type grammars, French-type grammars
 - b. Attestable: “Japanese” in 200 years, Joe’s “English”
 - c. Humanly computable: $p \rightarrow s / _ r$
 - d. Statable: $V \rightarrow V$: in prime numbered syllables: paka₂nu₃tipa₅forse₇ \rightarrow paka:nu:tipa:fose:

Clearly, the set of attested grammars is inappropriately small: it is, I hope, uncontroversial that the list of attested languages does not exhaust the possibilities provided by UG. Conversely, the set of statable languages is far too large: it seems like a pretty safe bet that no grammars refer to the set of prime numbers, or the sign of the Zodiac at the time of the utterance, or whether the interlocutor owns any blue collared shirts, etc.

The more pressing question is whether it is correct for a theory of UG to zero in on the set of attestable languages, or the humanly computable ones. In fact, it seems that many phonologists would argue that the subset relations I listed above are simply not true. One argument put forth by proponents of Optimality Theory (Prince & Smolensky 1993 [2004]) is that the ‘factorial typology’ generated by a free ranking of violable universal constraints represents an advantage over the “typological overkill” of rule-based theories, because the former more closely mimics the set of attested languages. As far as I can tell, the target for factorial typology is really intended to be the set of attested languages *as opposed to* the set of attestable ones. The claim is a more subtle (but in my opinion equally unfortunate) one, that the set of attested

grammars *is equivalent to* the set of attestable ones, and furthermore that this set is also equivalent to the set of humanly computable ones (see discussion of McCarthy (2002) by Odden (2003)). This is totally incompatible with the substance-free approach, which emphasizes the necessity of accounting for all humanly computable languages, even if they never arise in nature; we will return to this issue shortly. Put more strongly, we hold that the idea “that every possible [grammar] should be instantiated by some attested language... is naïve, just as it is deeply naïve to expect that all logically possible permutations of genetic material in the human genome are actually attested in individual humans” (Vaux 2008:24). In Newmeyer’s (2005) terms, we maintain that synchronic phonological theory should characterize only the set of *possible* languages, not *probable* ones. As Newmeyer (2005:174) explains, “it is not within the purview of generative grammar per se to account for typological generalizations.” Note that this is not in fact a quirk of the substance-free approach, but rather is a goal common to those who pursue the biolinguistic research program. Quoting Hornstein & Boeckx (2009:81), “the aim of (bio-)linguistics is (at least) to uncover and refine [the laws of the faculty of language], laws that define the class of possible (I-)languages (not the probable ones).” Specifically with respect to phonology, this means that the biases typically attributed to formal markedness should be explained by reference to properties of our perception and production systems, and to sheer accidents of history; this shifts much of the burden of explaining typological generalizations to the theory of sound change. We will discuss markedness in the next section and diachronic phonology in §2.3.3.

Before turning to these issues, though, I would like to address the status of rules and constraints in synchronic phonological theory. This has been a contentious issue since the early 1990's, when Optimality Theory took the phonological community by storm. I do not intend to argue against Optimality Theory in any depth here; plenty of others have already done so, on multiple conceptual, computational, and empirical grounds.⁷ It is also important to note that the question of whether phonological competence is best modeled using ordered rules or constraints (or a mixture of the two), and furthermore whether any constraints to be used should be violable or inviolable (or, again, a mixture of the two), is totally orthogonal to the question of substance-free vs. phonetically-grounded phonology. For example, Reiss (2008a) argues for a completely rule-based, substance-free approach; Blaho (2008) argues for a substance-free Optimality Theory; Vaux (2008) argues for a substance-free (or nearly so) combination of mostly rules with some inviolable constraints; Calabrese (1995, 2005) combines inviolable constraints (which refer to substance) with repair rules.⁸

It has been claimed that rules and constraints are both propositional, and therefore logically equivalent (Mohanan 2000). This would seem to make the rule vs. constraint debate moot. However, this belies the fact that rules and constraints, as employed in the phonological literature, are very different objects. I take as my starting point arguments from Reiss (2008a) and Chapter 8 of Hale & Reiss (2008).

⁷For a fairly comprehensive overview of these arguments, see Vaux (2008).

⁸This is still another family of loosely related approaches which I am setting aside completely here: Government Phonology, Dependency Phonology, Lateral Phonology, Radical CV Phonology, and Minimalist Phonology, to name a few. The proposal advanced in this dissertation has also been influenced in certain respects by this literature, as will be discussed in the relevant places.

Take a typical constraint. It states a condition, for instance: “don’t end a word with a consonant.” The input string is evaluated according to a procedure which determines whether the input matches the structural description of the constraint (in this case, whether it ends with a consonant) and then maps to one of the states {YES, NO} accordingly.⁹ A rule goes through the same mapping procedure but adds another step: if the result is YES, part of the input is re-written; if the result is NO, nothing happens. There is a commonality, then, between rules and constraints: both begin with the same initial step, a mapping from an input to {YES, NO}. But rules go one step further, integrating a repair contingent on the YES output, so they are essentially functions from one representation to another. (We return to this issue, and the precise formalization of rules, in Chapter 4.)

In contrast, the violation of a constraint says to the computational system: “this representation is ill-formed.” I summarize here what I take to be two of the main criticisms of constraints made by Hale & Reiss (we will see a third in the next section). First, we do not want to (and more to the point, *could not* even if we did want to) incorporate every single way a representation can be ill-formed into the grammar, particularly if that grammar is supposed to be innate and universal, because a linguistic representation can be ill-formed in an infinite number of ways. That is, a grammar should be stated positively, not negatively. The second argument resonates deeply with Minimalism. This is that even positive, inviolable constraints like “all

⁹I set aside the fact, noted by Hale & Reiss, that in practice some Optimality-Theoretic constraints are stated positively, e.g., “have an onset.” This requires adding a second step to the evaluation procedure which maps the output YES/NO to VIOLATION/NOVIOLATION depending on whether the constraint is positive or negative, but the arguments presented in the main text still hold.

branching is binary” are superfluous because they merely restate descriptive generalizations about the interaction of linguistic primes and structure-building operations. The guiding principle here, which I take to be paramount, is that constraints should always have theorematic status: we should seek out the deeper principles of grammar from which such generalizations arise, as emergent phenomena.

Part of the substance-free view is that such generalizations—particularly the ones which are not exceptionless, but rather strong tendencies—emerge from properties of the para-/extra-linguistic systems with which phonology interfaces, as well as properties of UG itself. In particular, we advocate for the view that biases in the perception and production systems mean that various phonological systems (rules, contrasts, etc.) are harder than others to learn, and that over time, the more easily learnable systems gain ground on the less learnable ones. Arguments that ‘markedness’ is a reflection of these phonetic biases, and therefore need not be stated in the grammar, have been articulated by Ohala (1981 et seq.) and by Blevins (2004 et seq.). In the next section we discuss markedness more thoroughly, and then in §2.3.3 turn to Blevins’ theory, Evolutionary Phonology, more specifically.

2.3.2 Markedness as a performance phenomenon

As mentioned in the previous section, one of the foundational ideas which characterizes the substance-free approach to phonology is that markedness is part of performance rather than phonological competence. We argue, in other words, that markedness is an e-language phenomenon rather than an i-language one, and as such properly lies outside the realm of theories which intend to model synchronic phonology

as manifested in an individual language user's grammar.

Since the introduction of the concept of markedness by Prague School linguists, and in particular Jakobson and Trubetzkoy, many different ideas about what exactly it means for a particular linguistic object or construction to be 'marked' have been proposed. I refer the reader to Battistella (1996) for a comprehensive overview of the historical use of this term, but I will mention at least a few different views here.

- (3) Six roles of markedness (based on Haspelmath 2006:41ff)
 - a. Prague School: Markedness is language-specific
 - b. *SPE*; (most) Optimality Theory: Markedness is innate and part of Universal Grammar
 - c. Greenberg: Markedness is purely for linguists' convenience and does not describe speakers' competence
 - d. Natural Morphology: Markedness is neither part of Universal Grammar nor particular grammars, but is explanatory (and must itself be explained by disciplines other than linguistics)
 - e. Battistella (and many others, beginning with Jakobson): Markedness is ubiquitous in human culture, not just language
 - f. Markedness is sometimes also used (by linguists) in a non-technical sense, e.g., 'marked' simply means 'unusual'

To this we can also add a long list of diagnostics from a variety of different areas which have been used (alone or in combination) to determine which member of a particular phonological opposition is marked:

“Phonetic instability, articulatory simplicity, perceptual salience, neutralization, epenthesis, assimilation, segment deletion, distribution, structural complexity, language acquisition, sound change, creole genesis, cross-language frequency, and implicational relations” (Hume 2004:2)

There are several problems with entertaining so many diverse definitions and diagnostics of markedness within a single discipline. The Haspelmath and Hume papers cited above are particularly illuminating with respect to this issue. Haspelmath categorizes the senses of markedness found in linguistic literature into four major types: markedness as complexity, markedness as difficulty, markedness as abnormality, and markedness as a multidimensional correlation. He makes the important point that, no matter which sense of the term one uses, markedness always *demands* rather than *provides* explanation—and it is explainable by other principles in all cases. For example, markedness as phonetic difficulty demands explanation in terms of the human articulatory and/or perceptual systems; markedness as structural complexity demands explanation in terms of the added demands which complexity makes on linguistic computation (memory limitations, etc.). So when we speak of markedness, we are really using shorthand for a number of deeper factors which are in large part extralinguistic.

Moreover, as Hume has noted, markedness as currently applied in mainstream phonology seems paradoxical: on the one hand, sounds with low perceptual salience (few acoustic cues) are targeted for assimilation, by which criterion they should be considered unmarked. But on the other hand, sounds which have *high* perceptual salience are also considered unmarked on the basis of other criteria. More concretely, epenthetic segments are considered unmarked, but so are segments which have a

propensity to delete (see Vaux & Samuels (2003) and discussion later in this section). Hume attempts to resolve this paradox by arguing that the criteria which supposedly diagnose markedness in all its various guises are actually characteristic of another property entirely: the predictability of the category in question. This may well be correct, but I would still argue that predictability is itself a symptom of multiple underlying causes; Hume only pushes the problem back. Regardless of whether the cover term we use is ‘markedness’ or ‘predictability,’ we must recognize that there are deeper principles at work.

The discussions in Chapter 7 of Hale & Reiss (2008) and §3 of Hale & Reiss (2000b) provide good summaries of the position which I adopt. Hale & Reiss (2000b) quote from the beginning of *SPE*'s Chapter 9, which introduces a theory of markedness (developed more fully in Kean (1975)) to address a “problem” in the earlier chapters:

“The problem is that our approach to features, to rules, and to evaluation has been overly formal. Suppose, for example, that we were systematically to interchange features or to replace $[\alpha F]$ by $[-\alpha F]$ (where α is +, and F is a feature) throughout our description of English structure. There is nothing in our account of linguistic theory to indicate that the result would be the description of a system that violates certain principles governing human languages. To the extent that this is true, we have failed to formulate the principles of linguistic theory, of universal grammar, in a satisfactory manner. In particular, we have not made use of the fact that the features have intrinsic content.” (Chomsky & Halle 1968:400)

The addition of Chapter 9, which introduces substance in the form of markedness statements, in effect takes *SPE* from a theory of humanly computable languages to at-testable ones. Hale & Reiss (2000b:163) argue that “switching the feature coefficients as described [above] might lead to the description of systems that are *diachronically* impossible human languages (ones that could never arise because of the nature of lan-

guage change), but not to ones that are *computationally* impossible.” Practitioners of substance-free phonology take this dichotomy very seriously. From our point of view, the task undertaken in *SPE*’s Chapter 9 was unnecessary and indeed fundamentally misguided; a theory of *phonological competence* should not incorporate typological patterns resulting from phonetics. In §2.3.3 I describe the non-teleological theory of sound change which I believe best accords with this view.

Another problem with building markedness into Universal Grammar is framed by Hale & Reiss as an argument against Optimality Theory but I think the point is a larger one. They note that if the consequence of a constraint violation is only that the resulting structure is ‘marked’ (ill-formed in a relative, but not absolute, sense), as in Optimality Theory, and if that constraint is innate, then it is either misleading or irrelevant to a child learner:

“Equipped with an [Optimality Theory]-type UG, a child born into a Standard German-speaking environment ‘knows’ that voiced coda obstruents are ‘marked’. However, this child never needs to call upon this knowledge to evaluate voiced coda obstruents, since there are none in the ambient target language. In any case, by making use of positive evidence, the child successfully acquires a language like German. Born into an English-speaking environment, the child again knows that voiced coda obstruents are marked. However, the ambient language provides ample positive evidence that such sounds are present, and the child must override the supposed innate bias against voiced coda obstruents in order to learn English. So, this purported UG-given gift of knowledge is either irrelevant or misleading for what needs to be learned.” (Hale & Reiss 2000a:173-174)

The larger message here is that markedness is not a particularly useful concept for language acquisition; the linguistic input is actually a far better source than a rich Universal Grammar for all the information which is necessary for the child to figure out alternations or distributions which innate grammatical principles of markedness

would supposedly help her to discover.

In the remainder of this section I would like to discuss a case which I think provides another argument against the utility and adequacy of competence-based theories of markedness. The process we will investigate is consonant epenthesis; this is a widely-attested phenomenon among the world's languages. The arguments I present here largely follow the treatment of consonant epenthesis by Vaux & Samuels (2003).

The basic problem when faced with the facts of consonant epenthesis is to determine (a) what are the consonants which it is possible to insert cross-linguistically; and (b) why a particular language develops a process which inserts the particular consonant that it does. From the perspective of certain phonologists, consonant epenthesis “provides valuable insight into markedness relations” because only unmarked segments are chosen for epenthesis (de Lacy 2006:79). In contrast, the view espoused by Vaux & Samuels (and in the present work) is that consonant epenthesis stems from the re-analysis of deletion rules and does not show anything like “emergence of the unmarked.” The restrictiveness of markedness-based accounts is actually a disadvantage, because when we look at the typology of consonant epenthesis, we find that virtually anything is possible.

One approach which a number of phonologists pursued in the early 1990's, and as early as Broselow (1984), was that consonant epenthesis involves the insertion of a default coronal (coronal being the least marked place), usually [t]. However, since so many counterexamples exist, this approach has largely been abandoned. More sophisticated approaches emerged with Optimality Theory: for example, Lombardi (2002) states that the glottal stop is the most frequent epenthetic consonant, and therefore

must represent the least marked place. She posits a universally fixed hierarchy of markedness constraints which refer to place, intended to capture this fact:

- (4) *DORSAL, *LABIAL >> *CORONAL >> *PHARYNGEAL

De Lacy (2006) takes a similar approach, positing constraints which eliminate the necessity of a fixed constraint ranking (problematic because free ranking is often taken to be a fundamental tenet of Optimality Theory) by building the hierarchy of place markedness directly into his constraints. Thus, rather than having constraints which penalize each place individually, he posits a constraint *{DORSAL} alongside *{DORSAL, LABIAL}, *{DORSAL, LABIAL, CORONAL}, etc. The *{DORSAL, LABIAL} constraint penalizes both dorsal and labial segments, and *{DORSAL, LABIAL, CORONAL} penalizes both of these plus coronals; dorsals (the most marked place) violate more of these constraints than any other segments, labials (the second-most marked place) violate one constraint less than dorsals, and so on down the line. Lombardi and de Lacy's approaches are but two of the many markedness-based accounts to consonant epenthesis which have been proposed.

Epenthesis of a glottal stop in such a system is trivial. And epenthesis of [t], as in the famous case of Axininca Campa /inkoma-i/ → [iŋkomati], can be achieved through inventory constraints: Axininca inserts a coronal rather than a glottal stop because its inventory lacks /ʔ/.¹⁰ However, there are several problems with such an account. We might ask ourselves

¹⁰Constraints which penalize segments of a particular sonority being adjacent to a vowel (e.g., *FRICV) can be used to ensure that [t] is inserted rather than other possible candidates for insertion, such as [h] and [s].

“what fact other than its propensity to get inserted reflects [ʔ]’s extreme unmarkedness? This is a harder question: the standard evidence for markedness, the implicational universals, suggest otherwise: [ʔ]’s presence in an inventory is not asymmetrically implied by the presence of all other C’s, or indeed by the presence of all other members of its stricture class. [...] I conclude that there is either no constant context-free, all-purpose preference for glottal as against other stops, or, if there is a preference, it is the opposite from the one needed to predict the proper choice of epenthetic C.” (Steriade 2009:173-174)

Again we see the confusion which arises when different ostensive diagnostics for markedness yield conflicting results. As Steriade also notes, homorganic glides are more commonly epenthesized next to vowels than are glottal stops, so the whole enterprise is suspect. But what’s even worse is that the range of consonants which are epenthesized cannot be described in terms of markedness at all. Vaux & Samuels (2003; see references therein) provide a sampling:

(5) Epenthesized consonants

t Axininca Campa, Korean, French, Maru, Finnish

d a French aphasic

n Korean, Greek, Sanskrit, Dutch, Swiss German, Armenian, Mongolian,
English

ŋ Buginese, Balantak

N Inuktitut, East Greenlandic

r English, German, Kakati, Assamese, Uyghur, Basque, Japanese, Spanish

l Bristol English, Midlands American English, Reading English, Motu,
Polish, Romanian

- j Turkish, Uyghur, Faroese, Greenlandic, various Indic languages, Arabic, Portuguese, various Slavic languages
- w Guajiro, Greenlandic, Arabic, Romanian
- v Marathi, Sinhalese, Portuguese
- b Basque
- f Basque
- ʒ Cretan Greek, Mani Greek, Basque
- g Mongolian, Buryat
- s/z French, Land Dayak, Dominican Spanish, child speech
- x Land Dayak
- k Maru

Cross-linguistic notions of markedness are simply not helpful for explaining this phenomenon or others like it—virtually anything is possible, though not all outcomes may be equally probable. Phonological theory must be able to account for the entire range of epenthesized consonants, however idiosyncratic. Appealing to a unitary, innate notion of markedness severely hampers the flexibility which is necessary to achieve the necessary empirical coverage.

2.3.3 Functionalism & phonological change¹¹

If synchronic phonological theory's sole task is to describe what is a possible synchronic phonological pattern/process—in other words, if markedness is not part

¹¹This section is partially based on discussions in Samuels (2006) and Samuels (2007).

of phonological competence—then what accounts for the fact that some patterns and processes are more common than others? In the introduction of *Evolutionary Phonology* (Blevins 2004), the working hypothesis of Evolutionary Phonology is stated very clearly (pp. 8-9; emphasis hers):

“[R]ecurrent synchronic sound patterns have their origins in recurrent phonetically motivated sound change. As a result, there is no need to directly encode the frequent occurrence of these patterns in synchronic grammars themselves. Common instances of sound change give rise to commonly occurring sound patterns. Certain sound patterns are rare or unattested, because there is no common pathway of change which will result in their evolution.”

Anderson (2009:807) succinctly describes the implications of such an approach for the locus of explanation when it comes to phonological ‘universals,’ or strong tendencies:

“Explanations of this sort do not depend on properties of the Language faculty in any essential way, and to the extent they can be generalized, deprive us of a basis for inferring properties of that faculty from phonological universals. On this view, the locus of explanation in phonology shifts from synchronic structure to diachrony, more or less as our neogrammarian ancestors told us. The regularities we find are regularities of the input data, as shaped by factors of phonetic production and perception in the operation of linguistic change. . . .”

The substance-free approach to phonology holds that being ‘deprived’ of this source of information about Universal Grammar is actually a good thing, because “many of the so-called *phonological universals* (often discussed under the rubric of markedness)” are not exceptionless, and “are in fact epiphenomena deriving from the interaction of extragrammatical factors such as acoustic salience and the nature of

language change” (Hale & Reiss 2000a:167).¹² Thus, the theory of diachronic phonology must play a more active role in explaining typology. The exact implementation of both the diachronic and synchronic theories is negotiable; the former is the subject of this section, and the latter will occupy us for the rest of this dissertation.

While I question certain aspects of Evolutionary Phonology, as will soon become clear, I fully support an approach to phonological typology which does not include teleological concepts in linguistic competence and which emphasizes the role of diachronic tendencies over grammatical constraints. The question of whether it is appropriate to apply evolutionary theory in such a theory of diachronic phonology, as Blevins notes, is rooted in the dialogue between formalism and functionalism that is in fact older than generative linguistics (this dialogue is, I would add, the same from which the substance-free and phonetically-grounded approaches emerged). This formalist-functionalist debate has occupied several generations of linguists, at least as far back as Edward Sapir and Trubetzkoy. And still today, even within the Optimality Theory paradigm, which has come to be associated with phonetically grounded, functionalist phonology, there exist a wide range of views, from those who happily employ

¹²Though one of the arguments for Evolutionary Phonology is that so-called phonological ‘universals’ typically have exceptions, I want to make clear that the presence of such exceptions is merely a *clue* that we should be looking to extragrammatical factors for an explanation of such tendencies; even exceptionless generalizations may not warrant grammatical explanations. As Hornstein & Boeckx (2009:81) write, when we turn our attention to true ‘I(nternalist)-Universals,’ or the laws of the faculty of language, as opposed to Greenbergian ‘E(xternalist)-Universals,’ “the mere fact that every language displayed some property P does not imply that P is a universal in the I-sense. Put more paradoxically, the fact that P holds universally does not imply that P is a universal.”

teleological constraints which enforce ‘preservation of contrast’ and ‘ease of pronunciation,’ to those who see Optimality Theory as bridging the formalist-functionalist gap (Darnell et al. 1998); Blaho (2008) even proposes a ‘radically substance-free’ Optimality Theory.

One of the ways in which this debate has manifested itself most clearly is in the way teleological explanations for phonological generalizations have been treated. Outside the realm of linguistics, teleology has come in and out of fashion at various times through the centuries: “for Aristotle, a non-teleological universe (even with respect to inanimate matter) was inconceivable. . . from Darwin on, teleology is pretty much anathema, or at the very least weak-minded, romantic or obscurantist” (Lass 1980:64). This is certainly not the case in the phonetically-based phonology tradition today, nor was it the case among some circles in the pre-generative era. The Prague School was particularly amenable to teleological explanation in diachronic phonology, since in their view all of language necessitated consideration from a functional perspective. Jan Baudouin de Courtenay and Otto Jespersen can be viewed as the intellectual forefathers of the functionalist movement, though it did not gain momentum until a couple of decades into the twentieth century. Roman Jakobson was one of the first linguists to develop the fledgling theory more fully, opining in a Prague Linguistic Circle paper that “the overlapping between territorially, socially or functionally distinct linguistic patterns can be fully comprehended only from a teleological point of view, since every transition from one system to another necessarily bears a linguistic function” (Jakobson 1927 [1962]:1). He also later wrote that “quand nous considérons une mutation linguistique dans le contexte de la synchronie

linguistique, nous l'introduisons dans la sphere des problèmes téléologiques" (Jakobson 1931 [1962]:218).

Such acceptance of teleological explanation was by no means the consensus outside of the Prague School, however. In other circles, the approach was met with strong criticism:

"While Jakobson's propositions diverged from the practice of other linguists in all of the major respects, this was especially true in his urging a concentration on the system of distinctive sound differences to the exclusion of other phonetic facts, and in proposing a teleological, system-determined conception of linguistic change. It is by no means clear that the latter notion ever really prevailed: while historical studies came soon to be cast in terms of changes undergone by the phonological system, the role played by the system in motivating change generally in a teleological fashion was stressed more by theoreticians... than by the mainstream of practicing historical linguists." (Anderson 1985:89)

Saussure sought to maintain a strict separation between synchrony and diachrony, a dichotomy which Jakobson rejected because it precluded the possibility of interpreting linguistic change teleologically (Anderson 1985:118). And Leonard Bloomfield, in stark contrast to Jakobson and Martinet, called teleology "a mentalistic pseudo-solution" that "cuts off investigation by providing a ready-made answer to any question we may ask" (Bloomfield 1934 [1970]:284).

The fundamental tenets of Evolutionary Phonology (Blevins 2004) resonate with arguments made by the Neogrammarians, Jespersen, Greenberg, and particularly Baudouin de Courtenay. These founding fathers of phonology were adamant that synchronic sound systems are best understood through the changes that produce them. Blevins adopts a similar view but differs from the tradition by rejecting teleology in sound change. In Evolutionary Phonology, the only goal-directed processes

that interact with pure phonological change are morphological analogy and the pressure to preserve paradigms where adhering to a regular sound change would cause paradigmatic contrasts to collapse. The elimination of teleological goals, such as ease of articulation and perceptual clarity, from the grammar is one major way in which Evolutionary Phonology differs from other theories. Adopting terminology from Newmeyer (2005), most theories on the market at present fall under the category of ‘atomistic functionalism’: they maintain a direct link between the properties of the grammar and the functional motivations for these properties. Evolutionary Phonology, on the other hand, holds to a type of ‘holistic functionalism’ in which the influence of functional motivations is limited to the language acquisition process and manifests itself in the patterns of linguistic change.

As I have already mentioned, Evolutionary Phonology provides a concrete theory of how to explain the relative frequencies of various sound patterns; it is the substance-free answer to factorial typology. In mainstream Optimality Theory, constraints on synchronic grammars and the cross-linguistically fixed rankings of such constraints serve to create a markedness hierarchy. The more marked a sound pattern, the rarer it will be. In contrast, Evolutionary Phonology treats markedness as an e-language concept belonging strictly to the domain of performance, not competence. Under this conception of phonology, because some sound changes are rare, the synchronic patterns created by those changes will also be rare. This has far-reaching consequences for synchronic phonological theory which are distinctly Minimalist in character: in short, ‘pure phonology’ shrinks considerably.

Another reason why some sound patterns are rare according to Blevins is that multiple independent sound changes must occur sequentially in order for those patterns to arise. Patterns formed by common changes or sets thereof will occur at a higher frequency than patterns necessitating rarer chains of events. The status of processes like final voicing is critical here. The Evolutionary Phonology hypothesis is that final voicing should be computationally possible, just like final *devoicing*, but only marginally attested because of the nature of the biases in perception and production which drive phonological change.

Apart from reiterating the possibility and desirability of using diachronic phonology to explain synchronic patterns, Blevins also proposes a new model of sound change itself. She holds that phonetically-motivated sound changes fall into one (or more) of three categories in the ‘CCC-model’ of Evolutionary Phonology: CHANGE, CHANCE, and CHOICE.

- (6) Evolutionary Phonology typology of sound change (Blevins 2004:32-33)
 - a. CHANGE: The phonetic signal is *misheard* by the listener due to perceptual similarities of the actual utterance with the perceived utterance.
 - b. CHANCE: The phonetic signal is accurately perceived by the listener but is intrinsically phonologically ambiguous, and the listener associates a phonological form with the utterance which differs from the phonological form in the speaker’s grammar.
 - c. CHOICE: Multiple phonetic signals representing variants of a single phonological form are accurately perceived by the listener, and due to this variation, the listener acquires a prototype or best exemplar of a phonetic

category which differs from that of the speaker; and/or associates a phonological form with the set of variants which differs from the phonological form in the speaker's grammar.

The first of these, CHANGE, covers the range of cases in which a learner mishears an utterance and treats it as a token of a different but perceptually similar utterance. An example of CHANGE that Blevins gives is the sequence /anpa/, pronounced with some degree of assimilation of the nasal to the following stop, being misinterpreted as having both surface form [ampa] and underlying form /ampa/ due to the weakness of the cues indicating the place of the preconsonantal nasal.

CHANCE changes are those in which the hearer reconstructs an underlying representation of an inherently ambiguous signal which differs from that of the speaker. A hypothetical instance of CHANCE would involve [ʔãʔ] being analyzed as /ʔa/, /aʔ/, /ʔaʔ/, or /ã/, provided this representation differs from what the speaker has in mind. Frequency guides the analysis, so less frequent sequences are less likely to be posited as underlying forms.¹³ The Feature-to-segment Mapping Principle, a property of the acquisition process which produces anti-identity (Obligatory Contour Principle) effects, also affects CHANCE: the Feature-to-segment Mapping Principle leads the learner to assume a single source for a single phonetic feature, disadvantaging a multiple-source analysis like /ʔaʔ/. The result of CHANCE is imperceptible, entailing no immediate

¹³Language-specific constraints, which themselves must be learned, come into play here in the Evolutionary Phonology model. I do not see how, on this view, the phonologist (or the child) can determine when to posit a constraint and when doing so would be redundant restatement of a generalization which emerges from the data. As discussed earlier, I posit a constraint-free model.

change in pronunciation.

CHOICE, on the other hand, produces tiny shifts in pronunciation akin to those documented in the Labovian tradition. When there are multiple variants of an utterance in circulation and the hearer adopts a phonological representation or “best exemplar” that differs from the speaker’s, this is an instance of choice. Upon hearing [kkáta] in alternation with [kǎkáta] and [kakáta], a listener could assume underlying /kkáta/ and an epenthesis rule, rather than the speaker’s underlying /kakáta/ with a vowel shortening/deletion rule. In none of these three types of sound change do we see ease of articulation or ease of pronunciation directly influencing the direction of change. Instead, like markedness, these are taken to be emergent properties.

Evolutionary Phonology’s CCC-model of sound change feels intuitive in some respects. It is hard to argue that mechanisms like CHANCE, CHANGE, and CHOICE do not play any role in sound change. However, it is less clear that they are the only players: explaining how these mishearings of individual words eventually explain Neogrammarian-style exceptionless sound change would not be a trivial task. It is not enough simply to say that completed sound changes undergo lexical diffusion (Blevins 2004:260). Nor is it readily apparent that distinguishing among these particular three categories elucidates anything. There seems little hope of ascertaining which ‘C’-process(es) are responsible for producing a specific change, either in principle or in practice. And if this cannot be done, then the categories are deprived of utility or individual character.

There is another dichotomy emphasized in Evolutionary Phonology, the distinction between ‘natural’ and ‘unnatural’ or ‘crazy’ phonology, which I feel could use

clarification. On several occasions Blevins switches between discussion of unnatural rule types and unnatural sound patterns, which are quite separate matters. A strange historical development can in theory give rise to a well-behaved synchronic system, just as one or more natural phonological changes in the history of a language can produce sound patterns that seem unusual. Blevins (2004:67) lists the following diachronic developments as potential sources of phonetically unnatural sound patterns:

(7) Four potential sources of phonetically unnatural sound patterns

	Original sound change	Subsequent development
a. analogy	*XaY > XbY	a or b extended to new environments on the basis of non-phonetic factors
b. rule inversion	*XaY > XbY	$b \rightarrow a / \sim X _ \sim Y$
c. rule telescoping	*XaY > *XbY > XcY	$a \rightarrow c / X _ Y$
d. accidental convergence	various	surface pattern is generalized

In the discussion that follows, Blevins writes that the “[t]hough the majority of work in phonological theory from the mid-1980s forward makes no principled distinction between natural and unnatural rule types, this contrast is central to Evolutionary Phonology” (p. 71). But a few pages later she opines that distinguishing between natural and unnatural sound patterns “seems unwarranted and indeed misguided” (p. 78). I want to stress that in the substance-free approach, there is no such thing as an ‘unnatural’ rule or pattern from a computational point of view. This is supported by the fact that “there is no independent evidence that ‘unnatural’ phenomena are treated any differently by speakers vis-à-vis common phenomena” (Mielke 2008:28), although the former have been shown to be more difficult to learn than the latter (Saffran & Thiessen 2003, Wilson 2003). A given pattern is either generable by some combination of phonological primes and abstract computations over those primes, or

it is not. The important point about ‘natural’ and ‘unnatural’ rules and patterns is that both exist—it is undeniable that phonological systems are full of arbitrary patterns, and the debris of historical accidents—and that whatever theory of representations and operations we adopt must be able to account for this.

Also relevant to the work undertaken here is the discussion of evolutionary terminology in the first pages of *Evolutionary Phonology*, and which gives the theory and the book its name. Blevins makes several mentions of Darwinian principles such as adaptation and in many cases utilizes language and reasoning that would not seem out of place in the evolutionary biology literature. However, she cautions that parallels to Darwin are necessarily “largely metaphorical” because phonological systems are learned, not transmitted in the DNA (Blevins 2004:xi). Here I think *Evolutionary Phonology* is too modest in its aims. I feel that, if we are serious about treating linguistics as “biology at a suitable level of abstraction” (Boeckx & Piattelli-Palmarini 2005:462), we should not shy away from pushing parallels between linguistic and (other) biological concepts as far as they will go.

If it is indeed possible to speak of language in evolutionary terms, what would we gain from doing so? The most significant consequence for phonology would be closing the door to (atomistic) functionalism once and for all. *Evolutionary Phonology*—building on the insights of predecessors like Ohala (1981)—provides an outline for a theory of sound change that seems both plausible from a psycholinguistic standpoint and satisfactory in terms of empirical coverage. The *Evolutionary Phonology* theory manages this while banishing markedness, ease of production, ease of perception, and other functionalist principles from phonology proper; these should be considered e-

language phenomena that are not part of linguistic competence, thus not demanding of explanation in the synchronic grammar. Instead, we shift the burden onto innate biases (in perception, production, and hypothesis-formation) which we identify in the language acquisition process. Such biases can explain why certain sound patterns are more frequent than others without the redundancy of hard-wired constraints disfavoring utterances that are difficult to produce or perceive. Eliminating these functional considerations in the grammar has the immediate advantage of slimming Universal Grammar and simplifying synchronic phonology.

Blevins presents Evolutionary Phonology as not only a full-fledged theory of sound change but also as an alternative to Optimality Theory as a synchronic theory. However, the Evolutionary Phonology theory of synchronic phonology is not concrete, and Blevins' belief that "most recurrent aspects of sound patterns found in the world's languages are encoded as language-specific synchronic constraints" is inconsistent with the arguments I summarized in the previous section, which support a constraint-free theory. In the chapters to follow, I present an alternative, substance-free synchronic theory which is consistent with the position that apparent constraints are epiphenomena of a simple set of phonological representations and operations, but which is still also based on the Evolutionary Phonology-style, non-teleological view of sound change.

Chapter 3

Representations & Constituency

3.1 Introduction

In this chapter, I undertake the task of defining the phonological representations which provide a workspace for the operations to be explicated in the next chapter. We will concentrate here on sub-word-level representations here; the issue of phonological domains which are larger than the word, and how these domains are derived from syntactic structure, will be the subject of Chapter 5.

We begin with discussion of phonological features, presenting arguments from spoken and signed languages for the emergent feature hypothesis (Mielke 2004, 2008, Morén 2007b) and providing evidence that emergent phonological categories can be learned via a self-supervised, cross-modal learning model (Coen 2006). We then discuss a closely related issue, the question of how much underspecification of featural values is allowed. I argue for a theory of ‘archiphonemic’ underspecification along the lines of Inkelas (1995), as opposed to ‘radical’ or ‘contrastive’ underspecification. This allows us to maintain a distinction between a perseverant form of underspecification that persists at all stages of the phonological and phonetic representations (see

Keating 1988, Hale et al. 2007) and a resolvable type that is potentially repaired by application of the SEARCH and COPY operations specified in the next chapter. In this next chapter we will return again to the topic of features and their organization, arguing for the algebraic approach of Reiss (2003b,c) as opposed to the feature-geometric approach (e.g., Clements 1985).

Zooming out, we next discuss the organization of segmental and suprasegmental material into strings. We focus on the idea that phonological representations are ‘flat’ or equivalently, ‘linearly hierarchical’ (Neeleman & van de Koot 2006). We compare phonological syllables and syntactic phrases, which have been equated by Levin (1985) and many others, with some even claiming that phrase structure was exapted from syllable structure (Carstairs-McCarthy 1999). I provide evidence, following Tallerman (2006), that these analogies are false, and provide evidence that many of the properties commonly attributed to syllabic structure can be explained as well or better without positing innate structure supporting discrete syllables in the grammar.

3.2 Phonological features

One of the most important advances in twentieth-century phonological theory was Roman Jakobson’s proposal that segments can be decomposed into distinctive features to which phonological processes refer.¹ Over the past fifty years, a huge number of phonological feature systems have been proposed, and debates in many areas of

¹The idea of decomposing speech sounds into features really dates back to Alexander Melville Bell’s *Visible Speech*, published in 1867, but this discovery gained little ground prior to the re-introduction of features into phonology by Jakobson in 1928. See Halle (2005) for an overview of the history of feature theory.

feature theory continue to the present day. Are features privative (present/absent), binary (+/-), or equipollent (+/-/Ø)? Are they articulatory or acoustic? Are they organized hierarchically (and if so, how)? Another set of questions concerns whether lexical entries can have featural representations which are less than fully specified, and if so, what principles govern this underspecification. Finally, there is a constellation of questions surrounding what we might call ‘applied feature theory,’ or how features can be manipulated in the phonology. We will discuss the general properties of feature systems and underspecification in the sections to follow, and much of Chapter 4 will be devoted to how phonological operations interact with the representations discussed in this chapter.

3.2.1 Emergent feature theory

As a means of approaching some of the issues just mentioned, I would like to address the following question: what are features meant to explain? The basic answer is that they are meant to capture the fact that various groups of sounds behave alike (i.e., they are all affected by or trigger a particular rule, or they are all subject to a particular distributional restriction). When one examines such groups of sounds, one finds that they typically—though not always—have in common a property which is acoustic (for instance, all the sounds’ first formants fall within a given frequency range) or articulatory (all the sounds are produced with vibration of the vocal folds).

Phonologists call these groups of similar sounds “natural classes.” The standard view, as expressed by Kenstowicz (1994:19), is that “the natural phonological classes must arise from and be explained by the particular way in which UG organizes the

information that determines how human language is articulated and perceived.” This is typically taken to mean that natural classes are defined by features which reflect phonetic properties, and those features are part of UG. By hypothesis, the phonological grammar operates over features; the sounds in question pattern together because there is a phonological process which refers to the feature which they share. Thus, an equation is made between *phonetically natural* classes, *featurally natural* classes, and *phonologically active* classes, definitions of which I provide below, from Mielke (2008:12-13).

(8) a. PHONETICALLY NATURAL CLASS

A group of sounds in an inventory which share one or more phonetic properties, to the exclusion of all other sounds in the inventory.

b. FEATURALLY NATURAL CLASS

A group of sounds in an inventory which share one or more distinctive features, to the exclusion of all other sounds in the inventory.

c. PHONOLOGICALLY ACTIVE CLASS

A group of sounds in an inventory which do at least one of the following, to the exclusion of all other sounds in the inventory:

- undergo a phonological process
- trigger a phonological process, or
- exemplify a static distributional restriction.

The main task of feature theory, then, is to find the phonetic features which accurately describe the attested phonologically active classes in the world’s languages. This goal has been met with varying degrees of success. A large-scale survey of 6,077

phonologically active classes from 648 language varieties representing 51 language families, undertaken by Mielke (2008), sought to quantify the accuracy of the major feature theories in this regard. The results of this survey show that almost one quarter of these classes do not comprise a natural class (i.e., they cannot be characterized by a conjunction of features) within any of the three theories he tested: the *Preliminaries to Speech Analysis* (Jakobson et al. 1952) system, based on acoustics; the *SPE* (Chomsky & Halle 1968) system, based on articulation; and the Unified Feature Theory (Clements & Hume 1995), also articulatory. The most successful of the three, the *SPE* model, captures just over 70% of the phonologically active classes in the survey.

One example of a featurally unnatural yet nevertheless phonologically active class comes from Japanese. This is the class of segments which undergo *rendaku* (voicing in morpheme-initial position, if there is no voiced stop in that morpheme). These *rendaku*-undergoing segments are {t, k, s, ʃ, h}. In *SPE* features, the relevant feature values for the segmental inventory of Japanese (excluding vowels and glides for simplicity's sake) are as follows (from Mielke (2008:52)):

	t	k	s	ʃ	h	p	b	d	g	z	m	n	r
VOCALIC	-	-	-	-	-	-	-	-	-	-	-	-	+
VOICE	-	-	-	-	-	-	+	+	+	+	+	+	+
NASAL	-	-	-	-	-	-	-	-	-	-	+	+	-
ROUND	-	-	-	-	-	-	-	-	-	-	-	-	-
SYLLABIC	-	-	-	-	-	-	-	-	-	-	-	-	-
LONG	-	-	-	-	-	-	-	-	-	-	-	-	-

The reason why this class is featurally unnatural should be immediately obvious from this chart. *Rendaku*-undergoing segments could be characterized as the conjunction of [-VOCALIC, -VOICE, -NASAL, -ROUND, -SYLLABIC, -LONG], but for one problem: /p/

shares all these values but does not participate.² The lesson to take away from this is that phonologically active classes are not always amenable to traditional featural descriptions, and what's more, fifty years' worth of proposals for different feature sets have not improved the empirical coverage of these difficult cases by much. In short, the explanation for phonological patterns is not likely to lie in the traditional feature-conjunction system.

What are we to make of this? Mielke (2008:77) notes that when phonetic cues are clear-cut, all three models do well, but “[i]n the phonetic gray areas, where universal features would be expected to define clear boundaries between two values of a feature, the phonological patterning of sounds is as varied as the phonetic cues are ambiguous.” This is particularly striking in the case of lateral liquids, which are described as [+CONTINUANT] in some feature theories and [-CONTINUANT] in others, while still others treat the continuancy of laterals as varying from language to language, reflecting the fact that they pattern almost equally with continuants and non-continuants cross-linguistically.³ For example, in Basque nasals and laterals (but not rhotics) assimilate to a consonant that follows; the class of assimilating segments can therefore be characterized as [+SONORANT, -CONTINUANT]. The forms shown

²There are in fact two ways to state this class, which is crucial since it is the referent of a phonological rule. However, both necessitate increasing the power of phonology beyond the conjunction of feature values, either by allowing for the subtraction or disjunction of features. We will return to this issue in §4.4.3.

³Yip (2005) makes a closely related point, which leads her to reject feature geometry: [LATERAL] seems to be affiliated with the Coronal node in some cases, and with the Sonorant Voicing node in others.

here are from Hualde (1991:96) and reproduced by Mielke (2008:61-62).

- (10)
- | | | | | | |
|----|---------|--------|-------------|---------|-----------------------|
| a. | egu[m] | berri | ‘new day’ | | |
| b. | egu[m̥] | fresku | ‘cool day’ | | |
| c. | egu[n] | denak | ‘every day’ | ata[l̥] | denak ‘every section’ |
| d. | egu[n̥] | ttiki | ‘small day’ | ata[ɬ] | ttiki ‘small section’ |
| e. | egu[ŋ] | gorri | ‘red day’ | | |

The opposite pattern, in which the lateral liquid patterns with continuants, is found in Finnish. When a stem-final consonant is followed by an /n/-initial suffix in Finnish, one of the two adjacent consonants undergoes total assimilation: the continuant segments /s, r/ and also /l/ cause /n/ to assimilate to them, while the non-continuant /t/ itself assimilates to /n/. This is shown below with the active potential form of the verb, involving the suffix *-nut*, and the second active participle, involving the suffix *-nee*. The data are originally from Sulkala & Karjalainen (1992:87-88) and are also discussed by Mielke (2008:62).

- (11)
- | | Root | <i>Active potential</i> | <i>2nd active participle</i> | |
|----|------|-------------------------|---|--------|
| a. | pur | purrut | purree | ‘bite’ |
| b. | nous | noussut | noussee | ‘rise’ |
| c. | tul | tullut | tullee | ‘come’ |
| d. | avat | avannut | avannee | ‘open’ |

The following (adapted from Mielke (2008:60ff)) is a list of phonologically active classes involving lateral liquids. The languages listed in (12a) have processes which are featurally natural only if [+CONTINUANT] is used to describe them, and the languages listed in (12b) have processes which are featurally natural only if [-CONTINUANT] is used to describe them.

- (12) Phonologically active classes involving lateral liquids and [CONTINUANT]

- a. [+CONTINUANT]: Arabana, Arapesh, Agulis Armenian, Bearlake Slave, Catalan, Central Outer Koyukon, Doyayo (2x), Dunquin Irish, Ecuador

Quichua, Ehueun (2x), Epie (2x), Estonian, Finnish (2x), Greek, Lumasaaba (2x), Manipuri, Misantla Totonac (2x), Mising, Okpe, Onti Koraga, Navajo, Runyoro-Rutooro, Shambala, Temne (2x), Tswana, Ukué, Umbundu, Wiyot, Yucatan Maya

- b. [-CONTINUANT]: Agn Armenian, Alyawarra, Anywa, Arabana, Basque, Catalan (2x), Dholuo, Dieri (2x), Gooniyandi, Guatuso, Kolkuma Ijo, Koromfé (3x), Libyan Arabic, Mishmi, Nangikurrunggurr, Spanish, Toba, Tsakhur, Tswana, Turkish, Wangkangurru, Yir-Yoront (3x), Yucatan Maya

The reason for this, Mielke claims, is that laterals have ambiguous phonetic properties; they do not exhibit either prototypical continuant or non-continuant properties. This suggests to Mielke, and to me, that traditional, innatist feature theory has the story backwards: feature specifications *emerge from phonetic properties*, not the other way around. Of course, since an emergent feature system is constructed on the basis of phonetics, we still predict the prevalence of phonologically active classes which can be described in featural terms; however, we also treat the 25% of sound patterns which involve featurally unnatural classes as a core part of phonology. As Mielke (2008:113) emphasizes, “emergent feature theory opens up new sources of explanation in formal phonology, without losing most of the insights of innate feature theory. Emergent feature theory is not a rejection of the work of Jakobson, Halle, Clements, and many others, but a continuation of it.”

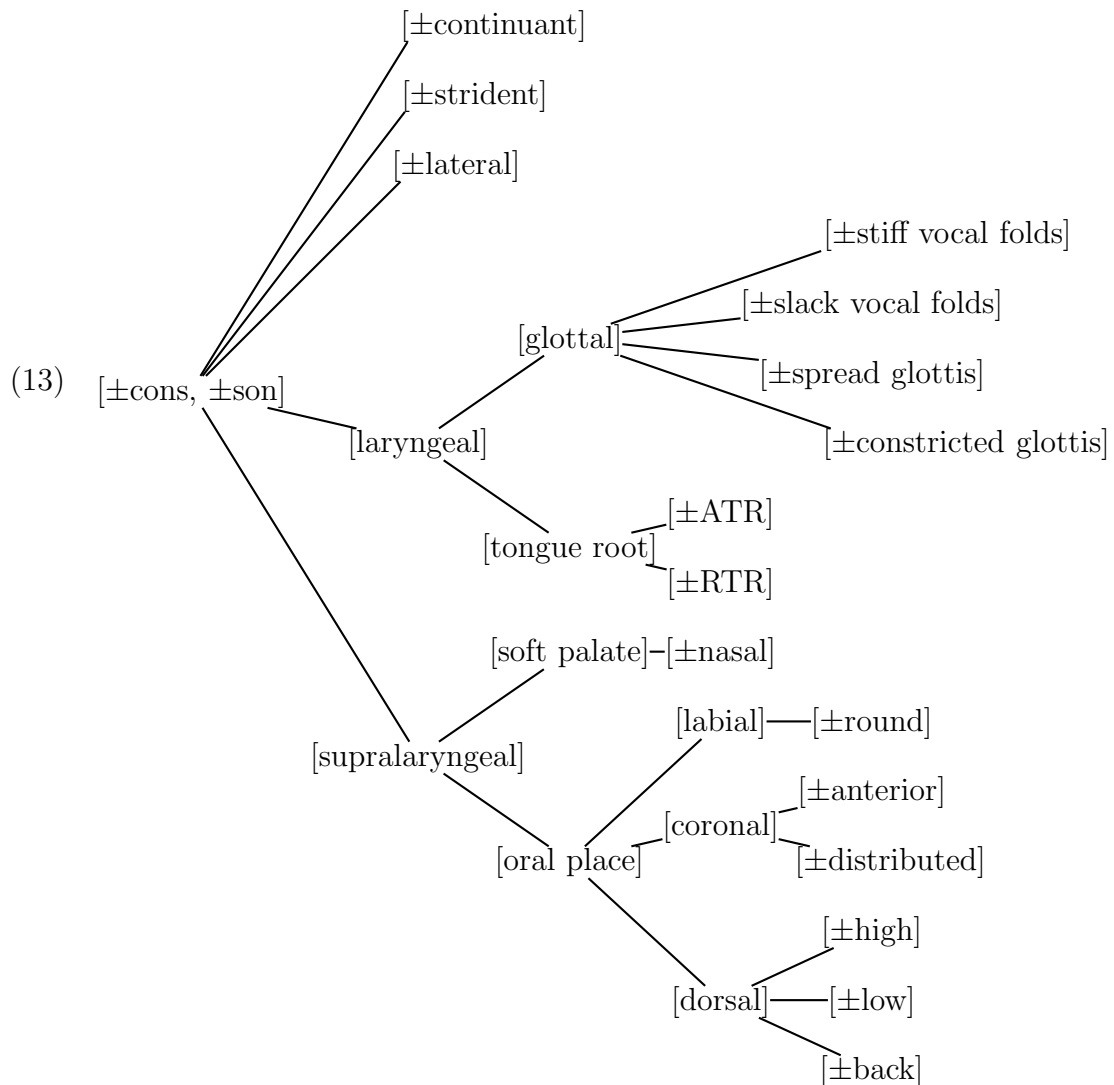
At the heart of emergent feature theory is the belief that

“the natural classes and distinctive features found in human languages can be accounted for as the result of factors such as phonetically based sound change and generalization, which can be described without reference to

a feature system. A feature system can be constructed (by a language learner or a linguist) on the basis of these results, but the feature system critically does not need to be a driving force behind sound patterns. Facts which have been attributed to innate features are accounted for by independently needed concepts (such as language change and similarity). It follows that phonological distinctive features no longer need to be assumed to be innate.” (Mielke 2008:4)

This position is clearly in keeping with the bottom-up approach to UG, the substance-free view of phonology⁴, and the Evolutionary Phonology view of sound change; emergent features should be the null hypothesis. Morén (2007b) stresses this point, using the feature geometry presented in Halle (1992) as an example (one could just as easily illustrate this with another model, such as Halle et al. (2000)):

⁴Note, however, that Hale & Reiss (2008) explicitly argue for innate features. However, as discussed in the main text, other phonologists working within the substance-free framework, such as Morén (2007b), have argued the opposite.



The geometry of this model exactly follows the organization of the human vocal tract. Why, then, should we posit an innate set of features and an innate geometry as part of Universal Grammar to explain phonological patterns, if that system only restates inevitable biological facts about our species' anatomy? There is a duplication of explanation here⁸ which ought to be eliminated. Obviously, our vocal tract anatomy is here to stay; it is innate feature theory which must go if we are going to eliminate this redundancy. But in order for emergent feature theory to be tenable, it must be demonstrated that (a) there is no evidence which can *only* be explained if

phonological features are innate, and (b) that a plausible acquisition scenario which would give rise to features can be constructed. These partially overlap, since some studies conducted on the perception of phonological contrasts at different ages have been interpreted as arguing for innate features.

Mielke's extensive survey provides a large amount of evidence from synchronic phonological patterns for point (a), showing that phonetically-based generalizations account for not only the featurally 'natural' phonologically active classes, but also for recurrent 'unnatural' patterns which have not found explanations in featural terms but can be accounted for in terms of the phonetic similarity of the segments involved. The point here is that generalizations/analogs which give rise to phonologically active classes are made on the basis of phonetics, not features: features can be used to *describe* the alternations which occur, but not to explain them directly. For example, Mielke found fourteen cases in which fricatives and sonorant consonants pattern together (Mielke 2008:125), which is totally unexpected on the basis of their feature specification. Nevertheless, a phonetic explanation is available: they are acoustically similar in that neither have release bursts or completely silent components like stops, and their amplitudes fall somewhere between stops and vowels. One case in which fricatives pattern with sonorants (here, nasals) occurs in Bukusu (Austen 1974:53ff; Mielke 2008:67). In this language, nasals delete before fricatives and other nasals, whereas in other contexts they assimilate or remain unaltered.

- | | | | | |
|------|----|---------------|-----------|-------------|
| (14) | a. | /i-n-fula/ | [e:fula] | ‘rain’ |
| | b. | /in-som-ij-a/ | [e:somia] | ‘I teach’ |
| | c. | /in-nuun-a/ | [e:nuuna] | ‘I suck’ |
| | d. | /in-pim-a/ | [empima] | ‘I measure’ |
| | e. | /in-ù[ex-a/ | [eɲèʒexa] | ‘I laugh’ |
| | f. | /i-n-goxo/ | [eɲgoxo] | ‘hen’ |

There are several other such cases of featurally-unnatural phonologically active classes: labials and velars patterning together without labiovelars, nasals and lateral liquids to the exclusion of flap, and so on. In short, as we have already discussed in the context of the substance-free approach, phonology needs to be able to state these generalizations which are not expressible in terms of a conjunction of features. We will discuss how these complex conditioning environments can be represented with a combination of SEARCH and COPY operations with feature algebra in §4.4.3.

A second piece of evidence for emergent features is that, if features are innate, we are left in a difficult position with respect to cross-linguistic typology. Large-scale studies such as Maddieson (1984) have shown that most languages use but a tiny fraction of the features which are attested cross-linguistically. We must be careful here not to make a specious argument like the one made regarding factorial typology in Optimality Theory—we certainly do not want to claim that the full range of possible feature systems should be attested—but I do wonder along with Pulleyblank (2006:20):

“Since languages use only a very small subset of the possible set of features, and since most features are indeed rarely if ever used, then how and why were such features built into UG? If learners must actively acquire a feature set, and if the feature set has no cross-linguistically consistent phonetic and phonological correlates, then does UG actually have any role to play in understanding the natural [i.e., phonologically active —BDS] classes observed in language?”

I suggest along with Pulleyblank and Mielke that the possible speech sounds are simply those which we can produce and perceive; UG has no hand in it.

A third, somewhat similar argument against the innate feature hypothesis comes from signed languages. Since Stokoe (1960), it has been widely recognized that sign

languages have phonology, and have phonological features. This underscores the modality-independence of phonology, and therefore the necessity of divorcing phonological representations and operations from phonetic substance. But features are an important point of contact between form and substance: they are the currency of abstract phonological computation, but they are also the elements which are transduced and interpreted phonetically. In light of the need to accommodate sign into the feature system, there are three options (modified from Mielke (2008:16)):

- (15) a. Maintain that features are innate, but give each feature two separate phonetic interpretations: one for spoken language and the other for sign.
- b. Maintain that features are innate and that their phonetic interpretation is always the same, but posit two sets: one for spoken language and the other for sign
- c. Allow for features and their phonetic interpretations to be learned, depending on the modality of the linguistic input

Adopting option (a) or (b) makes a strong prediction, namely that we should find a high degree of parallelism between signed and spoken language features/featural organization. But the evidence actually supports (c): studies of sign language phonology have shown that sign language features are very different from the ones posited from spoken language in several ways. First, there seem to be far more features in signed language: for example, Stokoe (1960) makes use of twelve place distinctions, and even the very conservative feature set posited for sign by Sandler & Lillo-Martin (2005) has nearly twice as many features as the *SPE* system. Secondly, the organization of these features appears to be quite distinct from geometries posited for

spoken languages (see Corina & Sagey 1989, Brentari 1998, Mielke 2008, *inter alia*). Furthermore, the features utilized in signed language seem to be learnable from facts about the articulators that are independent of language. For this reason, Corina & Sagey (1989) conclude that UG is not necessary to explain the properties of sign language features. This leaves an uncomfortable paradox: if some feature systems can be learned, why can't all of them be? Pulleyblank (2006) makes an identical point regarding spoken-language vowel systems. Only a small number of "core" features ([HIGH], [LOW], [BACK]) are used in a large number of vowel systems, while for the remainder of features (even nasality, ATR, length, roundness, etc.), less than 20% of languages utilize them. This led to the hypothesis entertained by Christdas (1988) that core features are universal while non-core features are learned. But again, if some features can be learned, why not all of them? Or if they are all universal, why are so few typically used? The emergentist position answers these questions by saying that "categories/features emerge as a result of contact with language data, and they naturally reflect the modality of the language being learned. . . . [T]he formal role of distinctive features and other primitives is the same for both modalities" (Mielke (2008:18); see also Brentari (1998)).

In the rest of this section I will focus on the matter of how plausible the learning scenario required by emergent feature theory is. I attempt to give an account of *how* phonological categories and features might emerge, and how children could come to learn them. This is crucial because the major criticisms of emergent features invoked by advocates of a fully innate feature system hinge on the argument that forming phonological categories (which we all agree must be learned to a certain extent) is

impossible without access to a complete, UG-endowed feature set (see, e.g., Hale & Reiss 2008). Though I will focus here on spoken language, the narrative for signed language would be exactly the same, modulo the different articulators and senses involved.

Let us begin by looking at the beginning of a language-learner's life. The human auditory system matures early, and many studies have shown that the youngest infants are capable of discriminating phonetic contrasts that are utilized in the various languages of the world (Werker & Tees 1984). But remarkably quickly, this power begins to wane; by six months of age, babies already exhibit a decline in their ability to discern non-native vowel contrasts, and their performance degrades with consonants not too long thereafter (Polka & Werker 1994). Learning a specific language with its particular subset of the possible contrasts seems to entail the loss of the ability to discriminate non-native contrasts (Eimas et al. 1971, Werker & Tees 1984, *inter alia*).

I believe it does not necessarily follow from this, as is commonly suggested, that children are born with a full set of phonological features and subsequently lose access to the ones which are not contrastive in their language. Importantly, it has been shown that sensitivity to the very same contrasts that are supposed to be irrevocably lost during early infancy actually remains; these contrasts are both detectable and learnable by adults under certain circumstances. Hay (2005) investigates both of these phenomena, focusing on the difference between English and Spanish speakers' perception of voice onset time (VOT) contrasts. It is known from prior studies on both perception and production that the English [\pm VOICE] contrast is served by a boundary at around +30/+35 ms. VOT (short-lag vs. long-lag), while in Spanish

the [\pm VOICE] contrast is between pre-voiced and short-lag, with few tokens being produced in the -30 to 0 ms. range. Correspondingly, English speakers perceive a category boundary at +15/+20 ms. VOT, but Spanish speakers perceive a boundary at 0 ms. instead. The English boundary/discrimination peak coincides with the positive auditory discontinuity—a bias in the auditory system, common to humans and most mammals, which produces a non-linear mapping between acoustic inputs and the percepts they produce.⁵ Auditory discontinuities seem to provide natural boundaries for speech categories (Kuhl 1993, 2000), but importantly, these are psychoacoustic biases which have nothing at all to do with human speech *per se*. Hay, confirming earlier work by Williams (1974) and Streeter (1976), shows that the areas of increased sensitivity corresponding to auditory discontinuities persist even in languages such as Spanish and Kikuyu, in which the discontinuities do not serve as speech category boundaries. What Hay found was that the same auditory discontinuities manifested in both English and Spanish speakers, but that the discrimination peaks centered on these discontinuities were of a different size and shape for the two groups of speakers when measured in both speech and nonspeech perception tasks. In other words, “the underlying perceptual mechanisms that facilitated discrimination in the first place remain intact, although sensitivities may be enhanced [by language acquisition]” (Hay 2005:103).

Further evidence comes from the fact that adults can distinguish non-native speech sounds when presented in a non-speech context. For example, Remez et al. (1981)

⁵These biases can be asymmetric; i.e., discrimination may be easier on one side of the discontinuity than the other. See Hay (2005) and references therein.

found that English-speaking adults could reliably distinguish [k] from [q] when they were told that the sounds they were hearing were produced by water dropping into a bucket (see also Best et al. (1981), Liberman (1982)). Maye (2002) raises a similar point: if discriminatory abilities take such a sharp downturn in infancy, how come early bilinguals can achieve native-like proficiency in a language to which they were not exposed during the first year of life? These observations all support the view that there is extensive ‘tuning’ of the perceptual system during infancy, and that children come to process speech in a special way as they acquire language. Still, this does not necessitate that phonological features are present from the start. Across several domains, we are beginning to discover that infants are born with generic biases which become more specific during the course of development. For instance, experiments undertaken on face perception by Pascalis et al. (2002) showed that six-month-old human infants are as good at discriminating non-human primate faces as they are at telling apart human faces. They suggest the following (references omitted):

“Our experiments support the hypothesis that the perceptual window narrows with age and that during the first year of life the face processing system is tuned to a human template. This early adjustment does not rule out the possibility that later in life individuals can learn how to discriminate a new class of stimuli on a perceptual basis. As is the case for speech perception, our evidence with face processing indicates the existence of an early tuning period that is likely dependent on experience. Although it is difficult to compare directly the tuning of speech perception with the tuning of face perception, there may be overlap between these systems. By 3 months of age infants are already relating these two types of information, as they are able to associate faces with voices. Systems for processing faces and for processing speech may thus develop in parallel, with a similar timing and a mutual influence. One possibility is that there is a general perceptuo-cognitive tuning apparatus that is not specific to a single modality and that can be described as an experience-expectant system. Alternatively, the concordance in age may simply be a developmental coincidence, thus reflecting a modality-specific,

experience-dependent process.”

While a definitive link between face processing and speech perception has yet to be established (or disproven), I would like to suggest that Pascalis et al.’s conclusions and all of the other perception studies which I mention above show exactly what we expect if children engage in the same type of category-building across multiple domains, including speech and face perception. Specifically for the case at hand, it is not necessary to invoke phonological features in the acquisition of phonological categories. Human infants are born with an auditory system which is sensitive to a variety of patterns, a subset of which are producible given the human vocal tract, and they eventually become “experts” at detecting some of these producible patterns upon repeated exposure to them. The human auditory system is largely shared with other mammals (and other, more distantly related clades), which is why many other animals can also make the category distinctions (such as VOT contrasts) utilized in our speech, as has been known since Kuhl & Miller’s (1975) pioneering work on chinchillas. Brown & Sinnott (2006), reviewing a large number of animal and human discrimination studies, found that humans and non-humans perceive similar categorical boundaries for seventeen of twenty-seven tested phonemic contrasts.

The +20ms. VOT boundary mentioned earlier, to which many mammals are sensitive, appears to be the strongest case of a speech category boundary matching with an auditory discontinuity, and recent work emphasizes that discontinuities are only part of the story; experience with structured input seems to play a major role (Kluender et al. 2006, Hay 2005). It seems that infants are born especially sensitive to contrasts which straddle auditory discontinuities (almost by definition), but as they grow and

are exposed to language, they undergo continuous cortical re-mapping which warps their perception of sound, specifically tailoring it to the input they receive. It is not the discontinuities which change—these are immutable and language-independent, being dictated purely by anatomy—but rather, the categorical perception boundaries. Category boundaries coinciding with sensory discontinuities are not only the most salient to infants but also the easiest for adults to learn, though other boundaries are also readily learnable (Hay 2005), even by language-impaired children (Wright 2006).

The cortical re-mapping hypothesis is highly plausible because we know the human sensory cortex undergoes this type of change in a number of different circumstances: for instance, when a person is blinded or deafened, the other senses can literally take over the brain areas which formerly served the now-absent sense, and the same occurs with amputees (Ramachandran & Blakeslee 1998). Learning a musical instrument which requires very fine motor control of the fingers can cause an increase in the amount of cortex associated with the digits (Elbert et al. 1995). And in oscine birds who exhibit ‘closed-ended’ song learning, we find that neurogenesis is associated with this process (see Anderson & Lightfoot (2002) §9.5.2). Birds also evidence the perceptual magnet effects characteristic of warping of the cortical map (Kluender et al. 1998). In short, the mechanism of cortical re-mapping is neither special to speech nor to our species—see Guenther & Gjaja (1996) for a wide variety of additional references supporting this point—but it creates a type of neural expertise which makes our processing of speech special.⁶

⁶Nevertheless, speech does not become *entirely* special: Hay (2005) demonstrates that linguistic experience can also affect the discrimination of non-speech sounds in certain circumstances.

Neurolinguistic studies provide an interesting piece of data, reinforcing the idea that learning a first language does involve a certain level of ‘neural commitment,’ but that this can be (at least partially) modified into adulthood, leading to the successful acquisition of non-native contrasts. It is well known that the categorical perception of speech sounds is associated with a particular electrophysiological response. Specifically, an early event-related potential (ERP) known as the mismatch negativity (MMN) is evoked at a latency of about 140-280ms. after stimulus presentation when a subject who has been accustomed to hearing one phoneme from his/her native language is then presented with a stimulus belonging to a different phoneme. It is also known that this MMN is significantly weaker when an acoustically different stimulus belonging to the same native phoneme is presented, or when a non-native phonemic contrast is tested.

A study undertaken by Dehaene-Lambertz et al. (2000) compared the performance of native Japanese and French speakers on the contrast between /ebzo/ and /ebuzo/. Under the hypothesis that language-specific phontactics affect even early speech perception, since the consonant cluster in /ebzo/ is phonotactically illicit in Japanese (but not French), the Japanese group was predicted to perceive an epenthetic vowel when exposed to that stimulus, and not to exhibit a strong MMN for /ebzo/ versus /ebuzo/. Indeed, a major effect of language is exactly what Dehaene et al. found: the French group far outperformed the Japanese. However, looking at a second ERP with 290-400ms. latency, Japanese subjects *did* show an effect of condition (i.e., different responses to /ebzo/ and /ebuzo/). Dehaene et al.’s electrophysiological evidence accords with behavioral data collected by Dupoux et al. (1999), who also tested

Japanese and French speakers on the perception of /VCCV/ and /VCuCV/ stimuli. They found that while French subjects were far better at discriminating these two conditions, Japanese speakers still performed significantly better than chance. While they reported hearing a medial /u/ in the /VCuCV/ condition 95% of the time, they only reported hearing /u/ in the /VCCV/ condition 65-70% of the time, and in an ABX task, their error rate was only 32%. In short, while infants' loss of ability to discriminate non-native contrasts is almost certainly associated with neurological changes, some ability to perceive non-native phonological patterns remains into adulthood, and there is evidence that some neural plasticity remains as well.⁷

Obviously, the Dehaene et al. study does not directly bear on the issue of whether phonological features are innate, but I believe it is still quite relevant because the arguments for the latent discriminability of non-native phonemic (featural) contrasts and phonotactics run in parallel; very similar MMN results have been obtained in both cases. Most strikingly, Tremblay et al. (1997) show that the weak MMN response to non-native contrasts can be strengthened by training: they taught native English speakers the non-native category of pre-voiced labial stops and found that after learning this new VOT boundary, the subjects generalized it to pre-voiced alveolars. This is predicted to be impossible by any account in which features become permanently inaccessible after infancy in the absence of sufficient presence in the child's input. Either the full feature inventory is innate and remains marginally ac-

⁷Interestingly, Frey et al. (2008) report that a 54-year-old man who received an allogenic hand graft 35 years after his own hand was amputated very quickly regained normal cortical responses to tactile stimulation of the transplanted hand. So in the tactile modality, like the auditory one, neural plasticity remains to a much greater extent than has typically been assumed.

cessible throughout life, or categories can always (albeit with some difficulty) be built, and features can be abstracted from these new categories. These two possibilities are admittedly extremely difficult to tease apart, but if the emergent feature set is the null hypothesis, as I have argued above, then what is important from this perspective is to show that there is an available explanation which does not require an innate feature set.

What's more, there is ample evidence for category-building and abstraction in various non-linguistic domains, non-human animals, and non-infant humans. Contrary to beliefs held by early proponents of the 'speech is special' hypothesis, categorical perception is not unique to speech. It has been demonstrated in humans for non-speech sounds, faces, and colors; it has also been shown that macaques, baboons, and mice perceive conspecific calls categorically (Cheney & Seyfarth 2007), and that crickets, frogs, blackbirds, sparrows, quail, finches, budgerigars, marmosets, and other animals also perform categorical labeling (see references in Hauser (1996) and Klender et al. (2006)). Quite relevantly to the possibility of learned/emergent phonological categories, there is both behavioral and neurophysiological evidence for categorical perception of distinctions which we know are not innate: monkeys can be trained to distinguish categories which are novel to them, such as dogs and cats (Freedman et al. 2001).

Coen (2006) develops a computational model which is meant to show how warping of the cortical map leading to categorical perception of sounds could plausibly occur. His model is based on the idea that

“in a notion reminiscent of a Cartesian theater — an animal can ‘watch’ the activity in its own motor cortex, as if it were a privileged form of

internal perception. Then for any motor act, there are two associated perceptions — the *internal* one describing the generation of the act and the *external* one describing the self-observation of the act. The perceptual grounding framework described above can then *cross-modally ground* these internal and external perceptions with respect to one another. The power of this mechanism is that it can learn mimicry... [It yields] an artificial system that learns to sing like a zebra finch by first listening to a real bird sing and then by learning from its own initially uninformed attempts to mimic it.” (Coen 2006:19)

Coen was able to demonstrate that the learning of bird songemes is possible for his cross-modally grounded category-building algorithms, and he also successfully modeled the English vowel system in this way. Even without priors such as the ultimate number of categories to be established, his artificial learner achieved a high degree of accuracy. Furthermore, in addition to the multimodal input produced by mimicry,⁸ Coen’s model can utilize input from multiple modes of external perception (in the case at hand, sight and sound). This is desirable since interplay between the senses is widespread—consider the tight relationship between olfaction and taste—but this fact is typically ignored in models of perception (see Coen (2006:19) for references).

Emphasizing the role of visual input even in ‘auditory’ speech perception explains three facts that have long been known (the first two of which are discussed in Coen (2006) §2.1): first, that watching the movement of a speaker’s lips can greatly aid comprehension; second, that speech sounds which are acoustically ambiguous can usually be distinguished by unambiguous visual cues; third, that visual input can affect

⁸Oudeyer (2006) and Guenther & Gjaja (1996) also emphasize the role of self-monitored experimentation (“motor babbling”) in connecting auditory and articulatory representations to produce phonological categories.

an auditory percept, as in the famous “McGurk Effect” auditory illusion (McGurk & MacDonald 1976), in which a subject presented with (for instance) a synchronized visual /ga/ and auditory /ba/ perceives /da/.⁹ Recent neurological studies corroborate this behavioral evidence: it has been shown that both visual and somatosensory input reaches the auditory cortical regions in macaques, and that watching lip movements produces a response in the supratemporal auditory cortex in humans (see Brosch et al. (2005), Ghazanfar et al. (2005, 2008), and references in Budinger & Heil (2006)). Also, Weikum et al. (2007) have shown that visual information alone is sufficient to allow four- to six-month-old infants to discriminate between languages.

The results Coen obtained are also consistent with those of de Boer (2001) and Oudeyer (2006), who model the emergence of vowel systems. For the present purposes, perhaps the most important feature of these three models (which differ both in their aims and in the parameters they assume) is that none of them make use of phonological features, yet they all do a very good job of approximating attested vowel systems. There is much more work to be done in this area—one obvious shortcoming of current research is that consonants need to be studied in addition to vowels—and I leave a detailed comparison of the existing models up to future research. But at the very least, it is quite suggestive that so much success can be had without positing innate features. This is consistent with the view that features are abstractions which children make over the categories they construct, that features are not involved in the category-building process itself, and furthermore that categories are made using a

⁹It is interesting to note that something similar to the McGurk Effect has been recently reported in female frogs’ perception of male frogs’ mating calls (Taylor et al. 2008).

learning mechanism which is shared by other cognitive modules and other species. As we will see throughout this dissertation, if the emergent feature view is correct, this is but one of many cases in which we find a domain-general solution being applied to the domain-specific and human-specific problem of language externalization.

I would like to conclude this section with a brief note on articulatory vs. acoustic features. One advantage of using data from multiple modalities, as Coen does, is that it allows for categories to be constructed from a mix of acoustic and articulatory properties; that is, features can be of either type. This provides a ready account for the observations made by Brunelle (2008). Brunelle obtained very interesting results from his acoustic and electrographic studies of register in Cham: different speakers appear to realize register contrasts in different ways. They utilize the various phonetic cues which distinguish registers (pitch, breathiness, etc.) to varying degrees, and moreover, some appear to use different articulatory targets for each register, rather than acoustic ones. On the basis of these data, Brunelle concluded that register contrasts cannot be the result of a universal pitch feature, but instead that learners of Cham induce different hypotheses about the phonetic correlates of the distinction made in their language. In short, the phenomenon makes sense only if learners have a much wider range of possibilities for how to realize a particular contrast than is available if features are innate and tied to articulation or acoustic properties only.

The idea that a particular feature/contrast can have multiple different articulatory or acoustic targets is not new. To take but one example, the feature commonly known as [ATR] has the following correlates in various languages:

- (16) Articulatory correlates of [ATR] contrast (Lindau & Ladefoged 1986)
- Tongue root advancement/retraction and larynx lowering/raising (Akan, Igbo, Ijò)
 - Tongue height (Ateso)
 - Phonation difference and tongue root advancement/retraction (Shilluk, Dinka)
 - Tongue height or root movement and sometimes larynx height (Luo)

Additional examples from both articulatory and acoustic properties can be found in Lindau & Ladefoged (1986) and Pulleyblank (2006).

Hall (2007:17) makes a related point with a distinctly substance-free flavor, namely that whether features refer to acoustic or articulatory properties should be immaterial to both phonology and phonetics:

“The phonological component does not need to know whether the features it is manipulating refer to gestures or to sounds, just as the syntactic component does not need to know whether the words it is manipulating refer to dogs or to cats; it only needs to know that the features define segments and classes of segments. The phonetic component does not need to be told whether the features refer to gestures or to sounds, because it is itself the mechanism by which the features are converted into both gestures and sounds. So it does not matter whether a feature at the interface is called [peripheral], [grave], or [low F2], because the phonological component cannot differentiate among these alternatives, and the phonetic component will realize any one of them as all three. In light of this, phonological features might not need names at all; for the purposes of describing the phonology of any given language, it would be possible to use arbitrarily numbered features [± 1], [± 2], [± 3], and so on.”

Throughout this dissertation, I use the names for articulatory and acoustic features that appear in the various literature from which I have obtained my examples, but

only out of convenience; the reader should feel free to substitute his or her own favorite labels when extensionally equivalent.

3.2.2 Are features equipollent or privative? (Yes.)

Regardless of whether they are innate, there are three more issues concerning features on which any phonological theory must take a stand: (a) what form(s) features take (privative/binary/equipollent);¹⁰ (b) what features/featural values are specified at what stages in phonological computations; and (c) how feature systems are organized. We will delay most of our discussion of the organization of features (i.e., feature geometry) until the next chapter (§4.4.3) and treat the remaining two issues, which are closely related, here.

In early theories (e.g., Jakobson et al. 1952, Chomsky & Halle 1968, etc.), phonological features were represented as binary oppositions. For example, Jakobson et al. (1952) introduce pairs of opposing monovalent features in their analysis of English: vocalic/consonantal, compact/diffuse, grave/acute, nasal/oral, tense/lax, and optimal constrictive/optimal stop. They then note that the number of features can be halved if each of these pairs is ‘compressed’ by allowing each feature to take two opposing values. Thus, they arrive at a binary system containing [\pm vocalic], [\pm compact], [\pm grave], and so forth.

More recently, many phonologists (see, for example, Steriade (1995)) have moved

¹⁰‘Binary’ indicates a +/- valued system and ‘equipollent’ indicates the possibility of +/-/ \emptyset values; that is, an equipollent system is essentially a binary one which allows for some degree of underspecification. When it is helpful to abstract away from this distinction, I will refer to binary and equipollent systems collectively as ‘polyvalent.’

towards systems in which features are monovalent or ‘privative,’ positing features such as [VOICE]. Others utilize a mixture of both privative and binary features; for example, Sagey (1990), Halle (1992), Clements & Hume (1995), Clements (2003) use the following mixed system:

- (17) a. [LABIAL], [CORONAL], [DORSAL], [RADICAL], [SPREAD GLOTTIS], [CON-
STRICTED GLOTTIS]
- b. [±SONORANT], [±CONSONANTAL], [±DISTRIBUTED], [±ANTERIOR],
[±STRIDENT], [±LATERAL], [±VOICE], [±NASAL], [± CONTINUANT]

While in practice it is possible to make privative and polyvalent systems work in essentially the same way, various additional assumptions are necessary within a privative system. The differences here are subtle but important. Take for example the feature(s) pertaining to tongue root positioning. It is widely recognized that phonological processes can make reference to the class of segments which are pronounced with the tongue root advanced, and those which are pronounced with it retracted. One typical process which refers to tongue root position is vowel harmony of the type manifested by Tangale, which we will discuss in more detail in Chapter 4. For present purposes, it suffices to note that the suffix vowels shown in capital letters in the left column take on the ATR value of a vowel in the root; thus, there is alternation in suffixes between [u] and [ʊ], as shown in the (a) and (b) forms below.

- (18) Tangale [ATR] harmony (modified from Mailhot & Reiss 2007:36)
- | | | | |
|----|-----------|-----------|------------------|
| a. | seb-U | [sebu] | ‘look’ (imper.) |
| b. | kɛn-U | [kɛnʊ] | ‘enter’ (imper.) |
| c. | dob-Um-gU | [dobumgu] | ‘called us’ |

It is widely accepted that the opposition between advanced and retracted tongue

root position is ‘symmetrical.’ That is, both advanced and retracted tongue root features can be active within the same language (see Steriade (1995) and references therein). So, in a privative system, one would need two monovalent features, [ATR] and [RTR]; in a polyvalent system, one would have [\pm ATR] (or equivalently, [\pm RTR]). I will use this case to illustrate several differences between the privative and polyvalent systems, all of which argue in favor of the having at least *some* polyvalent features. I am not alone on this point; for in-depth arguments in favor of equipollent features (some of which I highlight here), see Kim (2002). More cursory treatments can be found scattered throughout the phonological literature of the past sixty years.

One major flaw of privative feature systems is that there is no formal relationship between obviously paired features such as [ATR] and [RTR]. This is problematic because, for one thing, we want to capture the fact that the set of [ATR] segments is the complement set of the [RTR] segments; this follows automatically from binary features, but not from privative ones (see Adger (2008), who argues along similar lines for binary syntactic features). Not only that, but we must somehow rule out the possibility of having a segment which is specified simultaneously for [ATR] and [RTR] in the privative system, since obviously the tongue root cannot be in two places at once. This restriction is typically achieved by means of feature geometry: [ATR] and [RTR] are organized under a single branching node, with the added stipulation that no segment may contain more than one feature subsumed by that node (again, see Adger (2008) for parallel discussion on the syntactic side). In a polyvalent theory, there is simply no way to express a segment which is both [+ATR] and [-ATR].

Second, if [ATR] and [RTR] are two separate features, it is harder to express the

intuition that hypothetical Rules A and B shown below are substantively different:

- (19) a. Rule A: [ATR] → [RTR] / - [RTR]
 b. Rule B: [ATR] → [NASAL] / - [RTR]

The difference between Rule A and Rule B is more obvious when we restate them using binary features:

- (20) a. Rule A: [+ATR] → [-ATR] / - [-ATR]
 b. Rule B: [+ATR] → [+NASAL] / - [-ATR]

Rule A effects a process of regressive assimilation, and probably by anyone's standards would be considered 'natural.' Rule B is quite different: it's not obvious what the motivation for this rule would be, but whatever it is, we would not likely want to call it assimilation. In a polyvalent theory, this is difference easy to express; Rule A involves changing the value of a single feature, while the latter involves two separate features. Again, in the privative theory an appeal to geometry is necessary to make a similar distinction: Rule A swaps two features belonging to the same node, while Rule B involves two different nodes. In short, adopting a privative feature system requires an additional layer of hierarchy to express universals which are intrinsic to polyvalent features.

To clarify, I do not want to eliminate the possibility of Rule B entirely—of course, not all rules are assimilatory. Furthermore, as I argued in the previous chapter and again in §3.2.1, phonology needs the power to formulate arbitrary rules like this one. But to the extent that we do want to make a representational distinction between rules like A and rules like B, polyvalent features find support. It should be apparent by now

that a system without the added machinery of feature geometry is to be preferred, all else being equal. In fact, we will see in the next chapter when we discuss Reiss (2003b,c) that there are more reasons to reject feature geometry; it lacks the power that we need to formulate certain kinds of phonological rules. But for now, let us set this issue aside.

Another piece of evidence against (entirely) privative feature systems comes from the category of ‘exchange rules,’ introduced in Chomsky & Halle (1968). Exchange rules¹¹ are of the following type, where α is a variable ranging over the values $\{+, -\}$:

$$(21) \quad [\alpha F] \rightarrow [-\alpha F]$$

That is, the rule in (21) will take any $[+F]$ segments to $[-F]$, and simultaneously take any $[-F]$ segments to $[+F]$. It has been recognized that the existence of exchange rules requires that at least some phonological features are binary; see, for example, Kim (2002), Calabrese (2005). Vaux & Tseng (2001) and Fitzpatrick et al. (2004) give evidence for exchange rules in both synchronic and diachronic phonology and morpho(phono)logy from a long list of languages including Czech, Luo, Bashkir, Italian, Spanish, Armenian, Latin, Flemish, Greenlandic, Russian, Ainu, Nahuatl, Scots Gaelic, Vietnamese, Ponapean, Sanskrit, Tohono O’odham, Diegueno, Dinka, Margi, Yoruba, and the Nilotic and Cushitic families. Additional examples, a brief history of exchange rules, and a discussion of how they are problematic for Optimality Theory can be found in Moreton (2003), who argues along with research from the 1970’s that such processes are limited to morphology; Vaux & Tseng (2001) and Fitzpatrick et al.

¹¹These are also known as alpha rules, mirror-image rules, flip-flop rules, polarity rules, inversion rules, and complement rules.

(2004) provide numerous counterexamples.

The above discussion notwithstanding, I do not want to leave the reader with the impression that privative features have no place in phonology. The idea that *some* features might be privative is totally consistent with the theory of phonology proposed in the next chapter and beyond. This is particularly true if features are emergent: as Kim (2002) suggests, a child who receives no data to suggest that, for instance, [-ROUND] is active in her language could well end up with a privative [ROUND] feature. But the point is essentially moot. As Steriade (1995:149) notes, “this hypothesis leads to a notational variant of the claim that the feature is binary but possesses reversible markedness.” And since the substance-free approach to phonology explicitly disavows this use of markedness, there is really nothing gained either way. For this reason, I have no problem with Steriade (1995)’s claim that [NASAL], [SPREAD], and [CONSTRUCTED] are privative, but I do not want to rule out the possibility that we may someday find a language in which there is evidence for the binarity of any of these features, either. In rejecting an entirely privative system, I am primarily concerned with avoiding the problems which I have cited above, all of which arise when both members of an articulatory opposition are both phonologically active.

3.2.3 Underspecification

The next matter which needs to be discussed is whether to allow feature- or feature-value underspecification, and if so, to what degree. In the next chapter, we will connect the conclusions from this section to the phonological operations for which I will argue; for the time being, I would like to review the history of thought

concerning underspecification in phonology and argue for a variant of ‘archiphonemic underspecification’ (Inkelas 1995, Reiss 2008a), as opposed to the ‘radical’ (Kiparsky 1982, Archangeli & Pulleyblank 1994, etc.) or ‘contrastive’ (Clements 1987, Steriade 1987, Calabrese 1988, Dresher 2003, etc.) types.

Steriade (1995:114) identifies two *SPE*-era assumptions about phonological representations:

- (22) a. LEXICAL MINIMALITY: underlying representations must reduce to some minimum the phonological information used to distinguish lexical items.
- b. FULL SPECIFICATION: the output of the phonological component must contain fully (or at least maximally) specified feature matrices.

From a certain perspective, phonology exists to reconcile these two conflicting sets of needs brought by the lexicon and the externalization system: it serves to ‘unpack’ the minimal representations required by our limited memory capacity into full representations which contain all the appropriate instructions for the articulators.¹² For example, since the aspiration of stops in English is predictable on the basis of their distribution, it is not necessary to specify aspiration in the lexicon; it can be supplied by rule in the phonology. However, both Lexical Minimality and full specification have been rejected in some subsequent literature, and I think rightly so. But let me first set the scene as it was in the 1960’s, at the birth of generative phonology.

¹²See, e.g., Bromberger & Halle (1989:58): “phonology is concerned with the relationship between representations that encode the same type of information—phonetic information—but do so in ways that serve distinct functions: articulation and audition, on the one hand, memory, on the other.”

If both Lexical Minimality and full specification are taken seriously, there is really only one way to structure a phonological system: leave all predictable/redundant information out of the lexicon, and supply it anew in each derivation by means of phonological rules. This is indeed the tack that has been taken by proponents of various underspecification theories over the past half-century. For example, Halle (1959) employed feature-filling ‘segment structure rules’ (later known as ‘redundancy rules’) interleaved with feature-changing rules. On the basis of work by Lightner (1963) and Stanley (1967), this position was revised, and in *SPE* the feature-filling redundancy rules were moved into the lexicon. This move was a response to the position held by Stanley in particular: if unvalued features are allowed into the phonology, then the binary +/- feature-value system effectively becomes ternary, +/-/ \emptyset . At the time, this was seen as an abuse of the notation, but I will argue (and have in part argued previously; see Vaux & Samuels (2006), Samuels (In press)) along with Inkelas (1995) that Stanley’s objections to ternarity were unwarranted and that it is in fact the best way to express multiple well-attested phonetic and phonological phenomena.

Furthermore, since putting all redundancy rules before phonological rules completely eliminates the possibility of ‘using’ underspecified representations to do any work in phonology (as Stanley intended), it also eliminates any empirical advantages to adopting underspecification. The only reason for maintaining less-than-full specification in such a system is Lexical Minimality (see discussion in Steriade (1995)). Since we now know that the human memory capacity is far less limited than was formerly assumed, this principle seems less credible now than it was in the 1960’s. There is also empirical evidence that a strong form of Lexical Minimality cannot be

maintained. I illustrate here with an example from Archangeli & Pulleyblank (1994), but Steriade (1995, §2.3.1) provides multiple additional cases.

Whereas Archangeli & Pulleyblank (1989) had held a position similar to what was presented in *SPE*—they employed the Redundancy Rule Ordering Constraint to prevent phonological rules from referring to features before the redundancy rules filling in the values of those features applied—five years later (e.g., Archangeli & Pulleyblank 1994), the same authors no longer believed this condition could be enforced (see also Steriade 1995, §2.2.1). One of their arguments came from the Nilotic language Kalenjin, which has a process of [ATR] harmony; we will see other similar cases in the next chapter. In Kalenjin, some affixes freely alternate between [+ATR] and [-ATR], but a handful of ‘opaque’ affixes invariably surface as [-ATR] and stubbornly block [+ATR] from spreading across them. One such morpheme is the negative affix, *-ma-*. Thus, while in (23a) the root *ke:r*, which is invariably [+ATR], and all affixes surface as [+ATR], when *-ma-* is present as in (23b), the leftmost prefix surfaces as [-ATR]. (Following convention, vowels which are observed to alternate in their values for [ATR] are capitalized. The vowel transcribed as [a̠] is the [+ATR] counterpart of [a].)

- (23) a. kI-A-ke:r-In → [ki̠əɣe:rin] ‘I saw you-SG’
 b. kA-ma-A-ke:r-Ak → [kama̠əɣe:r̠ək] ‘I didn’t see you-PL’

Archangeli & Pulleyblank attributed the different behavior of vowels with respect to [±ATR] alternation to differences in their underlying representation. They hypothesized that invariant roots and affixes carry their [ATR] specifications throughout the derivation, but that the vowels which alternate remain unvalued for [ATR] until the

harmony rule (for them, a rule of autosegmental spreading) fills in those values. Specifically in the case of (23b), *-ma-* is underlyingly specified [-ATR] and prevents the root's [+ATR] value from spreading across to *kA-*. Crucially, *both* values of [ATR] must be present in the lexicon, which means Lexical Minimality cannot be correct: we need [+ATR] to spread and [-ATR] to block. The correct pattern is derived only by maintaining a three-way +/-/ \emptyset distinction at the time the harmony rule applies. In some sense, this is a return to the Prague School view of how to analyze such alternations; the \emptyset -valued segment is similar to the notion of 'archiphoneme' employed by Jakobson and Trubetzkoy (see Inkelas 1995). Anticipating the next chapter, this argument still holds even when the harmony is not achieved by autosegmental spreading but rather by the SEARCH and COPY operations argued for here, following Mailhot & Reiss (2007) and Samuels (In press).

As soon as we admit some degree of underspecification, a new question arises: which features are specified in lexical entries and which can/must be filled in by phonological rules? Several answers to this question have been proposed. Inkelas (1995) breaks the approaches up into three general categories: those which refer to markedness, those which refer to redundancy, and those which refer to predictability. Within each of these categories, there is a considerable range of variation: for instance, among those who believe that unmarked values are underspecified, there are some who take markedness to be universal, while others believe it is language-specific or contextually-determined. Cross-cutting these categories is the distinction between 'radical' and 'contrastive' theories. Radical underspecification theories can be divided into two types, those which subscribe to (24a.i) and (24b), and those which subscribe

to (24a.ii):

- (24) Assumptions of radical underspecification (Mohanan 1991:285-286)
- a. Underlying representations may not contain the specification of both values of a feature
 - i. in the same environment.
 - ii. in any environment.
 - b. The value specified in underlying representations is the marked one.

Kiparsky (1982), espousing a ‘context-sensitive’ view, assumes (24a.i) and (24b). On the other side, Archangeli (1988 et seq.) pursues the ‘context-free’ approach, which assumes only (24a.ii). It has been widely remarked that (24b) cannot be maintained in the context-free form of radical underspecification, because the markedness of a given feature value is context-dependent on anyone’s view. For instance, while for obstruents, voicelessness is unmarked, the opposite is true for sonants, as was already noticed by Greenberg (1963) and in *SPE*. But as Goldsmith & Noske (2000) note (see also Steriade (1995, §2.2.2)), even those who link (un)markedness and underspecification allow for ‘markedness reversals,’ or specification of the unmarked value (with the marked value supplied by rule). This seriously undercuts the premise of (24b).

We have already seen from examples like the one from Kalenjin presented in (23) that Lexical Minimality, which is the fundamental principle of radical underspecification, is not viable. That is, neither (24a.i) nor (24a.ii) can be maintained. Moreover, Kiparsky’s view that the unmarked feature value is always the lexically unspecified one (i.e., (24b)) is not an option that is available within the framework developed here, in which the language learner cannot make use of markedness, and admitting

markedness reversals essentially entails abandoning (24b) anyway. Thus, neither of the basic principles of radical underspecification are justified, and for this reason, I must reject this theory (see Mohanan (1991) and Steriade (1995, §2) for further arguments, though I do not endorse their ultimate conclusions).

An alternative position rejects Lexical Minimality (for the reasons described above) and instead holds to the ‘Contrastivist Hypothesis,’ which I give in two complementary formulations below (see also Clements (1987), Steriade (1987), Calabrese (1988), and Drescher (2003), all of which subscribe to this general principle):

(25) CONTRASTIVIST HYPOTHESIS

a. Steriade (1995:142):

- i. Feature values predictable on the basis of universal co-occurrence conditions or on the basis of positional neutralization statements can be omitted from underlying representations.
- ii. No other features may be underspecified.

b. Hall (2007:20):

The phonological component of a language L operates only on those features which are necessary to distinguish the phonemes of L from one another.

These two statements, though logically independent, when taken together provide the basic idea behind the contrastivist stance: only contrastive features are phonologically active, and only non-contrastive features may be underspecified. The most pressing question, of course, is what ‘contrastive’ means. The original test for contrastiveness, developed by Trubetzkoy (1939), has come to be known as the ‘minimal

pair test:’ if two phonemes differ by a single feature, then that feature is contrastive. Archangeli (1988)’s Pairwise Algorithm (which she uses to illustrate the shortcomings of this approach) formalizes the process of determining contrastivity via minimal pairs and subsequent pruning of non-contrastive features:

(26) PAIRWISE ALGORITHM (Archangeli 1988:192)

- a. Fully specify all segments.
- b. Isolate all pairs of segments.
- c. Determine which segment pairs differ by a single feature specification.
- d. Designate such feature specifications as ‘contrastive’ on the members of that pair.
- e. Once all pairs have been examined and appropriate feature specifications have been marked contrastive,’ delete all unmarked feature specifications on each segment.

Dresher (To appear) describes several problems with the representations that the Pairwise Algorithm generates, one of which I will describe here. A major concern is that the Pairwise Algorithm fails when there are “too many” features relative to the number of vowels in a system. Consider the following, a common five-vowel system, which can be described using three features:

	i	e	a	o	u
HIGH	+	-	-	-	+
LOW	-	-	+	-	-
BACK	-	-	+	+	+

The shaded feature values are those which are non-contrastive and will ultimately be deleted in step (e). In other words, [HIGH] distinguishes the minimal pairs {i,

e} and {o, u}; [LOW] distinguishes {a, o}, and [BACK] distinguishes {i, u} and {e, o}. The Pairwise Algorithm succeeds here but, as Drescher notes, this scenario is unrealistic: *all* features are supposed to be specified initially, so [LABIAL] should also be present at the outset. This revised feature inventory is shown below:

(28)		i	e	a	o	u
	HIGH	+	-	-	-	+
	LOW	-	-	+	-	-
	BACK	-	-	+	+	+
	LABIAL	-	-	-	+	+

Now the failure of the Pairwise Algorithm becomes apparent. [HIGH] is clearly doing work, separating the minimal pairs {i, e} and {o, u}. The problem arises when the learner prepares to discard the values for [BACK] and [LABIAL]. One of the two features is redundant, but only given the other's existence. Deleting all the values for both mutually-dependent features, [BACK] and [LABIAL], would be disastrous, leading to the collapse of several phonemic distinctions. The learner must decide which values to discard and which to retain, and the algorithm provides no basis on which to make this decision; in fact, the algorithm here directs the child towards failure.

Archangeli (1988) noted another instance in which the Pairwise Algorithm crashes entirely and is unable to provide any contrastive specification at all, namely when (by sheer accident) it just so happens that a language's vowel inventory is not spread over the featural space in such a way that there is at least one path through that space connecting all the minimal pairs of vowels. This is the case in Maranungku, which has the five-vowel inventory shown below:

(29)		i	æ	ɑ	ə	ʊ
	HIGH	+	-	-	-	+
	LOW	-	+	+	-	-
	BACK	-	-	+	+	+

The feature [HIGH] distinguishes {ə, ʊ}, [LOW] distinguishes {ɑ, ə}, and [BACK] distinguishes {i, ʊ} and {ɑ, æ}. But no segment differs from either [i] or [æ] only in its specification for [HIGH] or [LOW], so these features will be marked as non-contrastive on those segments. The result is that both [i] and [æ] are specified only as [-BACK]: the Pairwise Algorithm fails to distinguish them.

Because of these failures of the pairwise method of extracting contrastive features, Dresher (1998, et seq.) adopts an approach which can be found in rough form as the “dichotomous scale” or “branching diagram” in Jakobson & Halle (1956) and some subsequent work by Halle. Rather than starting out by finding minimal pairs, the “feature hierarchy” approach first assumes that all sounds in the language are allophones of a single phoneme. The inventory is then divided by successive cuts, rendering it into increasingly smaller sets until ultimately, each set has only one (contrastive, phonemic) member. Dresher (1998) formalized the Successive Division Algorithm¹³ for accomplishing this process, as shown below (version here from Dresher (To appear)):

(30) SUCCESSIVE DIVISION ALGORITHM

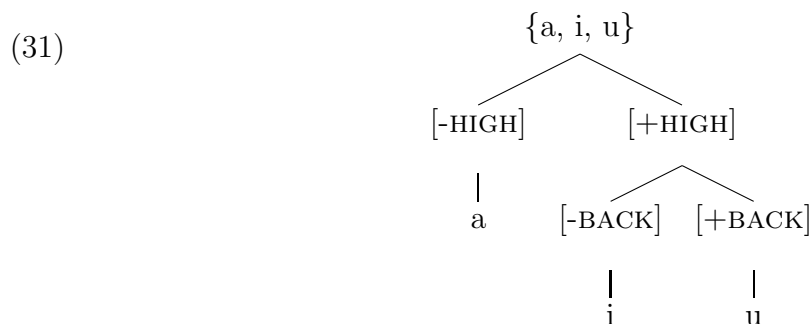
- a. Begin with no feature specifications: assume all sounds are allophones of a single undifferentiated phoneme.
- b. If the primordial allophonic soup is found to consist of more than one

¹³Actually, the version in Dresher (1998) is called the Successive Binary Algorithm and is intended to work only with privative features. Dresher (2003) presents the more general Successive Division Algorithm for n -ary features; the Successive Binary Algorithm is merely a special case of this, as noted in step (b).

contrasting member, select a feature and divide the set into as many subsets as the feature allows for. (With binary features, it becomes the Successive Binary Algorithm.)

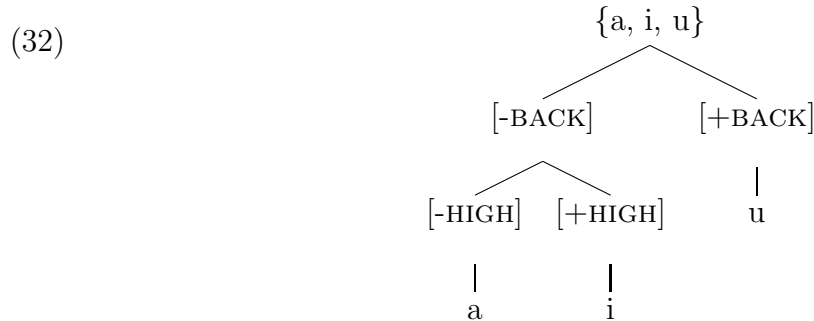
- c. Repeat step (b) in each subset: keep dividing the inventory into sets, applying successive features in turn, until every set has only one member.

This algorithm does not fail in the places that the Pairwise Algorithm does (but neither does it generate only the set of features which pass the minimal pairs test). An interesting property of the Successive Division Algorithm is that it produces a hierarchy of contrastive features with varying scope. In the absence of a universal feature hierarchy (i.e., something that would tell the algorithm which order it should try features in step (c)), for which there does not seem to be evidence, it therefore predicts that two languages with the exact same phonemic inventory can have different contrastive hierarchies. For example, the three-vowel system {a, i, u} could be divided first by height and then {i, u} could be differentiated on the basis of backness, in which case height would be higher in the feature hierarchy and thus have wider scope than backness:



Alternatively, {a, i, u} could be separated first by backness and then {i, a} could then be split by height, so that backness is higher in the hierarchy than backness and

[-BACK] takes scope over [\pm HIGH]:



Unlike Jakobson & Halle (1956), who sought to constrain the possible orders of acquisition by proposing a partially fixed universal hierarchy (i.e., a feature geometry), Dresher makes no such move, though others (Béjar 1998, Hall 1998, 2007, Mercado 2002) have done so in an attempt to restrict the exponential growth of the possible orderings. But in the absence of innate bias, Dresher predicts that all orders should be possible, and that the difference between them should be apparent in the phonological processes which these languages exhibit. Exactly what type of evidence a child would require and how this would be translated into the correct order of divisions (or whether some ‘backtracking’ would be allowed if the child’s first run through the algorithm yielded a scope order subsequently observed to be incorrect) is not clear to me.

Even more problematically, it is difficult to see how a child could begin with no feature specifications in step (a) of the Successive Division Algorithm yet still be able to identify contrasts in step (b). At the very least, this would require a theory of how to identify contrasts without relying on features—and once we go that far, there is hardly any difference between this ‘innate’ feature theory and a system in which categories come first and features emerge from them. Secondly, as Charles Reiss (p.c.) has pointed out to me, the sense of ‘allophone’ which is required in step (a) is quite different from the way allophones are typically defined. For example,

Kenstowicz (1994:66) introduces allophony by calling the reader's attention to eight different pronunciations of /t/ in English:

(33)	[t]	“plain”	stem
	[t ^h]	aspirated	ten
	[t]	retroflexed	strip
	[D]	flapped	atom
	[N]	nasal flap	panty
	[tʔ]	glottalized	hit
	[ʔ]	glottal stop	bottle
	[]	zero	pants

All of these allophones vary in their featural specifications. In Successive Division Algorithm step (a), however, it is explicitly stated that all sounds are treated as allophones but they *cannot* differ in their featural specifications because *there are no feature specifications*. Again, this issue arises because of the problematic assumption that the algorithm begins operating over an undifferentiated ‘primordial allophonic soup.’

In addition to this major conceptual difficulty with the Successive Division Algorithm, there are empirical problems with contrastive underspecification which have been discussed by Pulleyblank (2003) and Hall (2007). Both of these works argue that while non-contrastive features are not phonologically active, at least some of them must nevertheless be present in phonological representations ‘prophylactically,’ as Hall puts it. Here I will give the example of voicing assimilation in a particular group of Czech dialects discussed by Hall (2007); he also gives parallel arguments for similar processes in other dialects of Czech and in other West Slavic languages. The cases of glottalization and lenition in Nuuchahnulth and Oowekyala presented by Pulleyblank (2003) are of the same basic genre.

Czech has both final devoicing and regressive voicing assimilation. Final devoicing produces alternations like the ones shown below. The data in this and subsequent Czech examples can be found in Hall (2007:40ff).

- (34) Final obstruent devoicing
- | | | | |
|----|---------------|----------|--------------------|
| a. | <i>muž</i> | [muʃ] | ‘man’ (NOM.SG) |
| b. | <i>mužem</i> | [muʒem] | ‘man’ (INST.SG) |
| c. | <i>hrad</i> | [ɦrat] | ‘castle’ (NOM.SG) |
| d. | <i>hradem</i> | [ɦradem] | ‘castle’ (INST.SG) |

Regressive voicing assimilation in consonant clusters (which occurs both within words and across word boundaries) can be demonstrated with the prepositions *s* /s/ ‘with’ and *z* /z/ ‘from.’ Before sonorants, these prepositions show up in their underlying forms:

- (35) Realization of *s* and *z* before sonorants
- | | | | |
|----|----------------|----------|-----------------|
| a. | <i>s lesem</i> | [slesem] | ‘with a forest’ |
| b. | <i>z lesa</i> | [zlesa] | ‘from a forest’ |
| c. | <i>s mužem</i> | [smuʒem] | ‘with a man’ |
| d. | <i>z muže</i> | [zmuʒe] | ‘from a man’ |

Before obstruent-initial words, though, the prepositions undergo voicing assimilation:

- (36) Realization of *s* and *z* before obstruents
- | | | | |
|----|----------------|----------|----------------|
| a. | <i>s domem</i> | [zdomem] | ‘with a house’ |
| b. | <i>z domu</i> | [zdomu] | ‘from a house’ |
| c. | <i>s polem</i> | [spolem] | ‘with a field’ |
| d. | <i>z pole</i> | [spole] | ‘from a field’ |

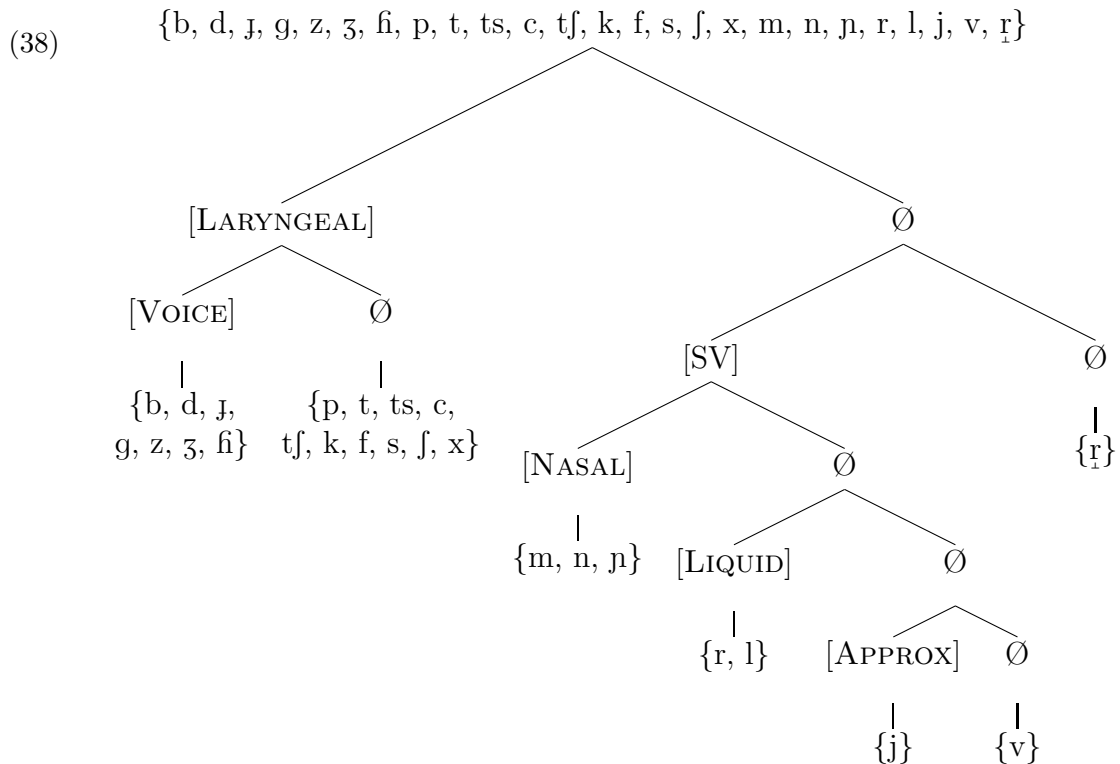
Two voiced segments, /v/ and /ɽ/, behave anomalously with respect to these processes.¹⁴ While /v/ and /ɽ/ both undergo final devoicing and regressive assimilation,

¹⁴Here there is some degree of dialectal variation; I focus on the dialects which treat these two segments differently from each other, because these allow for the easiest illustration of the point at hand. Hall (2007) discusses the dialect situation in detail.

neither one triggers assimilatory voicing when it is second in a consonant cluster, as illustrated below. Instead, in the dialects in question, /v/ remains voiced but /r̥/ is devoiced if it is adjacent to a voiceless obstruent on either side.

- (37) Non-triggering of assimilatory voicing by /v/ and /r̥/
- | | | | |
|----|-----------------|------------|----------------|
| a. | <i>s vránou</i> | [svra:nou] | ‘with a crow’ |
| b. | <i>květ</i> | [kvjet] | ‘flower’ |
| c. | <i>nářky</i> | [na:r̥ki] | ‘lamentations’ |
| d. | <i>středa</i> | [streda] | ‘Wednesday’ |

This anomalous behavior proves to be problematic for contrastive underspecification. Hall argues at length that the feature hierarchy assigned by the Successive Division Algorithm to the consonant inventory of Czech (place features excluded) is as follows (from Hall 2007:82):



On the basis of this feature hierarchy, we can describe voicing assimilation as the segments which are specified for [LARYNGEAL] (with or without [VOICE]) spreading

the LARYNGEAL node leftward. The sonorants, including /v/, are unspecified for [LARYNGEAL] so they do not trigger assimilation, as we saw earlier. Moreover, they are specified for the feature [SV] (Sonorant Voicing or Spontaneous Voicing), which means they cannot be devoiced; their voicing derives not from [VOICE] under the LARYNGEAL node, but from the distinct [SV] feature.

The problem is that /r̥/ has neither [LARYNGEAL] nor [SV]. (This is a pervasive problem, since the Successive Division Algorithm by its very nature always produces a segment with maximal underspecification (see Hall 2007, §1.2.7).) What happens when /r̥/ is in a position to get devoiced, and [LARYNGEAL] spreads onto it? The result should be that it becomes whatever the least specified voiceless obstruent is: the one which is specified only for [LARYNGEAL] and nothing else. The exact identity of this segment doesn't matter; let's assume for concreteness that it's /t/. The important thing is that this least-specified obstruent will *not* be /r̥̥/, because there is no phonemic /r̥̥/. Our rule of [LARYNGEAL] spreading thus incorrectly predicts that a devoiced /r̥/ should surface as /t/, which it demonstrably does not. There is a way around this problem, namely to assign a different feature hierarchy which does not result in /r̥/ being the least specified segment. But then we would predict that other segments would pattern with /r̥/ with respect to assimilation, when in reality it stands alone.

The solution to this problem, and others like it, is to redundantly or prophylactically specify /r̥/ for some non-contrastive feature. (Hall suggests [VIBRANT], but it does not really matter.) This feature need not be visible to the phonology at all, because it is totally inert; no phonological rule targets it, nor does it block any rules

from applying. It simply exists to prevent the addition of [LARYNGEAL] to /ɾ/ from giving an undesirable result. That is, if /ɾ/ is specified for [VIBRANT], when devoiced it will remain distinct from /t/ (or whatever the maximally underspecified obstruent happens to be). This solution is trivial in some sense, but it requires abandoning the fundamental principle of contrastive underspecification.

Further evidence against contrastive underspecification comes from an experiment on Georgian reported by Chigogidze & Reiss (2008), Reiss (2008a), and Hale & Reiss (2008). Georgian has an inventory of five vowels, {i, e, a, o, u}. It also exhibits an alternation between plain/clear [l] before {i, e} and velarized [ɫ] before {a, o, u}. This is easily described with underlying /ɫ/ and a rule taking /ɫ/ to [l] before {i, e}. According to the conventional wisdom, the formalization of this (or any) rule should be the most general one of the extensionally-equivalent possibilities which correctly cover the observed data, and hence would apply in the environment before [-BACK] vowels. But when Chigogidze and Reiss tested Georgian speakers on the production of lateral + [æ] sequences¹⁵, they found that the subjects actually produced [ɫ] before this non-native vowel. This is the opposite of what we expect if the environment for the rule producing [l] is specified as occurring before [-BACK] segments. Since [æ] is [-BACK], it would trigger the rule if this were the correct formulation.

Chigogidze, Hale, and Reiss take this as evidence that phonological rules are more specific (i.e., that learners generalize less) than is typically thought (see Reiss (2003a) for more arguments supporting this view). In particular, they suggest that rules are generated by intersecting the “subrules” which cover each particular instance of an

¹⁵They report that [æ] was accurately produced by the subjects.

observed alternation. For example, in this case the learner would start with two subrules (from Hale & Reiss 2008:96):

- (39) a. /ɫ/ to [l] before i
- $$\begin{bmatrix} +\text{LATERAL} \\ +\text{SON} \\ \vdots \end{bmatrix} \rightarrow [-\text{BACK}] \text{ before } \begin{bmatrix} +\text{HI} \\ +\text{ATR} \\ -\text{BACK} \\ -\text{LOW} \\ -\text{ROUND} \end{bmatrix}$$
- b. /ɫ/ to [l] before e
- $$\begin{bmatrix} +\text{LATERAL} \\ +\text{SON} \\ \vdots \end{bmatrix} \rightarrow [-\text{BACK}] \text{ before } \begin{bmatrix} -\text{HI} \\ +\text{ATR} \\ -\text{BACK} \\ -\text{LOW} \\ -\text{ROUND} \end{bmatrix}$$

Taking the intersection of these two subrules—removing the values where they disagree—would result in the following rule:

- (40) /ɫ/ to [l] before {i, e}
- $$\begin{bmatrix} +\text{LATERAL} \\ +\text{SON} \\ \vdots \end{bmatrix} \rightarrow [-\text{BACK}] \text{ before } \begin{bmatrix} +\text{ATR} \\ -\text{BACK} \\ -\text{LOW} \\ -\text{ROUND} \end{bmatrix}$$

Unlike the maximally-general rule of lateral fronting, the one produced by intersection correctly predicts that fronting will not be extended to laterals before [æ].

I feel more investigation along these lines is necessary before we can make secure conclusions about the nature of rule induction, although the suggestions that Chigogidze, Hale, and Reiss make on the basis of this experiment are very interesting. The point I want to make for now is a bit different, and is not dependent on the exact formulation of the rule in question. All we need to assume is that Georgian speakers do not perform lateral fronting before the front vowel [æ] because [æ] does not fall within the rule's structural description. If we take segments to be contrastively underspecified, we will not come up with the correct rule. Recall the table from Dresher

(To appear) shown earlier in this section (here I use the version without [LABIAL] for simplicity's sake):

(41)		i	e	a	o	u
	HIGH	+	-	-	-	+
	LOW	-	-	+	-	-
	BACK	-	-	+	+	+

/i/ and /e/ will be specified only [-BACK] and [\pm HIGH]. Thus, the rule which produces lateral fronting could only refer to [-BACK]: precisely the overgeneralization which we must avoid. The Georgian example, then, provides evidence for a claim that is even stronger than the one made by Hall (2007), who maintains that non-contrastive features may be present but are invisible to phonology. Instead, we find on the basis of Georgian that non-contrastive features *can* be active.

I hope to have convinced the reader with the arguments presented in this section that there is ample reason to reject both radical and contrastive underspecification, but that some degree of underspecification should be admitted. In what follows, I will outline my proposal.

As has been widely noted, there are two kinds of underspecification (see, e.g., Steriade 1995). One type, with which we will not concern ourselves here, is the ‘trivial’ or ‘intrinsic’ underspecification which arises automatically from the use of privative features. Any phonologist who uses privative features or anything like feature geometry has to accept this. The more interesting type of underspecification is (potentially) resolvable. I follow Inkelas (1995) and Reiss (2008a) in arguing that lexical representations are underspecified when there is evidence of alternating forms which contrast with non-alternating ones, and the alternation is predictable. Inkelas calls this ‘Archiphonemic Underspecification.’ The case which is typically used to il-

illustrate Archiphonemic Underspecification is feature-filling harmony, as we saw with Kalenjin in (23) and with Tangale in (18); we will see several more examples in the next chapter. This very restricted type of underspecification is reminiscent of (part 1 of) the Strong Naturalness Condition, here from Goyvaerts (1978:125): “lexical representations of non-alternating parts of morphemes are identical to their phonetic representations.”

In short, this type of underspecification arises when a morpheme’s exact featural content is unlearnable because it exhibits alternation of a contrastive feature value. Underspecification is potentially resolvable only in polymorphemic words because a morpheme already present in the derivation may provide featural specifications that subsequently-added morphemes can utilize. When the underspecified morpheme is concatenated with its host, only then does the needed feature value information become available. For example, take the case of the Tangale suffix *-u/-ʊ* which we saw earlier in (18). Given Archiphonemic Underspecification, we could (as I will further argue in Chapter 4) think of there being a single suffix underspecified for [ATR] which receives a value for that feature upon concatenation with a root. Phonological rules then get the chance to apply (the precise manner and circumstances of rule application will be specified in Chapters 4 and 5), resolving the underspecification with the host morpheme’s feature values. The necessary involvement of two morphemes—the underspecified recipient and the fully-specified donor—is why the phonological component is only able to repair underspecification when two morphemes are concatenated.

Note that I have claimed only that underspecification *can* be resolved in polymorphemic words, not that it *must*. I do not claim that all underspecification must be resolved, or that only polymorphemic words can be underspecified. This leaves open two possibilities: (a) underspecification in monomorphemic words, which will not be resolvable, and (b) underspecification in polymorphemic words which is nevertheless unresolved. Both of these phenomena are attested. They provide incontrovertible evidence against the principle of Full Specification, which demands that the output of a phonological computation is fully/maximally specified.

There are several well-known examples of ‘perseverant’ or ‘phonetic’ underspecification which persists from the underlying representation straight through to the phonetic component. Keating (1988) discusses examples such as English [h] and Slavic [x]. This type of underspecification is often characterized by gradient transitions from the specifications one flanking segment to the other, passing straight through the underspecified segment. It can also be characterized by a wide range of phonetic variation. Vaux & Samuels (2006) discuss this point with regards to laryngeal features: stops which are unspecified for the laryngeal gestures that produce aspiration (or lack thereof) exhibit a wide range of voice onset time values. We argue that this underspecified voiceless stop is in fact the unmarked¹⁶ stop series, and is often utilized by languages which do not have an aspiration contrast.

One particularly striking example of perseverant underspecification comes from Marshallese and has been described by Bender (1968), Choi (1992), Hale (2000), and Hale et al. (2007). Bender (1968) analyzes the underlying vowel inventory of

¹⁶Again, in the loose sense of the term.

this language as being specified along only two dimensions: ATR and height. Choi (1992)'s phonetic study shows that the vocalic formants show a smooth transition between the points of articulation of the flanking consonants, which may be plain (C), palatalized (C^j), or labialized (C^w). Thus, the surface vowel inventory is quite large, varying along the front/back, round/unround, high/low, and +/-ATR axes:

(42) Marshallese surface vowel inventory (Hale et al. 2007)

	$C^j_C^j$	C_C	$C^w_C^w$	C^j_C	$C^j_C^w$	C_C^j	C_C^w	$C^w_C^j$	C^w_C
[+HI, +ATR]	i	ɯ	u	iɯ	iu	ɯi	ɯu	ui	uu
[+HI, +ATR]	ɪ	ɣ	ʊ	ɪɣ	ɪʊ	ɣɪ	ɣʊ	ʊɪ	ʊɣ
[-HI, +ATR]	e	ɬ	o	eɬ	eo	ɬe	ɬo	oe	oɬ
[-HI, -ATR]	ɛ	ɸ	ɔ	ɛɸ	ɛɔ	ɸɛ	ɸɔ	ɔɛ	ɔɸ

This is true for all vowels in Marshallese: affixes, too, remain underspecified. Since no vowels are ever specified for backness or rounding, morpheme concatenation cannot rectify this situation. There is no donor from which an affixal vowel could ever obtain a feature value.

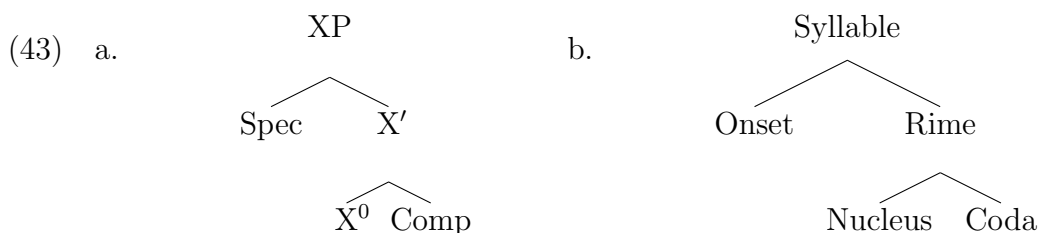
With this example, we conclude our discussion of underspecification, and of phonological features. We will make heavy use of the conclusions drawn here about underspecified representations, particularly in Chapter 4, which focuses on phonological operations. The role of Archiphonemic Underspecification in particular will be explored more fully at that point, as will feature geometry. For the time being, we will move on to discussion of how the feature bundles which we call segments are themselves arranged and grouped.

3.3 Towards a flat phonology

3.3.1 Syllables are not like phrases

One conclusion I would like the reader to take away from this dissertation is that, while phonology and syntax may look similar on the surface—and this is not likely to be a coincidence—upon digging deeper, crucial differences between the two modules begin to emerge. I focus here on one area where surface similarities hide striking differences: the comparison between phonological syllables and syntactic phrases. Syllables and phrases have been equated by Levin (1985) and many others, with some going so far as to claim that phrase structure was exapted from syllable structure (Carstairs-McCarthy 1999). I argue that these analogies are false, and that many of the properties commonly attributed to syllabic structure can be explained as well or better without positing innate structure supporting discrete syllables in the grammar.

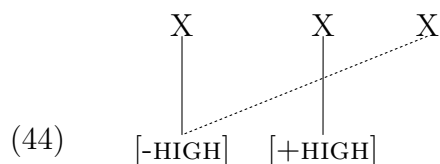
Before we address the myriad questions about syllables that have arisen in the past half-century, it is instructive to highlight the critical differences between the traditional views of syllables and syntactic phrases, even though the two have been explicitly equated. Compare the structures in (43):



On the left in (43a) is a generic X' template of the type used in syntax (since Chomsky (1970)), and on the right in (43b) is a typical characterization of syllabic structure (since Fudge (1969)). Both are right-branching, and both observe binarity

of branching. However, one important property immediately differentiates the two structures. The syntactic structure is recursive in a way that the phonological structure is not: the complement of X can be YP —itself the maximal projection off Y , which can have its own specifier and complement and so on ad infinitum. Syllables, in contrast, are like beads on a string; no syllabic or sub-syllabic node can dominate another node of the same type.

Scheer (2004), who also denies the validity of (43b), attributes the lack of recursion to the phonological component's inability to create arboreal structure. He further claims that the absence of hierarchical notions such as binding, locality, and relativized minimality in phonology provide evidence for a lack of the dominance dimension in this domain; in his view, such concepts “make sense only when certain chunks of the linear string, hierarchically speaking, do not stand on a par with others” (Scheer 2004:238ff). I believe Scheer's conclusion here is correct, but the evidence could be better chosen. Locality is certainly a valid notion in an object with any number of dimensions, though of course, it must be defined appropriately to the specific geometry. No one would deny that there are local phenomena in phonology: assimilation may occur only between two adjacent segments, and a great many processes in any given language are limited to applying locally within a larger domain (for instance, a prosodic phrase). Similarly, minimality has been captured in phonology by the No [Line-]Crossing Constraint (Goldsmith 1976) holding over autosegmental representations. This constraint is intended to rule out spreading of one feature value (here, [-HIGH]) across an instance of the opposite feature value (here, [+HIGH]):



Rizzi (2002), following Halle, has also argued that the concept of minimality is relevant in this domain. (We will adopt a similar position in Chapter 4.) It is not, then, that these notions are absent in phonology, but simply that in phonology, unlike in syntax, they must be defined in linear terms. That is to say, there is no evidence for a hierarchically-defined relation like c-command in this module (Carr 2006).

There are also properties of syllables which syntactic phrases lack. Syntactic elements belong strictly to one phrase or another,¹⁷ but phonological elements can act as though they are members of two adjacent syllables at once (“ambisyllabic”). Some words which have been taken by various theories to involve ambisyllabicity are listed below; these and more examples can be found in Kahn (1976:34ff).

- (45) Hammer, being, booing, bidding, money, lemon, pony, happy, attic, collie

Also, the range of possible syllabic shapes is relatively large. Every language allows

¹⁷Excluding the potential case of parallel merge (Citko 2005). But note that even in the claimed parallel merge cases (such as Right-Node Raising; see, e.g., Bachrach & Katzir 2007), the situation is different from phonology. The configuration we find in phonology is one element behaving as both coda and onset of two adjacent (“sister”) syllables, which would be equivalent to one syntactic element behaving as both complement and specifier. As far as I am aware, the parallel merge cases are either one element behaving as a complement or specifier to two sister phrases (but always the same position in both, as in right node raising), or as the complement to one head and the specifier of a higher one (as in *wh*-movement).

CV syllables (Jakobson & Halle 1956),¹⁸ but many also allow variations beyond this: syllables can be onsetless or have branching onsets, simple codas, or branching codas. This is not true of syntactic phrases, which always adhere very closely to the X' schema. If syllabic structure and phrase structure are both part of UG, we would be justified in wondering why the former is so variable but the latter is completely rigid. One might make recourse to parameters of syllable structure (Stillings et al. 1995) or to constraint rankings, but the force of the argument remains the same. Why are there no languages that, for example, disallow complements just as some languages disallow codas? The way phrases are recursively embedded in syntax makes this utterly impossible.

Another point of contrast between syntax and phonology involves the status of segments that violate the Sonority Sequencing Principle (e.g., the /s/ in the word *string*), which have been called 'appendices,' 'stray segments,' 'extrasyllabic material,' or 'demisyllables' (see Vaux (To appear) for a review of the literature on this topic). Various approaches exist for dealing with these segments: some consider the stray to be its own degenerate syllable, while others treat it as unsyllabified material that attaches to a higher node (usually the prosodic word) in the phonological structure. In either case, there is no analogue in syntax. The closest parallel would be with adjuncts, but adjuncts have a place within X' structure; they are not exempt from it, and they are not degenerate phrases.

¹⁸One ostensive counterexample to this universal has been raised, namely the language Arrernte; see Sommer (1970). However, one quarter of the Arrernte lexicon is actually consonant-initial; see Breen & Pensalfini (1999) and Duanmu (2008:274).

Finally, a couple ostensive syntax-phonology similarities are points of contention. These controversial notions are headedness and endocentricity. On one side of the debate are those who, like Hornstein (2005), Tallerman (2006), and Carr (2006),¹⁹ do not see these concepts as being applicable to syllables. The opposite view is taken by Carstairs-McCarthy (1999), who claims that syllables are endocentric, with peaks/nuclei (i.e., vowels) as their heads.²⁰ Clearly, if the syllable has a head, the only good candidate is the nucleus. The nucleus is always the most prominent segment in terms of sonority, and no syllable can exist without a nucleus. However, the first of these observations is a circular definition, and the second disappears when we consider that the method used to count “syllables” is actually just counting peaks—when we ask subjects to identify syllable edges, their judgments break down (see, *inter alia*, Kahn (1976), Steriade (1999)).

One type of task which is used to obtain such judgments indirectly involves asking subjects to perform infixation. Assuming that the infix is inserted at a syllable boundary (though see Chapter 4, §4.3), variability in performance on such tasks indicates uncertainty about syllabification. Such a study on expletive infixation in English was undertaken by Pritchett (1984). Pritchett found that in many cases of infixation into V_1sTV_2 sequences (where T stands for a voiceless stop), subjects tend to ‘split’ the [s], producing forms like *des-fucking-spotic* or *des-fucking-structable*.

¹⁹Carr also argues against other notions of ‘headedness’ in phonology, such as the Government Phonology conception of segments as consisting of head and subsidiary features.

²⁰Carstairs-McCarthy further equates nouns with consonants (syllable margins) and verbs with vowels (syllable heads). I will not discuss the issue here, but I refer the reader to Tallerman (2006) for extensive criticism of this position.

Steriade (1999:29), I believe correctly, “interpret[s] segment splitting as the strategy followed when speakers are uncertain how to parse the string.” This follows from the fact that the subjects split [s] most commonly when V_1 was lax and V_2 was stressed (i.e., in words like *despótic*). On the view that syllable divisions follow from word-edge phonotactics (to be explicated shortly), the prohibition on lax vowels word-finally argues against a syllable boundary after V_1 ; the unaspirated stop after [s] argues against a syllable boundary after [s]; a stressed vowel word-initially is also disallowed (it should in that case be preceded by a glottal stop). This means that *de-fucking-spotic*, *des-fucking-potic*, and *desp-fucking-otic* should all be malformed; speakers therefore experience uncertainty when parsing the string. We will see the consequences of this more fully in §3.3.4.

Returning to the question of endocentricity, it is true, as Brown & Golston (2004) note, that onsets and codas are more like each other than they are like nuclei. The same is true of specifiers and complements to the exclusion of heads, but onsets and codas cannot be interchanged (Tallerman 2006), whereas complements frequently become specifiers via syntactic movement. Furthermore, the head of an onset is considered to be its least sonorous element, while the head of the rime is taken to be its most sonorous element (van der Hulst & Ritter 2003, van der Hulst 2007). This is illustrated below with the syllable *tram*, from van der Hulst & Ritter (2003:164); the heads of each constituent are in bold face.



In syntax, this would be as if the possible heads of specifiers were a different set from the possible heads of complements. We may also note another curiosity related to headedness, namely that within a syllabic onset, sonority typically rises (i.e., it is left-headed; van der Hulst & Ritter (2003)), but within a rime/coda, it usually falls. If the least sonorous element is taken to be the head of the coda, then this means the coda is right-headed. Again, this is very different from what we see in syntax (see Tallerman (2006)); it would be as if the head parameter were always reversed in specifiers.

I hope to have shown in this preliminary discussion that the equation between syllables and phrases is not tenable, and based on this conclusion, that syllables ought to be rethought on their own terms. That is the task to be undertaken in the remainder of this section.

3.3.2 Mysteries of the syllable

Apart from failing to explain the differences between syllables and syntactic phrases detailed in §3.3.1, mainstream theories of syllable structure also lack explanatory adequacy where many phonological phenomena are concerned. In the current section I will describe these problems, and in §3.3.4 we will return to see how they are accounted for by the theory proposed here.

It has long been acknowledged that the problem with syllables is not how to count them—that much is straightforward. However, Duanmu (2008:1) shows that even this is not so clear in certain cases, such as when what might be deemed ‘syllabic’ resonants appear. He lists several different works and their differing views (some inferred) on

the number of syllables in the words *hour*, *flour*, *flower*, and *shower*:

	<i>hour</i>	<i>flour</i>	<i>flower</i>	<i>shower</i>
(47) Jones (1950)	1	1	1	1
Hanks (1979)	1	1	2	2
Baayen et al. (1993)	2	2	2	2
Kenyon & Knott (1944)	1	1	1 or 2	1 or 2
Kreidler (2004)	1	1	1	1
Merriam-Webster (2004)	1 or 2	2 or 2	1 or 2	1 or 2
Gussmann (2002)	2	2	2	2

It is even more difficult to determine how to divide syllables (already a subject of controversy in the 1970's; see, e.g., Kahn (1976). See also Duanmu (2008) and references therein on more recent experimental studies). Moreover, it is not clear that tests which purport to probe judgments on syllable boundaries actually do so. Duanmu (2008) discusses one such test, proposed by Giegerich (1992): ask speakers to pronounce each syllable of a word twice, beginning with unambiguous words like *after* and then moving on to more difficult cases like *apple* or *city*. But what can we conclude when (as Giegerich reports) the result of testing subjects on *apple* is *ap-ap-ple-ple*? Duanmu (2008:53) comments:

“[D]oes it mean that the syllables are *ap-ple*, or does it mean that *le-le* [l-] is an unusual sequence of syllables which the speaker would avoid? Second, consider the word *text*. If the result is *tek-tek-st-st*, one might conclude that the syllables is [tek] and [st] is outside the syllable. However, if the result is *text-text*, does it mean that the syllable is [tekst], or does it mean that the speaker is trying to avoid repeating a non-syllable cluster [st]? Finally, Giegerich does not discuss whether speaker judgment is always clear. My own test with some native speakers shows that the judgment can vary. For example, the output for *city* can be *cit-cit-ty-ty* or *ci-ci-ty-ty*. Therefore, the test does not seem to provide conclusive answers.”

Harris (2004) makes similar arguments against tests intended to probe syllabification; Harris believes that in such tasks, the fact that certain syllables (e.g., ones which end with short vowels, such as [si] in *city*) are too small to be phonological

words interferes with speakers' judgments. The notorious variability of native-speaker judgments on syllable boundaries in languages such as English that allow a range of syllable shapes is all the more striking when contrasted with judgments on phonotactics, which are clear and robust (Steriade 1999). Since phonotactic restrictions are traditionally defined over sub-syllabic constituents, one wonders why this sharp dichotomy exists. Discussions of syllabification also lead to questions about universality. Does all phonological content have to be syllabified (Vennemann 1988), and if not, could there be a language that does not have syllables, as argued by Hyman (1985)? We also wonder about variation: why is syllabification never contrastive within a language, while across languages we find identical sequences syllabified in different ways (Hayes 1989, Blevins 1995)? Steriade (1999) mentions the difference between Spanish and Cairene Arabic in this respect. When presented with (C)VTRV sequences, Spanish speakers give $(V)_\sigma(\text{TRV})_\sigma$ syllabifications but Arabic speakers instead judge such sequences to be split up as $(\text{VT})_\sigma(\text{RV})_\sigma$.

Other questions concern syllables as they are put to use describing other phonological processes. At least since Kuryłowicz (1948) it has been acknowledged that codas and word-final consonants behave alike in many respects (i.e., they often form a single environment for phonological rules, their phonetic realizations are often alike, they both tend to be lost over time, etc.), and that onsets and word-initial consonants also behave alike. Scheer (2004), calling these positions the 'coda' and 'coda-mirror' respectively, wonders why this should be. Moravcsik (1978) and McCarthy & Prince (1986[1996]) have noted that references to the syllable are notably absent from the domain of reduplication: a reduplicant can be of CV or CVC shape, but there are no

processes that copy the first syllable of a stem regardless of its shape. For example, in Tohono O’odham, a CV chunk is repeated to indicate plurality or plural agreement (Raimy 2000a:112).

- (48)
- | | | | |
|----|-------|----------|---------------|
| a. | ʔum | ʔu-ʔum | ‘thigh(s)’ |
| b. | hon | ho-hon | ‘body/bodies’ |
| c. | gimai | gi-gimai | ‘braggart(s)’ |
| d. | pualt | pu-pualt | ‘door(s)’ |

In contrast, Ilokano uses CVC reduplication to mark plurality on nouns and the progressive aspect on verbs (Raimy 2000a:128):

- (49)
- | | | | |
|----|--------|---------------|-----------------|
| a. | kaldín | kal-kaldín | ‘goat(s)’ |
| b. | púsa | pus-púsa | ‘cat(s)’ |
| c. | sájit | ʔag-saŋ-sájit | ‘(is) cry(ing)’ |

Both of these processes are more complicated than presented here, but the generalization still holds—a syllable-based analysis does not make the correct predictions, whereas a string-based one does. I argue in Chapter 4 of the present work that affixation, too, operates independently of syllable boundaries (see also Raimy (2008a,b)). These morphophonological processes readily make use of morpheme boundaries, so why can they not use syllable boundaries also, if they have the same status in phonology?

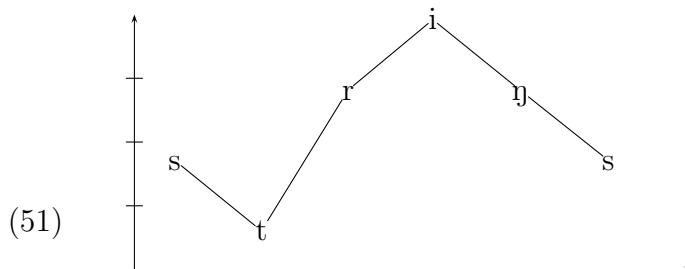
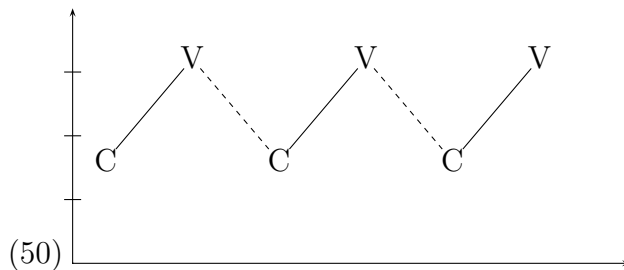
The third and final set of questions that I will attempt to address concerns sub-syllabic constituents. Over the past half-century, a great many parts of the syllable have been posited: onset, nucleus, body, margin, pre-margin, margin core, head, coda, rime, and mora, to name some of the more popular ones. Some of these divisions are mutually exclusive; for instance, the onset-rime model is opposed to the view that the onset and nucleus form a constituent to the exclusion of the coda. It is natural, then, to ask (as Blevins (1995) does) which of these competing models should be

adopted. In the Government Phonology tradition, a popular position is that a proper conception of sub-syllabic constituency can eliminate the need for a syllable node entirely (see, e.g., Harris (1994); Aoun (1979) provides an early example of such reasoning). This makes the need to identify the “real” nodes all the more pressing. Ultimately, a theory of syllable structure should also explain the numerous onset-rime (or onset-coda) asymmetries that we find. Why does only the rime figure into the computation of metrical weight—or alternatively, if the few putative cases of onset-dependent stress are real (Everett & Everett 1984), why are they so rare? The answer to this is typically something like “because only rime segments are moraic” (Hayes 1989), which simply begs the question. Additionally, why, as Scheer (2004) asks, are the coda and the coda-mirror virtually opposite in their tendencies? A related query has been posed in Optimality Theoretic terms: why should the constraints ONSET (penalizing onsetless syllables) and NOCODA (penalizing syllables with codas) exist instead of their opposites, NOONSET and CODA (see, e.g., Haspelmath (1999))?

In sum, current theories of syllable structure are at a loss to explain many important generalizations in phonology. The entire genre of approaches based on syntactic phrase structure has left many questions unanswered, and I have already suggested one major reason why: syllables are just not like phrases. Let us keep this, and the questions posed in this discussion, in our minds as we endeavor now to approach syllables from an entirely different angle.

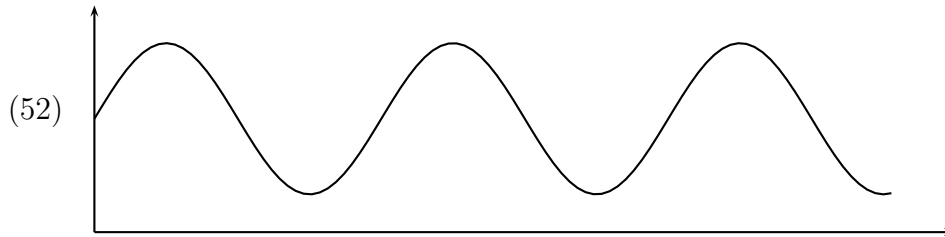
3.3.3 A string theory of syllables

We have already noted that syllables range widely in shape across languages. In (50) I give examples from opposite ends of the spectrum: a series of three CV syllables in (50), and a syllable in (51) that has a branching onset as well as a branching coda, and additionally an appendix. The relative heights of the segments in (50)-(51) represent an abstract scale of sonority (I do not intend to be making a claim about the units of this scale).



Much ink has been spilled in an attempt to accommodate patterns as different as (50) and (51) in a single framework, as alluded to in the previous section. When we move away from viewing one syllable in isolation and consider a continuous stream of speech, however, a different picture emerges. Multiple syllables of any of the attested

shapes concatenate roughly as shown in (52) below.



The peaks and troughs may not be so evenly dispersed, and they may not all be of the same amplitude, but the general shape is the same no matter whether the sonority values being plotted come from syllables that are CV, CVC, sCRV:CRs, and so forth, or any combination of these. This is hardly a new observation; it dates back to Lepsius & Whitney (1865) and de Saussure (1916). Ohala & Kawasaki-Fukumori (1997:356) point out that it is inevitable:

“Just by virtue of seeking detectable changes in the acoustic signal one would create as an epiphenomenon, i.e., automatically, a sequence showing local maxima and minima in vocal tract opening or loudness. In a similar way one could find ‘peaks’ (local maxima) in a string of random numbers as long as each succeeding number in the sequence was different from the preceding one.”

I take the wavelike sonority profile, therefore, to be a fundamental property of speech. It is an inevitable consequence of differentiation in the acoustic signal, which is necessary if there is to be more than one possible phonological output.

Like any wave, the plot of sonority over time can be broken up into periods—one period of the sonority wave is essentially the definition of a syllable proposed by Pike (1947), minus the “chest pulses” debunked by Ladefoged (1967). I suggest that the ability to break the wave up into periods aids with one of the primary tasks of language acquisition and perception, namely the identification of morpheme/word boundaries

(for the moment, I will conflate these two notions). If one morpheme is equivalent to one period of the sonority wave, then the task of parsing the speech stream into morphemes/words reduces to finding local minima/maxima in the wave and making the “cut” when one is reached. We already know that very young children—and tamarins—are sensitive to local maxima and minima (of probability distributions) in speech (see Gambell & Yang (2005) for references), so it is not implausible to suggest that the task may be accomplished in this way.

How might this parsing process be facilitated? Intuitively, the best strategy is to make the edges which are to be identified as different from the middles of morphemes as possible, either in terms of sonority or in some other fashion. There are numerous ways in which this might be accomplished. In (53) I give a partial list of possibilities, all of which I believe are used in natural languages.

(53) Strategies for word-edge identification

- a. Tag every word-end with a special marker.

Parsing strategy: place a word boundary after the end marker has been identified. Allowing only CV words would be a special case of this, in which vowels serve as the word-end markers.²¹

²¹Having a closed set of word-end markers with cardinality >1 is more economical than having only one because the markers can bear a phonemic opposition and therefore support words that differ only in their markers but still contrast lexically. Put in a different way, imagine two languages with the same phonemic inventory. Language A contains only words of the shape CV! (where ! is an edge marker), and Language B contains words only of the shape CV (vowels = edge markers). The number of possible unique lexical items in both languages is the same, yet Language B is more economical on the basis of having shorter words.

- b. Allow only C_1VC_2 words, where C_1 and C_2 are selected from disjoint sets.

Parsing strategy: place a word boundary after C_2 has been identified.

- c. Make word beginnings and ends mirror images of each other.

Parsing strategy: place a word boundary at the axis of symmetry. The Sonority Sequencing Principle/Generalization (word beginnings rise in sonority; word ends fall in sonority) is a frequent manifestation of this strategy.

These strategies never fail to produce unambiguous boundaries when every word in the language subsumes at most one period of the sonority wave. When words are longer, however, the situation becomes more complicated. Now, the parsing strategies that used to pick out word boundaries will sometimes be in error: they will sometimes identify a boundary that is actually word-internal. By simply increasing word length while holding everything else constant, words are suddenly “overparsed” into sub-lexical chunks that we call syllables. We have empirical confirmation that strategies which, like those in (53), yield unambiguous boundaries lead to successful word identification. Simulations of acquisition scenarios show that “the success of the conditional probability analysis in identifying words (and excluding nonwords) depends upon the clarity of syllable boundaries in the input” (Swingley 2005:112). On this view, it is no accident that word edges and syllable edges look the same (an intuition that dates back 2,000 years and is supported by experimental data; see Haugen (1956), Vihman et al. (1985), and Blevins (2003)): they *are* the same. The resemblance between word beginnings and onsets on the one hand and word endings and codas on the other hand has been formalized as the Law of Initials/Finals.

(54) LAW OF INITIALS (Vennemann 1988:32)

Word-medial syllable heads [= onsets —BDS] are the more preferred, the less they differ from possible word-initial syllable onsets of the language system.

(55) LAW OF FINALS (Vennemann 1988:33)

Word-medial syllable codas are the more preferred, the less they differ from possible word-final syllable codas of the language system.

Duanmu (2008:54-55) weakens these laws to state simply that word-initial onsets/rhymes should resemble word-final onsets/rhymes. His rationale for doing so is essentially that some word-final rhymes, such as the [-kst] in *text*, do not occur word-medially (really, morpheme-medially; consider compounds like *textsetting*). Moreover, it is true, as Duanmu notes, that affixation can create word edges which include more consonants than are typically found in monomorphemic contexts; examples include *texts*, *helped*, and *sixths*. This could actually help with parsing morphemes: if parsing proceeds as usual and a ‘normal-sized’ syllable is separated from the extra material, the result will be detection of the morpheme boundary. (And if the affix is larger, containing a vowel as well as a consonant, then (over-)parsing will also give a fair approximation of morpheme boundaries.) This idea of syllables as being epiphenomena of edge-detection also explains why we do not find hard restrictions on what onsets can go with which rhymes/codas, though there are surely some extragrammatical factors, like difficulty of production (Kessler & Treiman 1997), which make onset-coda pairs containing certain gestures less prevalent than others; this results from the mere proximity of the two gestures, and should not be seen as a function of them being parsed into the same unit. (This is consistent with my view that identity-avoidance

effects in phonology typically attributed to constraints like the Obligatory Contour Principle emerge from perception and production difficulties; see Walter (2007).)

I suspect many phonologists would accept the story I have just told, since up to this point we have been in the realm of virtual conceptual necessity. However, I depart from standard theory in saying that, not only are syllables not like syntactic phrases, as I argued in §3.3.1, apart from the existence of these “overparses,” there is nothing more to syllables. This eliminativist approach is in keeping with the guiding question of Chomsky (2007:4) and the Minimalist emphasis on ‘Third Factor’ principles, which we discussed in the previous chapter: “how little can be attributed to UG while still accounting for the variety of I-languages attained?” My proposal is also in the spirit of *SPE*, treating the syllable as epiphenomenal. The phonological portion of a lexical representation, in my view, is just a linear string of segments marked by a start marker # and an end marker %, à la Raimy (2000a), and with no internal structure; the arrows indicate precedence relations among the segments.

$$(56) \quad \# \rightarrow X_1 \rightarrow X_2 \rightarrow X_3 \rightarrow X_4 \rightarrow \%$$

Much of Chapter 4 will be devoted to discussing the implications of representing phonological objects in this way.

From this perspective, it is unsurprising that syllables vary so much (recall the question posed in §3.3.1: why, if syllabic structure is part of UG, is there so much variation beyond CV?); the more interesting question is what, if not syllable structure, constrains the variety of shapes that words take? Here, several factors are at play. One class of factors clearly involves articulation: some sounds are difficult to make in sequence because the movement needed to transition between them is too large

to accomplish rapidly, or because of articulator fatigue induced by repeating the same gestures (an ‘articulatory OCP;’ see Walter (2007)); also, there is a general tendency for speech sounds to arrange themselves in a way such that the jaw opens and closes in a smooth cycle, which yields rough correspondence to the Sonority Sequencing Generalization (Redford 1999). The jaw cycle also explains, at least partially, a preference for single consonants interspersed with vowels, rather than consonant clusters:

“All consonant segments are articulated during the closed portion of the jaw cycle and all vowels are articulated during the open portion (Redford 1999). Thus, in a cycle that contains multiple consonants and vowels, multiple articulations must be achieved quickly as the jaw moves continuously to or from a closed position. In contrast, a cycle with a single consonant and vowel, a single consonantal gesture, begun at the point of maximal closure, is continued through the opening phase and a single vocalic gesture, begun just before the point of maximal aperture, is continued through the closing gesture. Maximizing differences in jaw openness between adjacent segments therefore reduces the number of segments within a cycle and increases articulatory ease.” (Redford et al. 2001:35)

Another class of factors involves perception. Certain types of sounds are more perceptible in certain contexts than in others, and phonological changes such as metathesis frequently re-order sounds so as to enhance perceptibility (Blevins & Garrett 2004); less-perceptible sounds are often simply lost, as famously happens to word-final consonants. Memory constraints may also contribute to limiting the length of words or the distances between word-internal boundaries. Perhaps it is not an accident that the maximum number of elements in a traditional syllable (branching onset, nucleus, branching coda, plus two appendices) is seven, as in Miller’s (1956) Magical Number 7 ± 2 . It may also be relevant that the more usual number of elements between boundaries is less than four (i.e., a CV or CVC syllable), a cardinality that is within

the grasp of humans' (and animals') parallel tracking abilities (Kaufman et al. 1949; see also discussion in Chapter 6). Computational research also suggests that syllable shapes can emerge from functional constraints, lending support to the view that no innate structure constrains syllables. Redford et al. (2001) ran computer simulations with a set of constraints very similar to the ones discussed above—penalties for articulatory and perceptual difficulty, word length, and vocabulary size—and produced artificial languages whose syllable shapes and frequencies corresponded very closely to those found cross-linguistically.

3.3.4 Parsing & underparsing

In the previous section I proposed that syllables are the result of overparsing the speech stream when words subsume more than one period of the sonority wave. In the section to follow, we will discuss underparsing, other factors that interact with the strategies in (53), and how words ultimately come to be identified even if they are at first incorrectly parsed.

Though the task of identifying words becomes more difficult as words get longer, stress-based clues to correct parsing also become available. Primary stress occurs precisely once per word. As a result, whenever a listener hears two primary stresses, he can safely assume that there is one word boundary between them (Gambell & Yang 2005); this 'culminative function' of stress was noticed already by Trubetzkoy. For example, consider the following string:

$$(57) \quad C_1V_1C_2\acute{V}_2C_3V_3C_4\acute{V}_4C_5V_5C_6\acute{V}_6$$

This string must comprise precisely three phonological words: no more, and no less.

We also know that the boundaries of those words must be somewhere between V_2 and V_4 , and between V_4 and V_6 .

Moreover, many languages restrict stress to the same relative location in every word, and infants are sensitive to such prosodic information by 6-9 months of age (Jusczyk et al. 1999). If the stress is placed relative to the tail end of the word, there is a simple strategy: place a word boundary at a certain location after the stress. (This, too, was noticed by Trubetzkoy: the ‘delimitative function’ of fixed stress.) For example, recalling the string above, if the stressed vowel is always final, then the word boundaries must occur at C_3 and C_5 (on one side or the other). With stress placed relative to the beginning of the word, the same procedure can be applied retroactively; a word boundary will be located at a certain location before that stress. This is a less efficient strategy because it requires backtracking in the signal, and the amount of backtracking is increased the further back in the word the stress is. I suggest that this may explain the relative scarcity of initial versus final stress, the especially strong asymmetry between penultimate stress (frequent) and post-initial stress (rare), and the fact that antepenultimate stress is attested while post-post-initial stress is not (Heinz 2007). Combining fixed stress placement with one of the strategies in (53) narrows down the possible parses, as we will soon see.

Each language differs phonotactically; that is, different word-ends are found in different languages, and parsing strategies vary accordingly. These strategies are probably the result of statistical generalizations that infants are able to make over the lexicon based on transitional probabilities, prosody, allophonic variation, and other cues, within a hypothesis space pre-determined for them by UG (Gambell & Yang

2005). I depart from most theories of word-identification by rejecting the assumption that syllabification must be accomplished before word boundaries can be parsed, and I go one step farther than Steriade (1999:32), who argues that “syllabic parsing is an inference process based largely on word-edge properties.” Instead, I argue that a uniform parsing procedure applies only once to the incoming speech stream, with the purpose of finding word boundaries and the side-effect of syllabification. This is supported by the results of simulations undertaken by Cairns et al. (1997), who demonstrate that simply locating syllable boundaries is sufficient for the location of lexical boundaries. In the sense described in the previous paragraph, the parser is looking for word boundaries, but at the same time it will also return some overparses (“false positives”), which are what linguists have called syllable boundaries. The segmented speech stream then interfaces with the lexicon via analysis-by-synthesis, along the lines of Poeppel et al. (2007).

At this stage we may revisit two related questions mentioned in the previous section and now cast in a new light; why are syllabification judgments so tricky, and why is there ambisyllabicity? It has long been noted that for sonority in particular, finding local minima or troughs is much more difficult than finding the highly salient local maxima or peaks (Smalley 1968, Kahn 1976). Peaks are intrinsically acoustically prominent; they are selected from a class of segments determined by a language-specific cutoff on the sonority scale (sonority of n or higher is eligible to be a peak; see

Dell & Elmedlaoui (2002)), which, following Jespersen (1904) is roughly as below.²²

(58) Sonority scale

Voiceless stops < voiced stops/voiceless fricatives < voiced fricatives < nasals
< liquids < high vowels < non-high vowels

Additionally, languages are not required to be organized in a way that allows for the failsafe disambiguation strategies in (53) to apply—following the logic laid out in Chapter 2, such properties are favored over evolutionary time, but they are not necessitated. For this reason, unambiguous trough location, which is already inherently difficult, may not even be possible in principle in a given language.

I argue that when no unambiguous parse is available given the particular parsing algorithm that is in use, no parsing on the basis of phonotactics actually occurs. When the parser doesn't return a word boundary that it should, this will become apparent before too long; it will come across another primary stress, for example. Context and comparisons with the lexicon will in most cases resolve the issue. If not, the result could be a metanalytic change, such as *a napron* becoming *an apron*. If there is no word boundary at stake, the 'underparse' is allowed to remain; this is when we find cases of ambisyllabicity, as in the word *lemon* (as if lem.mon), or variable judgments, as in *agra* (a.gra, ag.ra, or ag.gra) and the similar cases discussed earlier in this chapter. Note that this latter example demonstrates why syllabification cannot be contrastive, a phonological universal we pondered earlier: it is precisely in the case

²²I remain agnostic about the exact nature of sonority, although I am sympathetic to the claim that it is a derived notion. For discussion see, among others, Ohala (1992), Ohala & Kawasaki-Fukumori (1997), Ségéral & Scheer (2008) and references therein.

where two different syllabifications are available in a single language (because multiple parses create possible word beginnings and endings) that an underparse occurs and no boundary is placed at all.

The possibility of underparsing provides an answer to another question raised in §3.3.2, namely why morphophonological processes like reduplication and infixation do not make use of syllable boundaries (we will discuss the empirical evidence in Chapter 4); it also explains why there are virtually no true syllabaries (Poser 2004, Samuels 2005), contra Ladefoged (2001) and Gnanadesikan (2008), and why speech errors fail to provide evidence for the existence of syllables (Shattuck-Hufnagel 2008). This follows naturally from the theory presented here, namely that there are fewer word-internal boundaries than typically assumed. This does not mean we have to give up all the benefits of syllables—we can still distinguish monosyllables from polysyllabic words, for example—but rather, we must understand that we are really just talking about the peaks on a continuous curve, not clearly demarcated, discrete units. I follow Kahn (1976:33ff) on this point:

“There need not correspond to every pair of adjacent syllables [read: peaks—BDS] a well-defined syllable boundary. It would seem reasonable to maintain, then, that while hammer is bisyllabic, there is no internal syllable boundary associated with the word. As an analogy to this view of syllabic structure, one might consider mountain ranges; the claim that a given range consists of, say, five mountains loses none of its validity on the basis of one’s inability to say where one mountain ends and the next begins.”

In many cases, syllable boundaries are simply not present; this is why they cannot be anchors for reduplication and infixation. Interestingly, when they are present—as in a language that disallows consonant clusters, vowel hiatus, and codas (e.g., is

rigidly CVCVCV)—they may be anchors, but this fact is obscured by the fact that there are other possible analyses of the process in question that do not make reference to syllables, as will be discussed in the next chapter (see also Yu (2007:133)). For instance, an infix in the hypothetical language just described might be described as occurring after the first syllable or after the first vowel, and there would be no means of disambiguating between these two possibilities.

3.3.5 Explanation without syllabification

Naturally, if anyone is to take this proposal seriously, it will have to be shown that the processes traditionally described with reference to syllable boundaries can be analyzed in different ways. The phenomena used to motivate the syllable, and to which we must therefore respond, fall into three basic types (here from Redford (1999:7), based on Kenstowicz (1994)):

- (59) Phenomena used to motivate the syllable
 - a. The existence of segment sequencing constraints in language
 - b. Phonological rules, such as those for linguistic stress assignment, are simplified by the concept of syllable
 - c. Certain phonological operations, such as the insertion of an epenthetic vowel, are best understood with reference to syllable structure

I will treat each of these in turn, while keeping in mind the goal of providing more satisfying answers to some of the nagging questions in §3.3.2. Some of these have already been mentioned in the two previous sections, and the remainder will be addressed in the discussion to follow.

Let us first discuss segment sequencing constraints (phonotactics), which have typically been considered to operate on the syllabic level: the onset and rime are taken to be two distinct domains, both subject to well-formedness conditions (e.g., Kahn (1976)), though not to co-occurrence restrictions, as I mentioned earlier. However, in recent years, the possibility of characterizing phonotactic restrictions in a purely string-based manner has garnered increasing attention. This is the approach taken by Steriade (1999), who argues for the independence of phonotactics from syllabification (“Segmental Autonomy”), thus resolving the paradox of why judgments on phonotactics are clear while on syllabification they are anything but. The keystone of her theory is Licensing by Cue (as opposed to the syllable-dependent Licensing by Prosody), the notion that the presence of a particular featural contrast is correlated with its degree of perceptibility in given context. Although I may disagree with the Optimality Theory implementation of Licensing by Cue, there are nevertheless many important insights to be gained from it. One crucial observation is that perceptibility is asymmetric; for instance, consonantal place cues are more perceptible before a vowel than after one (see Ohala (1990) for a summary of the literature), which may partially explain the preference for CV over VC sequences (i.e., the Onset Principle). Complementing the string-based theory of co-occurrence restrictions, Heinz (2007) implements linear phonotactic constraints using finite-state machines. He obtains very good results with his modeling using bigram languages—that is, using machines that consider two consecutive segments—and nearly perfect ones using trigrams (considering three segments in a row).

The second and third arguments for the syllable, those based on phonological rules/operations, are undeniably weak. A representative statement comes from Côté (2000:14): “phonological processes that are expressed with reference to the syllable can always be reformulated in sequential terms.” She discusses a two large categories of processes, epenthesis and deletion, which are often viewed as phonological repairs of ill-formed syllable structures (for instance, syllables which would violate the Sonority Sequencing Principle) and argues extensively that, while syllabically-based approaches to some of these phenomena may be empirically adequate, some are not, and in general such analyses suffer from a number of weaknesses:

(60) Weaknesses of the syllabic approach (Côté 2000:22)

a. *The syllabic approach is insufficient:*

- Epenthesis and deletion often fail to apply in contexts where syllable well-formedness predicts them to be applicable.
- Epenthesis and deletion often apply in contexts where syllable well-formedness does not predict them to be applicable.

b. *The syllabic approach is inadequate:*

Upon closer examination, the syllabic account cannot be maintained for several of the cases of epenthesis and deletion for which it has been proposed.

c. *The syllabic approach is unnecessary:*

For the patterns that are naturally compatible with a syllabic analysis, an equally simple sequential account that makes no use of syllable well-formedness conditions is easily available.

To illustrate these points, Côté (2000:32-33) offers a list of consonant deletion processes which have been claimed to result from stray erasure (i.e., the deletion of unsyllabifiable material). The languages in question include Attic Greek, Diola Fogny, Icelandic, Hungarian, Korean, Turkish, Menomini, Kamaiurá, Basque, Lardil, Québec French, and English. Côté claims that “[t]hese languages can be divided into two main groups. The [Attic Greek, Diola Fogny, Icelandic, and Hungarian cases] appear to be incompatible — or at least clearly problematic — for the Stray Erasure account. For the rest, the syllabic analysis could be maintained, but I argue that an equally simple sequential analysis is available.” Specifically, the constraints banning certain configurations in syllabic terms can be restated sequentially without any loss of generalization:

- (61) Correspondences between syllabic & sequential constraints (Côté 2000:35)
- | | | | |
|----|------------------|--------------------|--|
| a. | Korean/Menomini: | <i>Syllabic:</i> | *COMPLEX (CVC template) |
| | | <i>Sequential:</i> | Consonants are adjacent to vowels |
| b. | Kamaiurá: | <i>Syllabic:</i> | *CODA (CV template) |
| | | <i>Sequential:</i> | Consonants are followed by a vowel |
| c. | Lardil/Basque | <i>Syllabic:</i> | *F/CODA (coda condition) |
| | | | (F a feature or combination of features) |
| | | <i>Sequential:</i> | F is followed by a vowel |
| d. | Québec French | <i>Syllabic:</i> | Sonority does not increase from the nucleus to the edges of the syllable |
| | | <i>Sequential:</i> | Sonority maxima correspond to possible sonority peaks |

All of these sequential conditions are implementable as conditions on the DELETE operation to be formalized in Chapter 4, as we will see: it is possible in the framework developed here to delete a segment provided its neighbors have particular properties. That is, we can state a rule of Kamaiurá: “delete a consonant if it is followed by another consonant.” Similarly for Québec French, we can have a rule which deletes

a consonant word-finally provided its sonority is greater than that of the segment preceding it. This obtains the correct results for final sonorant deletion:

- (62) Final sonorant deletion in Québec French (Côté 2000:22)
- a. /putr/ [pʊt] ‘beam’
 - b. /katefism/ [katefis] ‘catechism’

A parallel approach, using an insertion/COPY procedure also formalized in the next chapter, would produce patterns of epenthesis rather than deletion. Take, for example, epenthesis in Chaha. Unless a final consonant cluster has falling sonority (i.e., (63a-b)), an epenthetic vowel appears. That is to say, the same configuration which triggers deletion in French—a word-final consonant which is more sonorous than its neighbor to the left—triggers epenthesis in Chaha. Again, no reference to syllables is necessary.

- (63) Vowel epenthesis in Chaha (Rose 1997, Côté 2000:22)
- a. /srt/ [sirt] ‘cauterize!’
 - b. /kft/ [kift] ‘open!’
 - c. /dβr/ [diβir] ‘add!’
 - d. /rk’m/ [nik’im] ‘pick!’

Even early proponents of the syllable conceded this point, saying that “all phonological processes which can be stated in a general way with the use of syllable boundaries can also be stated without them” (Vennemann 1972:2). The issue is not that some processes *necessitate* syllable boundaries, but rather that Vennemann, Hooper (1972), and others felt that, in the absence of the syllable, certain analyses served to obscure rather than enlighten.

I see two problems with this line of argumentation. First, the tremendous difficulties that linguists have had over the past century with formulating a coherent

and usable structural (or physiological, or phonetic) definition of the syllable militate against the idea that the appeals to syllable structure in phonological rules are somehow explanatorily deep. A truly deep explanation brings into service the principles of perception, production, cognitive organization, and whatever else may be relevant. It is fine, though not explanatory, to say informally (for example) that consonants delete in coda position, so long as we are clear that this is a shortcut which allows us to concisely express the underlying reasons for the behavior in question, such as the fact that an adjacent consonant masks some of the cues that lead to accurate perception (see references in Ohala (1990)). Such factors influence the shape of languages over evolutionary time (recall our discussion of Blevins (2004) in Chapter 2), creating tendencies that can become phonologized—still in syllable-free terms—so that they operate synchronically.

Secondly, as others have already argued, “conceptual economy, that seeks to minimize the set of primitive notions... [argues] against the syllable as a basic unit in phonology” given the fact that the syllable is never strictly necessary (Côté 2000:14). The illusory ‘simplification’ of phonological rules brought about by positing innate syllable structure comes at a high theoretical cost. It entails radically enriching lexical representations, and/or a phonological module that will syllabify and re-syllabify strings as warranted. This seems too high a price to pay for a construct that can be eliminated without sacrificing empirical coverage. In fact, segment-based analyses are sometimes better than syllable-based ones, as Steriade’s (1999) work on phonotactics demonstrates. As we discussed above, Côté (2000) has done the same for two other processes that traditionally depend on syllabification; she argues that for deletion and

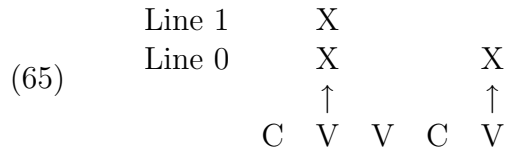
epenthesis patterns, syllabic analyses are not only unnecessary but also insufficient and inadequate. The recent trend of linear analyses continues: Steriade (2008), taking seriously the “interlude theory of weight” advocated by Sturtevant in the early twentieth century, moves away from an entirely syllable-based approach to metrical weight and emphasizes the rhythmic importance of the consonantal interlude, or the distance between two vowels, independent of syllabification. If the string-based theory proposed here is correct, this work certainly represents a step in the right direction. Still, much work remains to be done in this area, particularly with the relationship between syllable peaks, feet, and metrics; at present I can only suggest the following rough sketch.

I assume that some version of the bracketed grid approach (e.g., Halle & Vergnaud 1987 and many others subsequently) is correct and propose no amendments to it here; my only concern at present is how to get the correct set of marks onto the grid at the very beginning. What we need is a procedure to do two things: (a) project vowels/sonority peaks, and (b) project extra gridmarks when certain sequences of consonants appear in order to capture the effect of metrical weight. The first part is simple: project all segments above a particular sonority cutoff (in the normal case, all and only vowels). This is illustrated below.

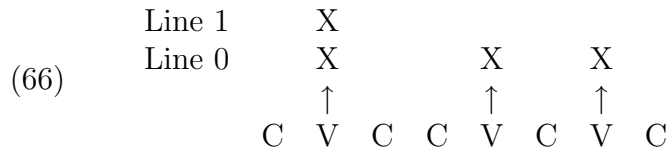
Line 0	X	X	X		X
	↑	↑	↑		↑
	C	V	C	V	C

(64)

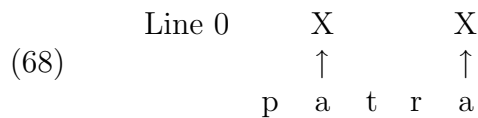
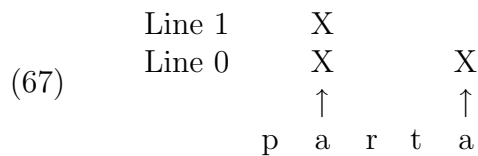
In a language where CVV counts as heavy, we would have a rule which projects to Line 1 if a vowel is immediately followed by another vowel (which does not itself project).



If CVC counts as heavy, in the simplest case a vowel projects to Line 1 if it is followed by (at least) two consonants. Note this yields final extrametricality of the Estonian type (Prince 1980)—CVV and CVCC are heavy in final position but CVC is not.



By adding extra rules to project a final vowel to Line 1 when followed by a consonant, not project a final vowel, etc., extrametricality can be “turned off” and other types of extrametricality can be generated. Additionally, by placing conditions on projection which are tied to sonority, it is possible to distinguish between complex onsets, which should not affect weight, from a coda plus onset sequence which does. For example, contrast the sequences *parta* and *patra* in a fictional language in which CVC counts as heavy. Here is what would happen if a vowel projects to Line 1 only if it is followed by two consonants (C_1 and C_2), and furthermore C_1 is more sonorous than C_2 (i.e., C_1 is a ‘coda’ and C_2 is an ‘onset’).



Though there is no syllabic parsing necessary to achieve this, the result will be as if *parta* is treated as (par)(ta) with a heavy first syllable and *patra* as (pa)(tra)

with two light syllables. Again, while it is beyond the scope of the present study to give a complete theory of metrical stress assignment, the type of picture which I have sketched here is at least in principle possible. This type of account also suggests an answer to why only codas seem to matter for metrical purposes. It is *not* in fact the case that only the coda matters, but rather, the sonority profile of all the consonants is taken into consideration.

The conclusions reached in this chapter about syllables, and also about features, are in some ways separable from the theory of phonological operations and the relationship between syntax, morphology, and phonology which will be explicated in Chapters 4 & 5, as well as the discussion of animal counterparts to phonological representations and operations in Chapter 6. That is to say, if the reader finds the idea of doing phonology without structural syllables to be distressing, he or she need not be discouraged from reading on. Nevertheless, the workings of the SEARCH, COPY, and DELETE operations as proposed in the next chapter depend on there being some degree of underspecification in underlying phonological representations, and the arguments presented in Chapter 5 for abandoning the prosodic hierarchy make more sense if there is no hierarchy in phonology below the word level, either. With these caveats stated, let us now turn to the procedural side of phonology.

Chapter 4

Primitive Operations

4.1 Introduction

Now that we have established the shape of phonological representations (putting aside until Chapter 5 the question of phrase-level groupings), we can now begin formalizing the repertoire of phonological computations which operate over these objects. The goal of the present chapter is to do exactly this.

Three operations will be formalized here: `SEARCH`, `COPY`, and `DELETE`. I introduce the first two using the example of vowel harmony, then show how the `SEARCH` mechanism can be extended in conjunction with `COPY`, `DELETE`, and the concepts of precedence relations and looping representations developed by Raimy (1999, et seq.) to capture a wide range of morphophonological processes including redplication, affixation, and subtractive morphology. I argue that these three parameterized operations yield a restricted typology of possible phonological processes that can achieve the necessary empirical coverage without positing autosegmental tiers or constraints on representations (against, e.g., the crossing of association lines). In connection with this, I discuss how, when, and why looped structures enter the phonological

derivation, and how, when and why they are removed by linearization.

The latter part of the chapter is devoted to a theory of ‘generalized’ SEARCH, COPY, and DELETE which builds on the more specific applications of these operations to harmony and morphophonology, and which casts new light on the parametric theory of rules developed by Archangeli & Pulleyblank (1994). In the course of this discussion, we pick up a loose thread from our discussion of features in the last chapter, namely whether feature geometry is necessary and/or sufficient for explaining the range of attested phonological phenomena. I contrast the standard feature-geometric approach (e.g., Clements 1985) with the algebraic approach of Reiss (2003b,c), arguing in favor of the latter.

4.2 Harmony: search & copy¹

4.2.1 Outlining the problem

Continuing the theme of Chapter 3, in this section I focus on one phenomenon, vowel harmony, which has commonly been thought to require hierarchical or tiered representations. I argue that vowel harmony does not require any such structure, and can be accommodated using a flat/linearly hierarchical model of phonology combined with simple search and copy operations which find parallels elsewhere in linguistics, and which may not even be specific to language (see Ch.6, §6.4).

First, let us discuss the typology of vowel harmony in terms that are as theory-neutral as possible. Vowel harmony takes the general shape in (69), taken from Turkish. A morpheme, typically a suffix, takes on a feature value—here, [α BACK]—

¹This section is based on Samuels (In press).

from a vowel in the root. Thus, we see alternation in the plural suffix between [e] and [a], and in the genitive between [i] and [ɨ] depending on whether the root contains a front or back vowel.

(69) Turkish [BACK] vowel harmony (Mailhot & Reiss 2007:33)

	NOM.PL	GEN.SG	GEN.PL	
a.	ip-ler	ip-in	ip-ler-in	‘rope’
b.	kiz-lar	kiz-in	kiz-lar-in	‘girl’
c.	sap-lar	sap-in	sap-lar-in	‘stalk’

Not every case of vowel harmony is this straightforward (and in fact, as we will see, the Turkish case is itself more complicated than shown above). In some languages, there are ‘opaque’ vowels which do not participate in harmonic alternations, and which appear to block suffixal vowels from obtaining their features from harmonic vowels in the root. Illustrated in (70) is one such case, [ATR] harmony in Tangale. Tangale has a nine-vowel system comprised of the [+ATR] series /i u e o/ and the [-ATR] series /ɪ ʊ ε ɔ a/. In the more abstract left column below, the high back vowel is represented as capital /U/ to indicate its surface alternation between [u] and [ʊ], shown in the middle column.

(70) Tangale [ATR] harmony (modified from Mailhot & Reiss 2007:36)

a.	seb-U	[sebu]	‘look’ (imper.)
b.	kɛn-U	[kɛnʊ]	‘enter’ (imper.)
c.	dob-Um-gU	[dobumgu]	‘called us’
d.	peer-na	[peerna]	‘compelled’
e.	pɛd-na	[pɛdna]	‘untied’
f.	dib-na-m-gU	[dibnamgʊ]	‘called you (pl.)’

The forms in (70a) and (70b) act in a fashion parallel to the forms in (69): The suffixal vowel represented as /U/ takes on a value for [ATR] that matches the value of the root vowel to its left. In (70c) we see that when /U/ appears more than once in a word, each token winds up with the same valuation. (70d) and (70e) show that

[a] does not undergo any phonetic alternation itself, regardless of the [ATR] values of the other vowels in the word. Finally, (70f) speaks to the opaque nature of [a]. When [a] comes between a [+ATR] root vowel and /U/, the suffixal vowel unexpectedly appears in its [-ATR] variant.

In (71) I present another case of [ATR] harmony, from Wolof, which provides an interesting contrast with the Tangale case. In this language, there are seven long vowels (/i: u: e: o: ε: ə: a:/) and eight short ones (/i u e ə o ε ə a/). The high vowels [i] and [u] do not alternate themselves, but they are “transparent” to harmony processes. As above, capital letters in the left column represent vowels with surface alternations; long vowels are transcribed as double (e.g., < ee > = [e:]).

- (71) Wolof [ATR] harmony (Mailhot & Reiss 2007:38)
- | | | | |
|----|--------------|-------------|-------------------------|
| a. | toxi-IEEn | [toxileen] | ‘go and smoke’ (imper.) |
| b. | tekki-IEEn | [tekkilɛɛn] | ‘untie’ (imper.) |
| c. | seen-uw-OOon | [seenuwoon] | ‘tried to spot’ |
| d. | tɛɛr-uw-OOon | [tɛɛruwɔɔn] | ‘welcomed’ |

In Wolof, unlike in Tangale, the non-alternating vowels act as if they simply do not exist for the purposes of computing harmony. The alternating vowels /E/ and /O/ can undergo harmony with the other mid vowels in the root, skipping over [i] and [u].

Consonants may participate in this type of alternation process as well. Here again in (72) Turkish provides an example.

- (72) Turkish [BACK] harmony with laterals (Nevins 2004:40)
- | | NOM.SG | ACC.SG | |
|----|---------------------|-------------------------|---------------|
| a. | usul ^y | usul ^y -ü | ‘system’ |
| b. | sual ^y | sual ^y -i | ‘question’ |
| c. | okul | okul-u | ‘school’ |
| d. | mefgul ^y | mefgul ^y düm | ‘busy’ (past) |

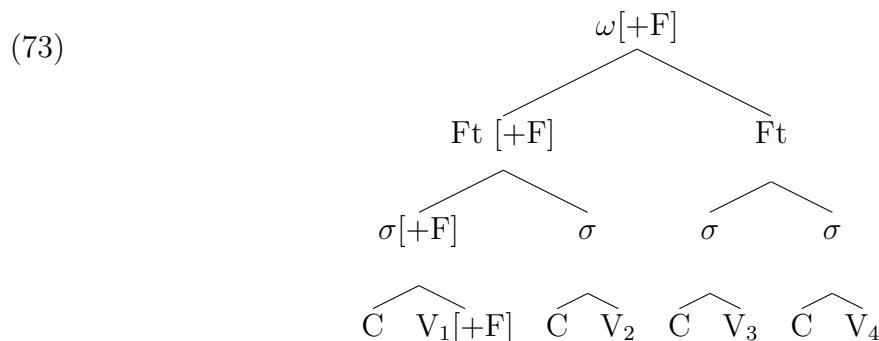
This is the same [BACK] harmony as in (69), illustrated now with different stems and a different case form. Turkish /l/ comes in a [+BACK] version, [l], and a [-BACK] palatalized version, [lʲ]. If a root ends in one of these laterals, a suffixal vowel will take on the backness value of the lateral, regardless of the values of the vowels in the root.

What is the minimum amount of apparatus necessary to account for such phenomena? All theories of which I am aware require a few basic assumptions, like segments composed of features; without features it is difficult to explain the relationship between harmonic vowels, or between alternants (allophones) of a single phoneme. All the theories presented here also assume that alternating vowels are underspecified in that they lack a value for the harmonic feature (recall our discussion of Kalenjin in the previous chapter). Apart from these commonalities, theories vary widely in how they choose to represent opacity, transparency, and the basic harmony process itself. In the discussion to follow, I briefly summarize four representational proposals from the literature that radically differ in the additional assumptions and representations that they require. I conclude by arguing for the proposal that provides the necessary empirical coverage with the least amount of theoretical assumptions, in accordance with the Minimalist aims of this thesis as outlined in Chapter 2.

4.2.2 Feature-percolation account

Halle & Vergnaud (1981) propose a model in which a feature (here, [+F]) percolates up from the nucleus of one vowel—the one that “donates” its feature value to the alternating vowels—to the Prosodic Word (ω) node, and then down through the

feet (Ft) and syllables (σ) to the other vocalic nodes in the word. The prosodic word after upward percolation is shown in (73).



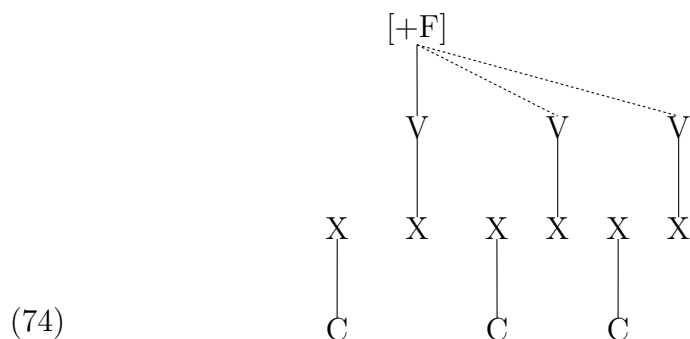
The shortcomings of this model are discussed by Neeleman & van de Koot (2006). One problem is that in addition to upward percolation, it is necessary in this approach to have a mechanism of downward percolation so V_2 , V_3 , and V_4 end up valued [+F] as well. Thus, the feature [+F] must percolate up—and back down—through the syllable, foot, and word nodes, but it is not interpreted in any of those places. Another problem with this model is that it cannot account for cases in which V_2 initially carries [+F], and this feature ends up being expressed only on V_2 and vowels to its right. Concretely, this is manifested by cases we have seen already like (70f) and (72a), in which vowels in the beginning of the word do not harmonize with the suffixal vowel. Because [+F] must percolate all the way up to the word level and then back down from there, it cannot be discriminating in where [+F] is expressed; it will simply result in every vowel receiving that featural specification. Furthermore, as Neeleman & van de Koot (2008) point out, downward percolation of features violates the principle of Inclusiveness, which requires recoverability of the properties expressed on nodes in the hierarchy (Chomsky 1995b).²

²If Narita (2009) is correct, there may in fact be no feature percolation in syntax at all.

In addition to the basic assumptions outlined at the end of §4.2.1, a model like this one requires a multilevel, hierarchical structure of phonological representations. As a result, it also requires some process of linearization of each C and V within a single word, akin to the linearization required to go from a syntactic tree to a linear string of words in a sentence. Finally, it needs a bidirectional mechanism of percolation with copying of the percolated feature onto each node it passes, and some notion of featural interpretability to keep the effects of [+F] confined to terminal V nodes.

4.2.3 Autosegmental accounts

A popular approach (or more properly, set of approaches) to harmony is the autosegmental one advocated by Goldsmith (1976) and many others subsequently. In this type of model, the harmonic feature [+F] is on its own tier and “spreads” by associating with other vowels in the word, as shown in (74). Typically, vowels and consonants appear on separate tiers so that [+F] can be said to target all nodes on the vocalic tier without having to ‘skip’ the intervening consonants.



Note that (74) would not be able to account for cases like (72), in which consonants participate in the harmony process. Nevertheless, setting this issue aside, the success of this kind of model shows that hierarchical structure (a phonological tree) is not

needed to capture the harmony facts. This theory also has the added advantage over the account in §4.2.2 of being able to handle directional cases, in which the source of [+F] is not the edgemoſt vowel and harmony only occurs in one direction. In (73) ſuch representations could not be generated because [+F] muſt percolate downward to every element dominated by ω , the Word node (Neeleman & van de Koot 2006).

While the autosegmental feature-spreading approach does not require hierarchical structure or a linearization procedure—linearity falls out of the theory automatically by virtue the X (timing) tier—it requires a number of different autosegmental tiers, ſeemingly one per contrastive feature in a language (e.g., Halle’s (2005) ‘bottle brush’ model). Rather than percolation and copying of features, it inſtead uses the creation of association lines linking [+F] to elements on the vocalic tier.

4.2.4 Search-based accounts

The autosegmental approach to harmony has been dominant for a number of years. More recently, Nevins (2004) proposed a mechanism that requires leſs structured representations but purports to account for more data (i.e., “disharmony” and diſſimilation processes). It involves a ſearch procedure outlined in (75) that bears obvious ſimilarities to Agree in ſyntax (as in, for example, Chomsky (2000)):

(75) (Dis)harmony ſearches (Nevins 2004:14)

- (i) A newly-introduced item in the derivation needs a value for a feature in order to “converge” at the interfaces
- (ii) Valuation is initiated by a *ſearch*
- (iii) This ſearch attempts to *minimize* diſtance, and find the *cloſeſt* ſource

of valuation

- (iv) This search may be *relativized* to certain values of features

Defective interveners may cause the search to terminate in failure, resulting in “default” valuation

The basic schema is as follows: a segment unspecified for [F] searches in a parameterized direction for the closest source, as defined by precedence relations (Raimy 2000a), that is valued contrastively for [F]. The search can also be bounded to a particular domain, so that it will end in failure and result in default valuation for [F] if an appropriate source is not found within the domain. In a harmony/assimilation process, the source value for [F] is copied onto the initiating segment. In a disharmony (dissimilation) process, the opposite of the source’s value for [F] is copied onto the initiating segment.

‘Copy the opposite’ allows for a unified account of assimilation and dissimilation, but it is a non-trivial and unusual procedure. I argue that, for empirical reasons as well as this theoretical one, the postulation of such an operation is not warranted. Historical linguists have long noted that assimilation tends to be a well-behaved, Neogrammarian-style regular sound change, while dissimilation often applies sporadically (see, e.g., any basic survey of historical linguistics such as Campbell (1999), Hock (1986)). Furthermore, while assimilation can apply to virtually any type of segment, dissimilation is mostly confined to liquids and nasals. And there is no attested language with ‘inverse vowel harmony’ requiring root vowels to be dissimilar from suffixal vowels. Nor is it clear to me that the few cases of (allegedly) regular dissimilation we see in the literature are in fact regular, active synchronic processes.

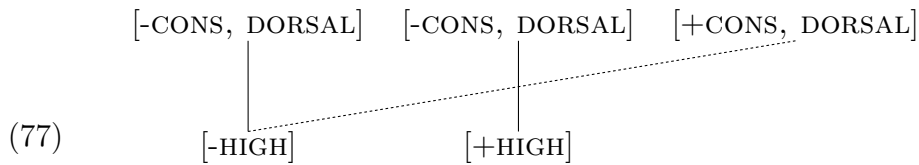
The famous Latin *-al/-ar* case that Nevins (2004) cites, for example: there are numerous counterexamples to the generalization that *-al* dissimilated to *-ar* in the presence of another [l], such as *globalis* and *pluvalis*, which suggest this was not a phonological rule. Instead of treating dissimilation like harmony, maybe we would do best to give it a Blevins (2004)-style diachronic, perceptual account of the type discussed in Chapter 2: this would be like her CHANCE example of [ʔaʔ] being misheard and subsequently mentally represented as /aʔ/ because of an inherent bias against postulating two sources for a single feature that seems to be spread over multiple segments. If, as I have suggested, dissimilation is a very different process from harmony, then there is reason to be skeptical about Nevins' theory. The difficulties with the “copy the opposite” procedure needed to capture dissimilation also argue for selecting a different theory. This is ultimately an empirical issue, however, and the typology of dissimilatory processes clearly requires more thorough investigation.

One way in which search-based models of harmony represent an improvement over autosegmental ones is that they can capture dependencies that run afoul of the No Line-Crossing Constraint on autosegmental representations. This constraint rules out empirically attested cases like the one in (76), which would in autosegmental terms involve spreading of the type shown in (77), where ‘uvularization’ is spreading of [-HIGH]:

(76) Height/dorsalization harmony in Sibe (Nevins 2004:85; 168ff)

Descriptively: a [-HIGH] vowel anywhere in the root triggers uvularization of a suffix-initial dorsal consonant.

- a. ildi(n)-kɪn ‘bright’
- b. muxuli(n)-kɪn ‘round’
- c. sula-qɪn ‘loose’
- d. ulu-kun ‘soft’
- e. tɔndɔ-qun ‘honest’
- f. χɔdu(n)-qun ‘quick’



The + variant of [HIGH] is invisible to the search for a value of [HIGH] for the suffixal consonant. If line-crossing were strictly prohibited, there would be no way to account for this skipping in the association process given that all instances of [HIGH] are on the same tier; a search, on the other hand, can circumvent the issue by referring directly to [-HIGH], ignoring [+HIGH] entirely. The flipside of this coin is ‘defective intervention,’ in which case a search terminates in failure when an otherwise legitimate source has a co-occurring feature that renders it unacceptable as a valuator. Importantly, intervention (following Calabrese (1995)) is parametrically relativized: either all values of [F] intervene, or contrastive values of [F] intervene, or marked values of [F] intervene. Nevins ties intervention to the sonority hierarchy via an implicational universal: if a vowel of sonority n is a defective intervener, then all vowels of sonority $< n$ in the language are, also. In this way phonetics plays a role

in defining what harmony processes are possible.

Essential to the theory is the binarity of features, since ‘copy opposite’ would be impossible otherwise. A theory of precedence is also required so that distance can be defined for the search algorithm, which needs to find the closest instance of a particular feature. As far as the search and copy procedure itself is concerned, this requires processes of searching, copying, and copying a value that is the opposite of the source feature’s value. This last operation would require a mechanism beyond what is required for ordinary copying.

4.2.5 Formalizing Search & Copy

In a recent paper, Mailhot & Reiss (2007; henceforth M&R) provide another way of computing harmony via a search. They begin with the theory of precedence proposed in Raimy (2000a) (as introduced briefly in Chapter 3; we will become acquainted with these representations shortly) and formally define SEARCH and COPY operations over such strings. Here in (78) and (79) is the formalism, where ς and γ when unindexed are feature specifications, and when indexed are tokens of segments with those features:

(78) SEARCH algorithm (M&R 30)

Search($\Sigma, \varsigma, \gamma, \delta$)

1. Find all x in Σ subsumed by ς and index them:

$\varsigma_0, \varsigma_1, \dots, \varsigma_n$

2. For each $i \in \{0, \dots, n\}$:

- (a) Proceed from ς_i through Σ in the direction δ until an element subsumed by γ is found

- (b) Label this element γ_i
3. Return all pairs of coindexed standards and goals, (ς_i, γ_i)
- (79) COPY algorithm (M&R 32)

Identify αF on γ_i and assign αF to ς_i if the set of conditions C on γ_i are satisfied

Assuming underspecification just like the rest of the theories we have seen, only segments that do not already have a specification for the harmonic feature(s) can initiate a search, i.e., ς can only be an alternating vowel. Easy cases like the Turkish [BACK] harmony in (69) thus follow straightforwardly. As yet we have made no provisions for feature-changing harmony, but as we will see, the addition of DELETE to the repertoire of operations makes this formally possible.

Where this theory diverges most notably from previous ones is that opacity and transparency, rather than being taken as properties of vowels (or vowel systems), are properties of the rules of SEARCH. Opaque and transparent vowels have something in common, namely they are already specified for the harmonic feature. Thus, they cannot initiate searches. This allows for a simple account of the Tangale data in (70) and repeated below, in which [a] is opaque: SEARCH to the left for [α ATR], then COPY [α ATR] to the initiator.

- (80) Tangale [ATR] harmony (M&R 36)
- | | | | |
|----|-------------|------------|--------------------|
| a. | seb-U | [sebu] | ‘look’ (imper.) |
| b. | kɛn-U | [kɛnʊ] | ‘enter’ (imper.) |
| c. | dob-Um-gU | [dobumgu] | ‘called us’ |
| d. | peer-na | [peerna] | ‘compelled’ |
| e. | pɛd-na | [pɛdna] | ‘untied’ |
| f. | dib-na-m-gU | [dibnamgʊ] | ‘called you (pl.)’ |

Crucially, this search does not fail or result in default valuation: [a] donates its own [-ATR] feature for copying. That value is not the default in any sense; it results from successful application of SEARCH and COPY just like anything else.

Transparency illustrates the independence of SEARCH and COPY, though ‘copy the opposite’ is still not allowed. So for Wolof, as in (71) and repeated below, we have a SEARCH left for γ specified [-HIGH, α ATR], but only COPY [α ATR] back to ς .

- (81) Wolof [ATR] harmony (M&R 38)
- | | | | |
|----|-------------------------|-------------|-------------------------|
| a. | toxi-IEEn | [toxileen] | ‘go and smoke’ (imper.) |
| b. | tekki-IEEn | [tekkilɛɛn] | ‘untie’ (imper.) |
| c. | seen-uw-OO _n | [seenuwoon] | ‘tried to spot’ |
| d. | tɛɛr-uw-OO _n | [tɛɛruwɔɔn] | ‘welcomed’ |

More complicated data require a combination of this approach plus rule ordering—multiple instances of harmony within a language, like [BACK] and [ROUND] harmony in Turkish, are treated as separate processes (in accordance with Chomsky’s (1967) Principle 6, the force of which we will discuss shortly) so it is possible for them to be ordered with respect to one another, and for unrelated rules to apply between them.

Rounding out the typology, there are cases like Kirghiz, as shown in (82). The Kirghiz vowel system includes the unrounded /i e ɨ a/ and the rounded /ü ö u o/.

- (82) Kirghiz vowel harmony (M&R 42)
- | | ACC.SG | DAT.SG | |
|----|----------|----------|---------|
| a. | taš-ti | taš-ka | ‘stone’ |
| b. | iš-ti | iš-ke | ‘job’ |
| c. | uč-tu | uč-ka | ‘tip’ |
| d. | konok-tu | konok-ko | ‘guest’ |
| e. | köz-tü | köz-gö | ‘eye’ |
| f. | üy-tü | üy-gö | ‘house’ |

The dative suffix, which is specified as [-HIGH], picks up [+ROUND] from a [+HIGH, +BACK] vowel, but not from a [+HIGH, -BACK] one.³ This is captured by a condition on COPY: SEARCH left for [α ROUND], then COPY [α ROUND] if γ is [-BACK]. The difference between Kirghiz and Wolof has been described with the following analogy:

“Suppose you are told to go out into the world, find a man with a hat, and take his hat. On the assumption that there are such things as men with hats and that they are findable, you will always return with a hat. But the outcome is potentially different if you are told to go out, find a *person* with a hat, and take the hat *only if that person is a man*. You may in this case return hatless, if the first behatted person you met was a woman. The first task involved a condition on the search termination — take the hat of the first person you meet who is both a man *and* a hat-wearer; the second involved a condition on the hat-taking (COPY) operation — take the hat of the first hatwearer, only if that person is a man.” (M&R 43)

Kirghiz, with the condition on COPY, corresponds to the “find a person with a hat...” case; Wolof, with its more restrictive SEARCH criteria, corresponds to the “find a man with a hat...” case.

Like the other search-and-copy model, this one can operate over a flat phonology (i.e., it does not need tiers or trees to establish the relations between the donor and recipient segments). However, this model, unlike Nevins’, is substance-free and as such, makes no recourse to phonetics or to any particular qualities of the vowel systems of harmonic languages. It still requires a theory of precedence relations and, of course, search and copy procedures, but it does not require the extra ability to do opposite valuation. This limits the empirical scope of the theory, but as I have argued in the previous section, this does not appear to be undesirable. Of all the theories we have seen, M&R’s makes the least theoretical assumptions while still maintaining

³[BACK] is also independently harmonic.

the advantages that search-based theories have over autosegmental approaches to harmony.

The success of searches over linear strings in accounting for harmony processes demonstrates that there is no need in this domain for hierarchical or tiered phonological representations. These models in fact achieve better empirical coverage than the models that depend on richer structure, with far less machinery. Within the genre of search-based theories, finer distinctions can be made. I argue that the M&R approach covers all the data that should be covered (i.e., harmony/assimilation but not dissimilation/disharmony) with the least amount of apparatus, and the SEARCH and COPY procedures that it does require are independently motivated elsewhere in linguistics (and elsewhere; see Chapter 6): they seem to be used widely in the syntactic module, for instance. We will see in the remainder of this chapter that SEARCH in particular has very broad applications in (morpho)phonology. Indeed, all phonological processes can be captured through a combination of SEARCH paired with two other operations: COPY and DELETE.

4.3 Morphophonology

In this section I attempt to lend further support to the notion that infixation, reduplication, and so-called ‘non-concatenative’ morphology are not substantively different from prefixation or suffixation (see, e.g., Marantz 1982, Raimy 2000a). They can all be captured by a unified mechanism of searching and concatenation of precedence relationships, a procedure which finds parallels elsewhere in phonology, such as in the case of harmony processes which we saw in the previous section. This is the first

step towards extending SEARCH, COPY, and DELETE to account for all phonological processes—a project which I continue in §4.4.

4.3.1 A typology of anchor points

One of the ways in which infixation and reduplication pattern together is that both processes seem to target the same set of attachment points (also called ‘anchors’ or ‘pivots’ by Yu (2003, 2007) and ‘t-junctures’ by Frampton (2004)). Several claims about the pattern of attestation of infixation in particular have been made in the literature. Though there have been many attempts to characterize these pivots, they are almost entirely descriptive and lacking in predictive power. Moravcsik (2000) provides one of the most extensive lists, based largely on data from Ultan (1975):

- (83) Possible infix positions (Moravcsik 2000:547)
 - a. after the first consonant
 - b. after the first consonant or consonant cluster
 - c. after the first vowel
 - d. after the first syllable
 - e. after the second consonant
 - f. after the vowel of the penultimate syllable
 - g. before the final syllable
 - h. before the final consonant

Yu (2003, 2007) develops a more constrained typology of pivot points for infixation, in accordance with the Salient Pivot Hypothesis: “phonological pivots must be salient at the psycholinguistic and/or phonetic level” (Yu 2007:68). In practice, the inventory of salient pivots includes elements that associate with word boundaries or primary stress. The attested pivot points actually comprise a subset of the possibilities predicted by the Salient Pivot Hypothesis. Specifically, the pivots shown in

parentheses in (84) are predicted to be pivots on the basis of their saliency but Yu finds no unequivocal evidence that they are targeted by infixation. I discuss these cases later in this section. Note that Yu presents the only predictive theory (however weakly so); the rest are purely lists of what the authors believe to be attested.

- (84) Potential pivots of infixation (Yu 2007:52)
- | | |
|----------------------|----------------------|
| Edge pivots | Prominence pivots |
| a. First consonant | g. Stressed foot |
| b. First vowel | h. Stressed syllable |
| c. (First syllable) | i. Stressed vowel |
| d. Final syllable | |
| e. Final vowel | |
| f. (Final consonant) | |

Although this will not be the exact inventory of pivots for which I ultimately argue, it is nevertheless important to recognize the apparent validity of the Salient Pivot Hypothesis and consider why it might hold. I suggest that the salience of word edges as pivots specifically results from the use of a positional memory mechanism to encode lexical information, as argued on the basis of experimental evidence by Endress & Mehler (2008a,b). In a positional memory system, the position of each element within a given sequence is encoded relative to the beginning/end of that sequence. If words are stored in such a way, we have a ready account of why, for example, people experiencing tip-of-the-tongue phenomenon tend to remember only the first and/or last phoneme of a word, and why speech errors involving the transposition of phonemes tend to involve the exchange of segments in the same position within the affected words (again, see Endress & Mehler (2008a,b) and references therein). The SEARCH mechanism which we will adopt in this chapter for locating anchor points is also positional in a similar way: since searches necessarily begin at # or % (i.e., the ends of words), any targets which they identify will necessarily be oriented relative

to those endpoints.⁴

Nevins & Vaux (2003), looking at both infixation and reduplication, provide a slightly different typology based on Yu (2003) and the experimental study by Pierrehumbert & Nair (1995), which we will discuss shortly.

- (85) Pivots of infixation & reduplication (Nevins & Vaux 2003)
- | | |
|--------------------|----------------------|
| Edge pivots | Prominence pivots |
| a. First consonant | e. Stressed syllable |
| b. First vowel | |
| c. First foot | |
| d. Final syllable | |

Fitzpatrick (To appear) also begins with Yu's (2003) typology with a few additions:

- (86) Anchor points for reduplication & affixation (Fitzpatrick To appear:13-14)
- | | | |
|--------------------|--------------------|----------------------|
| Initial X | Final X | Prominence points |
| a. First consonant | f. Final consonant | j. Stressed syllable |
| b. First vowel | g. Final vowel | k. Stressed vowel |
| c. First segment | h. Final segment | l. Stressed foot |
| d. First syllable | i. Final syllable | |
| e. First foot | | |

Finally, Frampton (2004), focusing entirely on reduplication, presents a different view: a t-juncture may be inserted to either side of a vowel, consonant, or timing slot that is at the edge of a morphological domain (the stem/root) or a prosodic domain (the leftmost foot). Obviously, this approach differs from the others I have presented in eliminating the entire category of prominence pivots and references to the syllable. Because it is based on autosegmental representation, this model also uniquely allows reference to the first timing slot in a domain. Frampton's typology finds a parallel

⁴We will see later in this chapter that 'subsidiary' or secondary applications of SEARCH, as for the second anchor of an infix, begin from a previously identified target. Still, the point in the main text holds for most applications of SEARCH.

with that of Nevins & Vaux, to the exclusion of the other theories: Frampton and Nevins & Vaux allow reference to the leftmost foot, while for Yu, only a stressed foot can be a pivot, and for Ultan and Moravcsik, a foot is never a pivot.

Below in (87) is a chart summarizing the typologies presented in this section.

(87) Pivots for infixation/reduplication

	Fitzpatrick	Frampton	Moravcsik	Nevins & Vaux	Ultan	Yu
First C	✓	✓	✓	✓	✓	✓
First V	✓	✓	✓	✓	✓	✓
First X	✓	✓				
First σ	✓		✓		✓	predicted
First Ft	✓	✓		✓		
Stressed V	✓					✓
Stressed σ	✓			✓		✓
Stressed Ft	✓					✓
Final C	✓	✓	✓		✓	predicted
Final V	✓	✓				✓
Final X	✓	✓				
Final σ	✓		✓	✓	✓	✓
Second C			✓		✓	
Second σ					✓	
Penult V			✓			

In sum, there are only two infixation pivots upon which the theories presented here agree: the first consonant and the first vowel. The rest of the proposed pivot points range from mildly to highly controversial. We will discuss the data surrounding each in turn.

4.3.2 Controversial anchor points

This section is meant to reduce the chaos of (87) to a single list of anchors that are unambiguously attested, to the extent that it is possible to do so. In pursuit of

this goal we will analyze the empirical data that have been adduced in favor of the various controversial anchors listed in the previous section.

First segment

Frampton (2004) introduces the possibility of infixation after the first segment (timing slot) of a word, regardless of its identity as a consonant or a vowel. Since Frampton uses autosegmental tiers, this is only natural; if a process can target the first timing slot linked to a consonant and the first timing slot linked to a vowel, one would assume that it could also target the first timing slot regardless of what it is linked to.

I can find no clear examples of infixation (or reduplication) occurring after the first segment in Frampton's work; his examples of the "x-rule" largely come from Semitic languages analyzed in such a way that only consonants are present in the stem at the stage when the rule applies. However, Pierrehumbert & Nair (1995) provide experimental evidence for this pivot. They trained subjects to infix /ɪ/ in monosyllables, using models such as 'bin' ~ /bɪn/, 'tag' ~ /tɪæg/ and then tested them on polysyllables with varying stress locations. They found that some subjects (comprising about 32% of responses for both stress categories) produced the infix in coda position for vowel-initial words: 'adobe' ~ /əɪdobi/, which suggests that these subjects generalized from the models to a rule of infixation after the first phoneme. Additionally, one dialect of Pig Latin described by Vaux & Nevins (2003) seems to obey this rule. I give an example of how this particular version of the game works in (88):

(88) Pig Latin targeting the first segment

	<i>Input</i>	<i>Output</i>
a.	car	ar-cay
b.	truck	ruck-tay
c.	oven	ven-o-ay

There are several possibilities for explaining why infixation after the first segment is nonexistent in natural language but achievable in language games and in an experimental setting. The one I will pursue here is diachronic (see below for a similar analysis of the final consonant pivot). It is simply very difficult to find a prefix that comfortably fits in the environments C_V as well as V_C, as an unambiguous case of this pivot would necessitate. Moreover, any language that allows infixation in this position must already accept onset clusters; this means the infix could potentially find itself in a C_C environment, which reduces the possibilities considerably. Therefore I suggest that this pivot is available, just very difficult to utilize.

First syllable

Infixation after the first syllable is expected by Ultan (1975), Moravcsik (2000), and Yu (2007), but empirical data supporting this prediction is famously difficult to find. This observation is paralleled for reduplication: Moravcsik (1978) is credited with discovering that reduplication never copies just the first syllable of a word. It may copy a CV chunk or a CVC chunk, but in the latter case, when the first syllable is open, the copy will include a consonant from the next syllable.⁵ As we discussed

⁵Of course, as I mentioned in the previous chapter, in languages which do not (putting it in traditional terms) allow codas, many cases of infixation and reduplication are ambiguous between targeting a certain syllable or the corresponding vowel; only theory-internal considerations can distinguish between these options.

in Chapter 3, this makes sense if syllables are not afforded with primitive status in phonology; morphophonology cannot therefore refer to them. But whether we can do without syllables in characterizing morphophonological processes is at heart an empirical question, so we must pay careful attention to the cases of infixation and reduplication which have been claimed to require explanation in syllabic terms.

Yu (2007:133ff) gives three examples that are suggestive of infixation after the first syllable. Of these, two (Koasati *-ho-* and Mandarin *-li-*) are quite restricted in the stems to which they can affix, and Yu is rightly skeptical of these unproductive patterns. A more solid set of data comes from Cantonese, which appears to have two separate infixation processes (involving the intensifier *-kwai-* and the *wh*-words *-matkwai-* and *-mɛ-*) that can infix after the first syllable of monomorphemic words, including loanwords. These are illustrated in (89) below.

(89) Cantonese post- σ_1 infixation (Yu 2007:134)

	<i>-kwai-</i>		
a.	lɔn.tsɔn	lɔn-kwai-tsɔn	‘clumsy’
b.	jɔk.syn	jɔk-kwai-syn	‘ugly’
	<i>-matkwai-</i> & <i>-mɛ-</i>		
c.	jɔk.syn	jɔk-matkwai-syn	‘ugly’
d.	ku.hɔn	ku-matkwai-hɔn	‘stingy’
e.	ki.li.ku.lu	ki-mɛ-likulu <i>or</i> kili-mɛ-kulu	‘gibberish’

In light of the Cantonese data, we are again left wondering what the status of this pivot should be. Given that Cantonese metrical feet are typically monosyllabic (Wong et al. 2005), and particularly in light of the form in (89e), it is tempting to analyze these infixes as targeting a foot boundary, which is a better-attested pivot. At the very least, then, these cases are ambiguous, and in the absence of any other supporting evidence I believe they should be classified as cases of infixation at a foot boundary (discussed later in this section).

If we treated (89) as genuine cases of infixation after the first syllable, we would be at a loss to explain why unambiguous cases of infixation at syllable boundaries are extremely thin on the ground, not only for the first syllable, but also for all other syllables, and why they are totally nonexistent for reduplication, as has been duly noted by Moravcsik (1978), Broselow & McCarthy (1983), and McCarthy & Prince (1995).

Haugen (2008a,b) claims that the Uto-Aztecan languages Yaqui and Mayo have processes of first-syllable reduplication. However, he notes that in Yaqui, “which reduplicant [i.e., CV, CVC, or a different shape—BDS] goes with which stem is not predictable from the phonological make-up of the stem, and must be somehow lexically-stipulated” (Haugen 2008b:3). This is tantamount to conceding the point that there are separate processes of CV and CVC reduplication, both independent of syllable boundaries. In Mayo, Yaqui’s closest relative, the “accented” class of words with initial stress shows free variation between first-CV reduplication and heavy syllable reduplication, which includes any first-syllable coda and otherwise involves gemination (see Haugen 2008a:41). The behavior of the “unaccented” class, which copies into the second syllable, casts doubt on this analysis. When the reduplicant is taken to be a foot in all words, a unified analysis of both classes becomes possible, as argued by Hagberg (1993). This has the further advantage of explaining other facts about Mayo phonology that only make sense in light of degenerate footing in the accented class.

First foot

Fitzpatrick (To appear), Frampton (2004), and Nevins & Vaux (2003) admit the possibility of infixation after the first foot. Frampton and Yu provide multiple examples of infixation and reduplication which they claim target this juncture: for instance, the Cantonese *wh*-infixation discussed above could be analyzed in this manner. Closer to home, English expletive insertion (McCarthy 1982) also occurs at the juncture between two feet. Very long words like *multiplication* allow for optional infix placement (at least in some dialects), as in *multi-fuckin-plication* alternating with *multipli-fuckin-cation*, which demonstrates at the very least that expletive insertion does not target only the first foot (see Yu (2007:120); cf. also Davis (2005)). However, multiple competing analyses are still available for this case; they could be disambiguated by testing this process with words having upwards of four feet.

However, some of the examples claimed to refer to the foot can be analyzed in other ways. Frampton shows a process in Orokaiva in which he says the first foot is reduplicated, and the reduplicant is then placed after any unfooted material at the left edge of the stem. A vowel-initial syllable is taken to be unfooted. The result of this is that in a V-initial word, the reduplicant appears as an infix, whereas in a C-initial word, it is prefixed.

(90) Orokaiva verbal reduplication (McCarthy & Prince 1986[1996]:11)

	<i>Base</i>	<i>Reduplicated</i>	
a.	waeke	wa-waeke	‘shut’
b.	hirike	hi-hirike	‘open’
c.	tiuke	ti-tiuke	‘cut’
d.	uhuke	u-hu-huke	‘blow’

Discussing this case, McCarthy & Prince (1986) parse (d) as *uh-uh-uke*, but in

the 1996 version of the same work, they say that this is almost certainly incorrect, and instead adopt the infixation analysis described above. This is significant because there are two possible ways to capture this pattern which do not require reference to feet, depending on which parse is correct. If the infixation account is right, then we can view this process as reduplication of the string from the first consonant to the next vowel. If McCarthy & Prince's original thought is correct, then we could say that this is reduplication from the left edge through the second segment, and this would be the only evidence for such a pivot. I know of no way to tease these two accounts apart empirically.

Stressed constituents

As we have seen, a number of typologies (Frampton 2004, Moravcsik 2000, Ultan 1975) disregard the possibility of prominence pivots entirely. It is true that the data arguing in favor of stressed elements as pivots are subtle and rare. Yu (2007:118) notes that

“[l]ogically, there are six possible edges [of stressed elements —BDS] an infix can target: the left edges of a stressed foot, a stressed syllable, or a stressed vowel, and the right edges of those respective units. However, clear examples that can substantiate this six-way typology are hard to locate. . . . [T]he edges of the different stressed pivots often coincide.”

It is not just that different stressed pivots often coincide; stressed pivots and edge pivots also often coincide. Many cases that could potentially involve prominence pivots also involve edges: this is only natural since stress itself is edge-bound. A further difficulty with isolating instances of prominence pivots lies in the fact that few stems in any language have enough feet/syllables to allow for unambiguous analysis.

However, Yu (2007:122ff) provides clear examples of infixation both before and after a stressed vowel: Nakanai nominalization (infix *-il-* before the stressed vowel) and one variant of the Upriver Halkomelem plural (infix *-le-/-l-* after the stressed vowel).

(91) Upriver Halkomelem plural infixation

- a. lhóqwet lhó-le-qwet ‘wet something(s)’
- b. kw’és kw’é-le-s ‘(many) got burned’

(92) Nakanai nominalization

- a. il-áu ‘steering’
- b. t-il-ága ‘fear’
- c. g-il-ógo ‘sympathetic’

None of the examples in Yu’s book involving infixation near stressed constituents other than vowels seem to be unambiguous, which is perhaps why his §4.7 contains mostly examples of internal reduplication: reduplication can be anchored to the stressed vowel and copy a word chunk that mimics a syllable or foot.

Final consonant

Infixation before a final consonant is also very difficult to find in the field, though it is expected by all but one of the typologies in (87). Ultan (1975) cites three cases of infixation before the final consonant, namely Proto-Indo-European, Ket, and Tzeltal. To this Yu (2007) adds two different infixes in Hunzib and one from Hausa. Additionally, Frampton (2004) gives at least one example (Creek adjectival pluralization) in which a CV reduplicant is attached before the final consonant of a stem.

I concur with Yu (2007:133) that this pivot is probably rare for diachronic reasons: “Languages often have restrictions on codas. In particular, coda clusters are often disfavored or banned altogether. Thus the contexts in which a final consonant pivot

can be unequivocally established are difficult to obtain.” Furthermore, following Ullman (1975), Yu (2003, 2007), and many others, we know that metathesis is a major diachronic source of infixation (and see Halle (2008) for a synchronic account which reduces both metathesis and infixation to special cases of reduplication). Very few pairs of consonants are able to swap positions without running afoul of the Sonority Sequencing Principle (Sievers 1876, Clements 1990), which is a non-trivial consideration given that infixes consisting of a single (sonorant) consonant are the most common type (Moravcsik 2000).

Final vowel

Fitzpatrick (To appear), Frampton (2004), and Yu (2007) consider a stem-final vowel to be a pivot. I concur. Most of Yu’s multiple unambiguous examples of processes targeting either side of the final vowel are instances of internal reduplication, but he also shows two unambiguous examples of pure infixation to the left of the final vowel.

Final segment

Only Fitzpatrick (To appear) and Frampton (2004) consider it possible to target the position before the final segment of a stem. Frampton sometimes formulates rules that refer to the stem-final segment, but when he does this (e.g., for Temiar and Semai; see discussion also in Raimy (2000a)), the rules could just as easily be made to refer to the final consonant since stems in these languages, as Frampton admits, are invariably consonant-final. For example, in Semai forming the continuative involves (from a descriptive point of view) copying both the initial and final consonants such

that the following is obtained:

(93) Semai continuative (Frampton 2004:162)

	<i>Root</i>	<i>Continuative</i>	
a.	bʔəl	b-l-bʔəl	‘painful embarrassment’
b.	dɨɔh	d-h-dɨɔh	‘appearance of nodding’
c.	kmrʔɛ:c	k-c-kmrʔɛ:c	‘short, fat arms’

Perhaps we should not be surprised at the lack of evidence for this pivot, since its closest relatives—the initial segment pivot and the final consonant pivot—are also rare birds (noted by Moravcsik (2000); we will discuss this shortly). I can see no principled reason for ruling the final segment pivot out entirely, so at this stage it seems most prudent to consider it a possible, albeit highly improbable, anchor point. Of course one hopes that either further research will provide empirical evidence for this pivot, or that future theories will find a reason why it cannot exist at all.

Final syllable

Infixation before the final syllable is expected by all the theories presented here, except for Frampton (2004). The confirmed existence of this pivot would be particularly striking given the dearth of evidence for the other purported syllable pivots. Yu (2007:108ff) gives cases of infixation and reduplication before the final syllable from each of two different languages: Koasati verbal pluralization and punctual reduplication, and the Tigre intensive and frequentative. However, none of these examples turn out to require reference to the final-syllable pivot.

(94) Koasati plural infixation (Kimball 1991:327)

	<i>Singular</i>	<i>Plural</i>	
a.	aká:no-n	aká-s-no-n	‘to be hungry’
b.	maká:li-n	maká-s-li-n	‘to open the eyes’
c.	stipí:la-n	stipí-s-la-n	‘to be sexually attractive’

- (95) Koasati punctual reduplication (Kimball 1991:325)
- a. aló:tka-n alot-ló:-ka-n ‘to be full’
 - b. copóksi-n copok-có:-si-n ‘to be a hill’
 - c. talásba-n talas-tó:-ba-n ‘to be thin’

First, Kimball (1991) notes that the pluralization process in (94) is not productive, but is restricted to only ten verbs. Second, in both (94) and (95), the singular forms are actually bimorphemic: *-n* is the switch-reference marker. Thus, if infixation occurs before suffixation, these are cases of infixing before the final consonant.

- (96) Tigre intensive (Rose 2003:112, 115)

<i>Stem</i>	<i>Intensive</i>	
a. dənzəz-	dən-a:-zəz	‘to be (very) numb’
b. mərmər-a:	mər-a:-mər-a:	‘to examine (thoroughly)’
c. fəntər-a:	fən-a:-tər-a:	‘to scatter (many) seeds’

- (97) Tigre frequentative (Rose 2003:112, 115)

<i>Stem</i>	<i>Frequentative</i>	
a. dənzəz-	dənə-za:-zəz	‘to become (a little) numb’
b. gərf-a:	gə-ra:-rəf-a:	‘to whip (a little)’
c. məzz-a:	mə-za:-zəz-a:	‘to give (a little) responsibility’

Halle (2008) discusses the case of Tigre in some detail. The first thing to note is that this is a Semitic language, and the schwas in (96)-(97) are in predictable locations; also, the final *-a:* shown on some forms is suffixal. We can (and should) disregard these, as Halle does, in the formulation of the infixation/reduplication rules. Once we have limited ourselves to consonants in this way, the notion of syllable becomes irrelevant. However, it does require us to refer to the penultimate consonant of the stem. In the intensive, the infix gets placed before the penultimate consonant; the same occurs in the frequentative, but the penultimate consonant is itself reduplicated. (This is somewhat different from the terms in which Halle discusses these processes, which for him involve metathesis; see Halle (2008:342ff).)

In short, neither the Koasati examples nor the Tigre examples require reference to syllables. Yu reports one additional case of infixation before the final syllable in KiChaga, the analysis of which is extremely uncertain. The syllabification which Yu gives for the stems is indicated by dots. He does not indicate morpheme boundaries in the stems so I have added them (as dashes), but the dashes flanking the infixes in the intensive forms are his. Note that (a-c) are adjectives, while (d-g) are verbs; the verbs have a suffix *-a*, and the form in (g) also contains the reciprocal infix *-an-*.

(98) KiChaga intensive infixation (Yu 2007:108, as reported to S. Inkelas by L.

Moshi)

	<i>Stem</i>	<i>Intensive</i>	
a.	lyi.an.gu	lyian-n-gu	‘light’
b.	mu.il.i	mui-n-li	‘white’
c.	mu.i.u	mui-n-u	‘black’
d.	-ka.pa	-ka-n-pa	‘hit’
e.	-o.lon.g-a	-olon-n-ga	‘point’
f.	-aam.bi-a	-aambi-n-a	‘look at’
g.	-aam.bi-an-a	-aambia-n-na	‘look at each other’

First, the syllabification shown in (98) defies conventional wisdom about both Bantu in general and Chaga in particular (see, e.g., McHugh 1999); the only consonant clusters allowed in Chaga are nasal + homorganic obstruent onsets, and no codas are allowed. Moreover, if the syllabification reported for the stems were correct, then (b) would be in need of explanation. If the infix is really placed to the left of the final syllable, why is the intensive form of *mu.il.i* not **muil-n-i*? But we could grant this as a simple typo or mistake. The more pressing problem is that we don’t know *which -n-* is infixes in (a), (e), and (g). This is not trivial because if we assume, contra Yu, that the *first -n-* in the intensive forms in (a) and (e) is the infix, then there is an exceptionless generalization to be made that makes no reference to syllables:

infix after the penultimate vowel. (With the correct syllabification, Yu would have to amend his analysis in the same way.) We will see shortly that Alabaman provides an almost exactly parallel example.

Another ambiguous case for the final syllable as an anchor comes from plural reduplication in Kaingáng (Wiesemann 1972, Poser 1982, Steriade 1988, Halle 2008). There are a number of ways to make the plural form of a verb in Kaingáng, including infixation, ablaut, reduplication, and all possible combinations thereof. I show only reduplication alone here, since these less complicated examples are sufficient to illustrate the point at hand.

(99) Kaingáng plural reduplication (Poser 1982:5)

	<i>Singular</i>	<i>Plural</i>	
a.	vã	vã-vã	‘to throw away’
b.	jẽmĩ	jẽmĩ-mĩ	‘to grasp’
c.	kry	kry-kry	‘to itch’
d.	vãsãn	vãsãn-sãn	‘to exert, fatigue’
e.	mrãn	mrãn-mrãn	‘to strike’

This case has been analyzed by Wiesemann, Poser, Steriade, and Halle as involving reduplication of the final syllable, with Marantz (1982) providing a dissenting opinion (the pluses and minuses of which I will not discuss here, since it would take us quite far into the realm of copy-and-associate approaches to reduplication). But note that the forms in (99) are consistent with the following informal explanation: reduplicate from the end of the word to the right of the penultimate vowel, if there is one, or the whole stem otherwise.⁶ Though an analysis in terms of syllables would allow us to collapse these two conditions, the penultimate-vowel analysis has the advantage of

⁶There is a potential counterexample given by Steriade (1988:76): *jengag-gag*. But this is glossed as ‘roasted meat’ so what relation this could have to the process which makes a verb plural is unclear.

allowing a single pivot to account for for infixation and vowel raising which also are used (either alone or in conjunction with this type of reduplication) to make some verbs plural, and both of which target the penultimate vowel. We will see examples of this infixation when we discuss the penultimate-element pivots.

Second elements

Ultan (1975) cites the Syrian Arabic augmentative and frequentative suffixes and “a very few examples” from Miskito as cases of infixation which use the second consonant as a pivot. For Syrian Arabic, Moravcsik presents an alternative analysis: if the base is syllabified CV.CVC, the infixes can be said to come after the first syllable. However, as we have seen, there are no unambiguous cases of infixation in this position; the Arabic case is no different and should likely therefore be categorized as infixation after the first vowel, which is amply attested. (More in line with the above analysis of Tigre, we could also do the infixation after the first consonant, ignoring the vowels.) We will see later, though, that there is no way to rule out the second consonant as a pivot, so this case remains formally ambiguous.

The Miskito infixes in question, which mark inalienable nouns for first and second person, normally come after the first vowel, as in (100a). In a certain phonologically predictable set of cases we find the morphemes surfacing as suffixes (see Yu (2007:104)) and a mere handful of cases in which they surface as an infix after the second vowel.⁷

⁷I do not know why Ultan (1975) characterizes this infixation as occurring after the second *consonant*; the forms clearly show this is not the case.

(100) Miskito person infixation (Lin 1994:36)

	<i>Stem</i>	<i>First person</i>	<i>Second person</i>	
a.	byara	bya-i-ra	bya-m-ra	‘abdomen’
b.	maispakaia	maisa-i-pakaia	maisa-m-pakaia	‘to inform’
c.	maisnikaia	maisa-i-nikaia	maisa-m-nikaia	‘to comfort’

It is quite relevant that all but one of Lin’s cases with infixation in the second syllable begin with *mai-*, and the remaining example is uncontroversially bimorphemic: *ninara* ‘behind’ = *nina-* ‘behind’ + *-ra* ‘in, into, on, onto, upon.’ An ‘infix’ after *nina-* could just as easily be analyzed as a suffix added before *-ra*. The more relevant cases, then, are the ones shown in (100b,c) above. *Mai-* is a free morph in the language, though Lin claims that, in terms of semantics, it cannot be analyzed as such in these cases. However, the possibility that *mai-* is still being treated as a prefix cannot be dismissed offhand, and if this is the case, the forms in (100b) and (100c) are unexceptional.

A better case of reduplication which extends to the second vowel of the word comes from Diyari, which reduplicates nouns and verbs for a number of purposes:

(101) Diyari reduplication (Poser 1982:7)

	<i>Stem</i>	<i>Reduplicated</i>	
a.	ɲama	ɲama-ɲama	‘to sit’
b.	wakari	waka-wakari	‘to break’
c.	kanku	kanku-kanku	‘boy’
d.	ɲankaŋ̩ti	ɲanka-ɲankaŋ̩ti	‘catfish’
e.	ɕilparku	ɕilpa-ɕilparku	‘bird (sp.)’

Note that if the target of reduplication were a two-syllable chunk (or perhaps even a foot), we would expect that the ostensive codas in (d) and (e) would be copied. Poser (1982) noted that this was not the case, and proposed that this follows from a restriction on word-final codas—that is, that the first two syllables are reduplicated in full, and then the edge of the reduplicant (which forms its own word) is adjusted

to remove any consonantal material at the right edge. While this is possible, it seems more complicated than the analysis I suggest.

There is another case of reduplication, in Oykangand, which is minimally different from the Diyari example shown above: here, the material up to but *not* including the second vowel is copied.

(102) Oykangand reduplication (McCarthy & Prince 1986[1996]:12)

	<i>Stem</i>	<i>Reduplicated</i>	
a.	eder	ed-eder	‘rain’
b.	algal	alg-algal	‘straight’
c.	igu-	ig-igun	‘go’

This is very similar to the case of Mangyarrayi (Raimy 2000a, Halle 2008), which reduplicates the string from the first vowel up to, but not including, the second vowel:

(103) Mangyarrayi plural reduplication (Raimy 2000a:135)

	<i>Singular</i>	<i>Plural</i>	
a.	gabuji	g-ab-abuji	‘old person(s)’
b.	jimgan	j-img-imgan	‘knowledgeable one(s)’
c.	yirag	y-ir-irag	‘father(s)’
d.	wan̩gij	w-an̩g-an̩gij	‘child(ren)’

As evidence for the second consonant as a pivot, we can add the reduplicative processes which copy a CVC chunk (i.e., from the beginning of the word through the second consonant). We saw one of these earlier, from Yaqui.

Penultimate elements

Finally, infixation that seems to occur after the penultimate vowel is found in the Alabama language, but again, the data are ambiguous. As Moravcsik (2000) notes, it is quite possible to analyze the Alabaman infix as occurring before the final syllable.

But there is another possible analysis of the data, which is shown in (104) below, namely that the infix occurs before the final consonant.⁸

(104) Alabaman comparative infixation (Hardy & Montler 1954:385-386)

	<i>Positive</i>	<i>Comparative</i>	
a.	kano	ka-h-no	‘to be good/better’
b.	caaha	ca-h-ha	‘to be tall/taller’
c.	laawa	la-h-wa	‘to be lots/more’
d.	kasatka	kasa-h-ka	‘to be cold/colder’
e.	cacobaci	cacoba-h-ci	‘I am big/bigger’

As should be apparent from (104), which is an exhaustive list of the examples reported by Hardy & Montler, one cannot definitively say that reference to syllabification is necessary to account for this phenomenon. In fact, there is no way to tease apart the penultimate-vowel analysis from the final-syllable one because the infix ‘overwrites’ what would be a coda of the penultimate syllable, as seen in (d); we could prove or disprove the final-consonant analysis by looking at what happens to consonant-final forms.

However, since we have now analyzed the Tigre intensive and frequentative as making use of a penultimate consonant pivot, and in light of the analysis I have suggested for KiChaga, we would be remiss to disregard the possibility of a penultimate vowel pivot. The case of Kaingáng provides additional evidence. Steriade (1988) calls this case “insertion of *g* into the coda of the penultimate syllable,” but in all the

⁸Since CVVC syllables are allowed in Alabaman, the vowel shortening associated with infixation is not automatic.

examples given, anchoring the infix to the right of penultimate vowel is sufficient:^{9,10}

(105) Kaingáng plural infixation (Steriade 1988:76)

	<i>Singular</i>	<i>Plural</i>	
a.	kavi	ka-g-vi	‘to stretch’
b.	kana	ka-g-na	‘to be dried out’
c.	jeten	je-g-ten	‘to strike with a hammer’
d.	juryn	ju-g-ryn	‘to sharpen’

4.3.3 A new typology

Integrating the above data, we arrive at a new typology of infixation and reduplication, which I present below. Of course, some of the possibilities are extensionally equivalent or very difficult to tease apart. Where the only evidence for a particular pivot is ambiguous, I have marked that cell with a question mark.¹¹

⁹Though all examples given here could be analyzed as infixation after the first vowel, longer forms such as *jakygjen* from *jakajen* (featuring both infixation and vowel raising) illustrate that the penultimate vowel analysis is the correct one.

¹⁰Steriade characterizes *jengyg-gag* as exhibiting all three pluralization processes at once: raising of the penultimate vowel, infixation, and reduplication. This would again provide the lone counterexample. But here there is no evidence of infixation at all; the /g/’s in the base are both underlying. Since some plurals do exhibit vowel raising and reduplication without infixation, there is no problem as analyzing this form as such—putting aside the fact mentioned in an earlier footnote, that this does not appear to be a verbal form at all.

¹¹One might question my decision to leave the syllable pivots off the chart, given that there is some ambiguous evidence for them. However, it is worth noting that *all* the evidence for the syllable pivots is ambiguous, whereas there is unambiguous evidence for each of the other pivots (C, V, X, foot) in almost every position (first, second, stressed, penult, and final)—with the exception of the foot, for which crucial data is lacking, but it is certain that processes referring to feet in at least one of the other positions is attested. If incontrovertible evidence for the syllable pivot is found, this

	Affixation	Reduplication
First C	✓	✓
First V	✓	✓
First X	✓	✓
First Ft	✓	✓
Second C	?	✓
Second V		✓
Second X		?
Stressed V	✓	✓
Stressed Ft	?	?
Penult C	✓	✓
Penult V	✓	
Final C	✓	✓
Final V	✓	✓
Final X	✓	✓
Final Ft	?	?

Unfortunately, there are some gaps in the data that prevent us from fully completing the chart in (106). In addition to the juncture after the first foot, we saw earlier that other foot junctures are also possible pivots. It remains to be discovered whether there is any interaction between the direction of footing and pivothood (i.e., whether Nevins & Vaux's (2003) statement about the "first foot" should be taken literally). Additionally, we do not yet know whether it is sufficient to refer to just the stressed foot or just the final foot, or whether both pivots are necessary. From the English expletive insertion data presented above and in consideration of symmetry, it seems likely that the full three-way range is required, though this is not yet assured.

Given that reduplication and infixation are considered to be closely related processes in the theory advocated here as well as many others, any differences between the two must be explained. If there is genuinely infixation after the first syllable, this would provide another contrast with reduplication, since there appears to be no

can be accommodated with minimal disruption to the theory.

reduplication of the syllable proper (Moravcsik 1978). However, given that only one language seems to use this pivot, it seems premature to conclude anything about whether this pivot is ruled out entirely, or whether it is just very difficult to use, as I suggested earlier. There is also evidence to suspect that reduplication can in fact target either side of the first vowel, but for some reason it can or does not target the first consonant, as Yu (2007:89) argues: “no unequivocal cases of a reduplicative infix appearing to the right of the first consonant are found. All potential instances of infixing a reduplicant after the first consonant can equally well be analyzed as sub-categorizing for the first vowel of the output.” The example Nevins & Vaux (2003) give for reduplication after the first consonant is Mangarayi, which Yu (2007:92) notes could also be analyzed as a case of reduplication before the first vowel. In the absence of onset clusters, these two cases are impossible to differentiate; I am not aware of any cases that must unequivocally be analyzed as reduplication after the first consonant. However, note that attested dialects of Pig Latin which minimally differ from the one presented in (88) with respect to their treatment of vowel-initial words demonstrate use of this pivot (Vaux & Nevins 2003, Idsardi & Raimy 2005).

Moving forward, I will operate under the assumption that all the junctures listed in (106) are possible pivots, but that some are less probable than others, not because of anything inherent to the mechanism responsible for generating infixation/reduplication, but because it is more difficult for them to develop diachronically (in the spirit of Chapter 2).

4.3.4 Loop morphophonology

In undertaking the task of formalizing the procedures which yield reduplication and affixation (and other morphophonological processes as well), I believe it is helpful to begin with a summary of Raimy’s (1999, et seq.) conception of the connection between reduplication and affixation, and the way these processes can be characterized as operating over linear strings of phonological elements.¹² To the extent that the arguments laid out in Chapter 3 and earlier in the present chapter (also in Chapter 5) that phonology is ‘flat’ are tenable, it will be necessary to reformulate all phonological processes to be compatible with a flat phonology. But even if those larger arguments fail, the mechanisms described here are still quite viable. After this introduction to the formalism, we will see how to constrain it using the typology of anchor points developed above.

Reduplication can be very informally defined as a process that causes some portion of a word to appear twice. A given instance of reduplication can be either total or partial—in many languages both appear, with different morphological functions. Just as the base may contain elements not found in the reduplicant (“partial reduplication”), the reduplicant may likewise contain elements not found in the base (“fixed segmentism”).

Raimy (1999, 2000a,b) develops a framework in which reduplication stems from linearization requirements in phonology. Each word comes specified with precedence relations (denoted by arrows or as ordered pairs; $X \rightarrow Y$ and (X, Y) are both read as

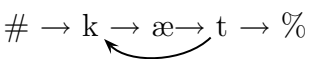
¹²I will not discuss ‘templatic’ or ‘copy-and-associate’ approaches to reduplication here. See Poser (1982), Raimy (1999, 2000a) for arguments against these methods.

“X precedes Y”) ordering all its elements. In the usual case, a lexical representation consists of a linear string initiated by the symbol # and terminated by %:

$$(107) \quad /kæt/ \text{ is shorthand for: } \quad \# \rightarrow k \rightarrow \text{æ} \rightarrow t \rightarrow \%$$

or as ordered pairs: $(\#, k), (k, \text{æ}), (\text{æ}, t), (t, \%)$

However, the morphological component may direct the insertion of a new precedence relationship, which may create a loop (specifically here a ‘backward’ loop) in the string:

$$(108) \quad \text{Add } (t, k): \quad \# \rightarrow k \rightarrow \text{æ} \rightarrow t \rightarrow \%$$


In this case, the direction that /t/ precedes /k/ has been added, while the instructions that /t/ precedes % and that # precedes /k/ remain. This creates a set of precedence relations for this string that is ‘non-asymmetric’ (see discussion in Raimy (2003)). If precedence is to be asymmetric, then if A precedes B, the statement B precedes A can never be true. But notice that by adding the direction that /t/ (directly) precedes /k/, while it is still the case (via transitivity) that /k/ precedes /t/. The precedence relations in this string are therefore no longer asymmetric.

Raimy argues, following Kayne’s (1994) asymmetry hypothesis for syntax, that a non-asymmetric phonological representation must be made asymmetric, as all phonological output is on the surface. (Think of this as a bare output condition.) In other words, all loops must be linearized. This linearization process will be the subject of §4.3.6. For now, let it suffice to say that as many precedence relationships as possible, and in particular all the morphologically-added relations, are represented in the final linear structure, and that the route taken is also economical. We will see in

§4.3.6 that this process is in fact deterministic, even if it may not seem so on the basis of this description. For the structure in (108), the result of linearization (which indeed preserves every precedence relation) is what we would characterize as total reduplication:

$$(109) \quad \# \rightarrow k \rightarrow \text{æ} \rightarrow t \rightarrow k \rightarrow \text{æ} \rightarrow t \rightarrow \% = /k\text{æ}t k\text{æ}t/$$

There is no principled distinction between total and partial reduplication in this theory: total reduplication happens when a loop just happens to establish precedence between the segment after $\#$ and the segment before $\%$.¹³

One great success of Rainy’s approach (see in particular Rainy (2000b)) is that it accounts for both reduplication and affixation with exactly the same mechanism. Consider now a case which differs minimally from the reduplication examples discussed above: X directly precedes Y, and a morpheme Z is inserted with the instructions “X directly precedes Z and Z directly precedes Y.” This situation is illustrated in (110):

$$(110) \quad \text{Add (X, Z), (Z, Y):} \quad \# \rightarrow \begin{array}{c} X \rightarrow Y \rightarrow \% = XZY \\ \quad \quad \quad \curvearrowright \\ \quad \quad \quad Z \end{array}$$

Unlike the example in (108), the loop in (110) is in the ‘forward’ direction. The precedence relations of the loop cannot be realized while all the precedence relations

¹³Admittedly, it is not clear how this could explain an implicational universal which was first noted by Moravcsik (1978), namely that the presence of partial reduplication in a language implies that it also has total reduplication. However, the answer may lie in the idea that the positions of all segments are encoded with respect to the ends of the string (recall our discussion of positional memory earlier in this chapter). The ends themselves are thus easiest to locate.

in the root are also realized: specifically, the newly-added relations conflict with the direction that X directly precedes Y. Because the instructions regarding the placement of Z come from the morphology, these ‘m-links’ are realized at the expense of their lexical counterparts. Thus, (110) produces a string XZY, or in other words, XY with an infix Z. Thus, infixation and reduplication are two sides of the same coin: the former derives from a forward loop and the latter derives from a backward loop. Prefixation and suffixation are the special cases in which a forward loop involves the addition of a forward loop between # and the first lexical segment, or between the last lexical segment and %. This predicts a high degree of parallelism between reduplication and affixation, which is precisely what we saw in the previous section.¹⁴

Forward loops can also be used to account for ‘templatic’ or ‘non-concatenative’ morphology (a true misnomer), which on this view is simply multiple infixes (see discussion in Raimy 2000a:32ff). It also provides a mechanism for subtractive morphology, which has been explored by Gagnon & Piché (2007); see also Gagnon (2008). Indeed, the possibility of having a ‘jump link’ in which a forward loop skips one or more lexical segments, effectively deleting the skipped string, cannot be ruled out without additional stipulations. One case which can easily be accommodated using this approach is the Tohono O’odham perfective:¹⁵

¹⁴It should be noted that Raimy is hardly the first—or the last—to unify reduplication and infixation; McCarthy (1979) and Marantz (1982) had previously argued in favor of this approach (though see Raimy (2000a) for a criticism of the templatic copy-and-associate method they used, which prevents reduplication and affixation from being truly integrated with each other), and more recently, so have Frampton (2004) and Halle (2008).

¹⁵A few vowel-final stems do not conform to this pattern; in these cases, the imperfective and the

(111) Tohono O’odham perfective (Zepeda 1983, Gagnon & Piché 2007)

	<i>Imperfective</i>	<i>Perfective</i>	
a.	hi:nk	hi:n	‘bark(ed)’
b.	ñeid	ñei	‘see/saw’
c.	golon	golo	‘rake’
d.	si:sp	sis	‘nail(ed)’
e.	ʔí:i	ʔí:	‘drink/drank’
f.	mo:to	mort	‘carry/carried’

From the above data it should be obvious that the final segment in the imperfective is not predictable from looking at the perfective forms, which suggests an analysis of the perfective in terms of subtraction from the imperfective. As Gagnon & Piché (2007) have argued, when viewed in this light, the rule for formulating the perfective is very simple: delete the final segment. In loop notation, this is represented as below:

(112) $\# \rightarrow g \rightarrow o \rightarrow l \rightarrow o \rightarrow n \rightarrow \%$

Since the m-link takes precedence over the lexical ones, the result will be deletion of the final segment.

Halle (2008) adds metathesis to the list of processes which can be described in these terms. (The notation Halle uses is different; however, the result holds for loop notation as well.) For example, the string ABC can be transformed into BCA by the following:

(113) $\# \rightarrow A \rightarrow B \rightarrow C \rightarrow \%$

Another of the claimed advantages of Raimy’s formalism is that it can account for ‘backcopying’ effects known since Wilbur (1973). The general pattern of these

perfective are identical. Following the literature, I assume these exceptional forms to be lexicalized.

cases is that a phonological process for which the structural description is only met in one copy actually applies in both the base and the reduplicant. This is demonstrated below with the example of Malay nasalization: [NASAL] on either a vowel or consonant spreads rightward to all the following vowels, but is blocked by oral consonants (/ʔ, h, w, y/ are transparent to this process).

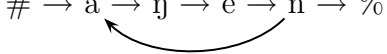
(114) Malay nasalization with reduplication (Raimy 2000a:16)

- | | | | | |
|----|------|------------|-----------|----------------------|
| a. | hamẽ | ‘germ’ | hãmẽ-hãmẽ | ‘germs’ |
| b. | waŋĩ | ‘fragrant’ | wãŋĩ-wãŋĩ | ‘fragrant (intens.)’ |
| c. | aŋãn | ‘reverie’ | ãŋãn-ãŋãn | ‘ambition’ |
| d. | aŋẽn | ‘wind’ | ãŋẽn-ãŋẽn | ‘unconfirmed news’ |

We see in the reduplicated forms that the first vowel is nasalized even though on the surface, it is not preceded by a nasal segment. Neither nasalization before copying, nor copying before nasalization, will yield the correct results: the former would give *aŋẽn-aŋẽn, and the latter would give *aŋẽn-ãŋẽn.

Raimy’s innovation was to show that the attested pattern can be seen to follow from certain assumptions about how phonological rules can apply to looped representations. For example, take the case of aŋẽn:

(115) # → a → ŋ → ẽ → n → %



Here, the initial /a/ is preceded by a nasal, /n/. However, it is not ‘exhaustively’ preceded by /n/; it is also preceded by #. Nevertheless, nasalization still applies to /a/. Remember that neither applying nasalization before copying, nor copying before nasalization, will yield this result. But if instead we allow nasalization to apply to the *looped* representation and assume that being ‘non-exhaustively’ or ‘non-uniformly’ in the triggering environment is sufficient to trigger nasalization, then we

can explain the Malay pattern. We will further discuss this and other, similar cases in §4.3.6, ultimately concluding that allowing phonological rules to apply to looped representations is actually unnecessary from an empirical standpoint and undesirable from a theoretical one.

4.3.5 From strings to loops

Now that we have seen how adding precedence relationships in this way can account for a wide variety of morphophonological processes from reduplication to subtractive morphology, I would like to connect this to the typology of anchor points developed earlier in this chapter. This will address the questions of how, why, and where loops enter phonological representations; these questions were not addressed in Raimy (1999, 2000a), as Downing (2001) has pointed out. Fitzpatrick (To appear) and Raimy in subsequent work (2005, 2008a) have both attempted to answer the ‘where’ question. Since Fitzpatrick adopts a set of pivots which I take to be incorrect (on the basis of the conclusions developed earlier in this chapter), I will not discuss this proposal any further. However, Raimy’s parametric approach to anchor point theory is close in spirit to the one for which I argue, and therefore warrants discussion. After I describe my own view, we will see how it differs from Raimy’s.

If we take seriously Marantz’s (1982) view that reduplication involves the introduction of a phonetically null morpheme, but depart from Marantz by not assigning any skeletal structure to that morpheme, it becomes possible to treat reduplication and affixation as initiating SEARCH in order to find a host for a newly-introduced morpheme. In other words, each time a string enters the phonological workspace,

before anything else happens, it must be combined with the string which is already present. The details of *when* this occurs will be discussed in the context of Distributed Morphology and syntactic phases in the next chapter. For now, however, we should note that, contra Fitzpatrick (2006), I advocate for the view that there are no looped representations in the lexicon (see Gagnon (2007) for empirical and theoretical arguments in support of this conclusion). Just as I argued in the previous chapter that underspecification is not driven by economy of lexical representation, the same can be said of loops. They are created only when it is necessary to concatenate two morphemes, as will be specified in the discussion to follow.

The SEARCH algorithm I will use is the same one which Mailhot & Reiss developed for harmony, with one additional variable, and we will see that their COPY mechanism can be used to integrate the precedence relationships carried by the new morpheme with the ones already present in the host word. This combination of SEARCH and COPY creates looped representations of exactly the type we discussed in the previous section. However, by virtue of the SEARCH mechanism itself and the values of the parameters which it takes, the variety of representations which are generable end up closely matching the typology in (106).

First, for the sake of concreteness, let's consider the case of suffixing *want* with the past tense *-ed*. I assume that what it means to be an affix is to lack one or both terminal elements, # and %. This means there is a "sticky end" on the affix which enables it to concatenate with another string. In more formal terms, the sticky end is a variable (which I label as ς for reasons that will soon become clear), and concatenation of the two strings is achieved by replacing that variable in the

affix with information copied from the existing string—exactly how underspecified representations are repaired in Mailhot & Reiss’s view. So in the lexicon, *want* and *-ed* are represented as below:¹⁶

- (116) a. # → w → a → n → t → %
 b. ζ → e → d → %

The role of ζ is exactly the same as it was when it represented a segment with an underspecified feature value in the case of harmony; that is, ζ is the ‘standard’ or the initiator of a search into the string (Σ) which is already in the workspace. The goal (γ) of SEARCH will ultimately replace ζ via an application of COPY, eliminating the sticky end and integrating the two morphemes. We desire, then, for the values which γ can take to reflect the possible anchor points for affixation (and of course for affixation and the other morphophonological processes). These anchors, as we established in (106), are the {first, second, stressed, penult, last} elements of type {X, C, V, foot} in the string.^{17,18} As we saw earlier, the SEARCH algorithm has a direction parameter (δ) which allows it to traverse the string either to the left or to the right. This means there are really only three positions to consider, namely {first,

¹⁶In this section I represent example morphemes as they are spelled to keep things as simple as possible.

¹⁷Putting feet aside, since so few words have a large enough number of feet to see the pattern of attestation clearly, of the possible combinations of these two sets of parameters, only two—stressed C (for obvious reasons) and penult X—are completely unattested.

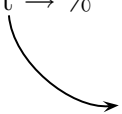
¹⁸This means γ could be broken into two subparameters, one specifying the position and the other specifying the type of element. I keep the single γ here to make the parallelism with other types of searches, for which this distinction is not relevant, more transparent.

second, stressed}. The ‘last’ and ‘penult’ are just the first and second from the right, respectively. (We will address a minor complication to this typology momentarily.)

Going back to our example of *want* + *-ed*, then, the parameters on the SEARCH specified in the lexical representation of *-ed* would be as follows:

- (117) a. Σ (string in the active workspace):
 $\# \rightarrow w \rightarrow a \rightarrow n \rightarrow t \rightarrow \%$
 b. ς (initiator of SEARCH):
 $\varsigma_i \rightarrow e \rightarrow d \rightarrow \%$
 c. γ (target of SEARCH):
 First X
 d. δ (direction of SEARCH):
 L (i.e., beginning at %)

Upon completion of this search, the target /t/ will be copied into the precedence statement which contained the initiating ς :¹⁹

- (118) $\# \rightarrow w \rightarrow a \rightarrow n \rightarrow t \rightarrow \%$

 $e \rightarrow d \rightarrow \%$

The difference between a suffix like *-ed* and an infix is that the former contains %, while the latter has two sticky ends. I give an example from Tzeltal below. In this language, a verb is made intransitive by infixing *-h-* after the first vowel:

- (119) Tzeltal intransitivization (Yu 2007:102)
- | | | | | |
|----|------|-------------------|---------|----------------------|
| a. | puk | ‘to divide among’ | pu-h-k | ‘to spread the word’ |
| b. | kuč | ‘to carry’ | ku-h-č | ‘to endure’ |
| c. | k’ep | ‘to clear away’ | k’e-h-p | ‘to be clear’ |

¹⁹Note that Raimy (2000a) explicitly states that two morphemes which combine, such as *want* and *-ed*, actually come to share a single beginning/end marker (i.e., in any type of affixation there is one instance each of # and %). I show two tokens of % to emphasize that a prefix/suffix has only one ‘sticky end’ as opposed to an infix which has two.

There are a few important things to note about the way SEARCH proceeds for infixation. First and foremost, since there are two sticky ends, there must be *two* applications of SEARCH, which share the same search space (Σ) and direction (δ) but not the same standard (ς) or target (γ). Secondly, for all cases of (pure) infixation, the target identified in the first search is the *starting point* for the next search. I represent this by adding one additional parameter to SEARCH, β , which indicates the point from which the search is initiated, and I suggest that the value of β can be only one of the terminals or the target of the previous search ($\#, \%$, or γ_{n-1}). This effectively divorces the beginning point of SEARCH from the target of COPY, which is necessary when one considers that the affix to be added is not yet integrated into the string in the phonological workspace when SEARCH takes place (that integration, of course, being the result of SEARCH and COPY); if ς is not in the string which will be scanned, then it is impossible for SEARCH to begin from there.

In this particular case, the first search is to the right from $\#$, for the first vowel in the word. But rather than beginning at $\#$ again for the second search, the starting point is that first vowel in Σ which was just identified (i.e., the previous γ). The second search then proceeds to the right, looking for the first segment it encounters. This ensures that the two sticky ends are attached to adjacent segments, and thus the m-link which is created does not have the effect of deleting anything in the base. (Though keep in mind that we do not want to stipulate that the second application of SEARCH is *always* ‘first X from previous γ .’ We want to allow for a jump link to be created when the second search looks for some other target.) The end result is as desired, taking *puk* as our example:

$$(120) \quad \# \rightarrow p \rightarrow \begin{array}{c} u \rightarrow k \rightarrow \% \\ \curvearrowright \\ h \end{array}$$

Formally, this can be expressed with the following parameters:

- (121) a. Σ (string in the active workspace):
 $\# \rightarrow p \rightarrow u \rightarrow k \rightarrow \%$
 b. ς (initiator of SEARCH):
 $\varsigma_i \rightarrow h \rightarrow \varsigma_j$
 c. γ (target of SEARCH):
 γ_i : First V
 γ_j : First X
 d. δ (direction of SEARCH): R
 e. β (beginning point of SEARCH):
 β_i : #
 β_j : γ_i

Now let's look at what happens in reduplication. We first have to distinguish between reduplication which does not add any material to the original string and reduplication with fixed segmentism. In both cases, the affix enters with two sticky ends. However, this affix is extremely abstract if no fixed segmentism is involved: it consists only of the precedence relation $(\varsigma_i, \varsigma_j)$. And unlike what we just saw for affixation, the second SEARCH in a case of reduplication can either begin afresh at one of the terminal nodes, *or* begin from the target of the first search. English *shm*-reduplication (here, *fancy-shmancy*) provides an example of the first type, where the two searches are totally independent (and in fact even have different settings for δ , which we have not yet seen up to this point).

- (122) a. Σ (string in the active workspace):
 $\# \rightarrow f \rightarrow a \rightarrow n \rightarrow c \rightarrow y \rightarrow \%$
- b. ς (initiator of SEARCH):
 $\varsigma_i \rightarrow sh \rightarrow m \rightarrow \varsigma_j$
- c. γ (target of SEARCH):
 γ_i : First X
 γ_j : First V
- d. δ (direction of SEARCH):
 δ_i : L
 δ_j : R
- e. β (beginning point of SEARCH):
 β_i : %
 β_j : #

Note that the ‘backwardness’ of the loop is an epiphenomenon resulting from the accidental fact that γ_i happens to precede γ_j . Nothing about the shape of the affix, or about any one particular parameter setting, guarantees this result. We can verify this by considering the case of a language with final stress where both searches begin at # and proceed to the right, but the first terminates at the stressed vowel and the second terminates at the second vowel. This produces a backward loop. Conversely, in a word with initial stress, the exact same SEARCH parameters will result in a forward loop:²⁰

- (123) From stressed vowel to second vowel (final stress)

$$\# \rightarrow C \rightarrow V \rightarrow C \rightarrow V \rightarrow C \rightarrow \acute{V} \rightarrow \%$$

²⁰Searching for the stressed vowel is always ambiguous as to direction; since there is only one primary stress in a word, the result will be the same either way. Which choice(s) from among the multiple possible parameter settings learners actually entertain is a different question entirely, and one which I feel it is premature to answer.

(124) From stressed vowel to second vowel (initial stress)

$$\# \rightarrow C \rightarrow \acute{V} \rightarrow C \rightarrow V \rightarrow C \rightarrow V \rightarrow \%$$

Kamaiurá presents an example from the opposite end of the reduplication spectrum: no fixed segmentism, but the target of the first search provides the beginning for the second search.

(125) Kamaiurá aspectual reduplication (Yu 2007:111)

Singular Plural

- a. omokon omoko-moko-n ‘he swallowed it (frequently)’
- b. ohuka ohuka-huka ‘he (kept on) laughing’
- c. jeumirik jeumiri-miri-k ‘I tie up (repeatedly)’

(126) a. Σ (string in the active workspace):

$$\# \rightarrow o \rightarrow m \rightarrow o \rightarrow k \rightarrow o \rightarrow n \rightarrow \%$$

b. ς (initiator of SEARCH):

$$\varsigma_i \rightarrow \varsigma_j$$

c. γ (target of SEARCH):

γ_i : First V

γ_j : Second C

d. δ (direction of SEARCH):

δ_i : L

δ_j : L

e. β (beginning point of SEARCH):

β_i : %

β_j : γ_i

There is one more issue which must be addressed, namely the difference between reduplicative patterns like the kind exhibited by Diyari on the one hand and the one found in Oygangand (and Mangarrayi) on the other. In Diyari, the whole stem up to *and including* the second vowel is reduplicated, whereas in Mangarrayi, the whole stem up to *but not including* the second vowel is reduplicated. I repeat the data here:

(127) Diyari reduplication (Poser 1982:7)

	<i>Stem</i>	<i>Reduplicated</i>	
a.	ɲama	ɲama-ɲama	‘to sit’
b.	wakari	waka-wakari	‘to break’
c.	kanku	kanku-kanku	‘boy’
d.	ɲankɑ̃̀ti	ɲanka-ɲankɑ̃̀ti	‘catfish’
e.	ɕilparku	ɕilpa-ɕilparku	‘bird (sp.)’

(128) Oygangand reduplication (McCarthy & Prince 1986[1996]:12)

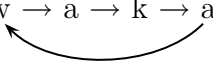
	<i>Stem</i>	<i>Reduplicated</i>	
a.	eder	ed-eder	‘rain’
b.	algal	alg-algal	‘straight’
c.	igu-	ig-igun	‘go’

The Diyari pattern is handled straightforwardly by the theory as it has been presented up to this point. Take *wakari* as an example:

- (129) a. Σ (string in the active workspace):
 $\# \rightarrow w \rightarrow a \rightarrow k \rightarrow a \rightarrow r \rightarrow i \rightarrow \%$
- b. ς (initiator of SEARCH):
 $\varsigma_i \rightarrow \varsigma_j$
- c. γ (target of SEARCH):
 γ_i : Second V
 γ_j : First X
- d. δ (direction of SEARCH):
 δ_i : R
 δ_j : R
- e. β (beginning point of SEARCH):
 β_i : #
 β_j : #

The result will be as desired:

(130) $\# \rightarrow w \rightarrow a \rightarrow k \rightarrow a \rightarrow r \rightarrow i \rightarrow \%$



However, to capture the reduplication in Oygangand which excludes the second vowel, what we need is a SEARCH procedure to establish a loop between the first segment in the string and the segment *to the left of* the second vowel. This was

already noted by Raimy (2005, 2008a), who proposes a ‘Placement’ parameter which can be set to establish a loop at, before, or after an anchor point. This could be formally implemented in my theory by a ‘subsidiary SEARCH’ which searches from the segment identified in one iteration of SEARCH for the first X to its left or right, then establishes a precedence relation from that segment to one identified by another SEARCH. We will independent motivation for such a process later in this chapter, during our discussion of phonological rules. At the present time I have no choice but to stipulate that a subsidiary SEARCH (in the context of reduplication) is always for the first X—i.e., an adjacent segment. Without this restriction, it should be possible for SEARCH to identify the second vowel, then subsidiary SEARCH to find the first vowel after that; this would effectively yield a ‘third vowel’ pivot, which is unattested. As we will see later in this chapter, though, subsidiary SEARCH for something other than ‘first X’ is necessary as a condition on certain types of phonological rules.

Before moving on to the question of how loops are ultimately linearized, I would like to discuss the differences between the theory presented here and the one developed by Raimy (2005, 2008a), to which I just alluded. Raimy also posits a system of parameters which constrain m-link placement. The parameters Raimy proposes are:

(131) Anchor Point Theory parameters for structural description (Raimy 2005:6)

- a. Placement: {at/before/after}
- b. Count: {first/last}
- c. Plane: {x-tier/metrical/syllable/consonantal}
- d. Target: {plain/stressed(head)/consonant}

Raimy’s parameters do not correspond to the parameters on SEARCH in a neat

way: for example, I consider {first, second, stressed} to be different settings of a single parameter γ , while for Raimy, reference to the penultimate element is obtained by setting the Placement parameter to ‘before’ and the Count parameter to ‘last.’²¹ This method of deriving the second/penultimate elements does not seem to go far enough: I can see no way to find the segment before the second vowel (the Oykan-gand/Mangarrayi pattern) using Raimy’s parameters. Another difference between my theory and Raimy’s is that I obtain reference to elements defined relative to the tail end of a string by setting the δ parameter to L and the β parameter to %, whereas Raimy’s mechanism always looks from the left. This may seem trivial, but I believe it is not. If we believe that a search algorithm is at work, then it is undesirable to refer to the last item which is found, since the search will have to traverse the entire string in order to determine which this is. Searching from the opposite end for the first item will achieve the same results but in a more economical fashion. Additionally, not only in order to avoid requiring tiered representations but also for theory-independent considerations of symmetry, I prefer not to treat vowels and consonants differently, as in (131): for Raimy, consonants are accessed by setting the Target parameter to

²¹Raimy achieves references to foot-like chunks in this fashion: setting one anchor to, for example, {after, first, metrical, plain} =peninitial vowel. Only these chunks, not true feet, are relevant to reduplication in Raimy’s view. I find this unsatisfactory in light of the cases discussed in the main text, which seem to involve true metrical feet. Apart from this, the two theories therefore make different predictions about words with lots of feet. I can refer to the first, second, stressed, penultimate, and final feet, but Raimy can only refer to initial, final, and possibly stressed pseudo-feet. Since I am not aware of data which shows the typology of foot-based reduplication clearly, I set this issue aside for the time being.

‘consonant,’ whereas vowels are accessed by setting the Plane parameter to ‘metrical.’

4.3.6 From loops to strings

In the previous section we discussed how to get loops, or non-asymmetric precedence relations, into phonological representations. Now we turn to the matter of how to *remove* loops. As I mentioned earlier, Raimy has pursued the idea that the linearized output is the shortest path through the looped string, as many precedence relations as possible are realized (concretely, this means taking backward loops as soon as possible), and where there are conflicts, the m-links are realized instead of lexical links. Fitzpatrick (2006) formalizes the first two of these principles with a fixed ranking of OT constraints which he calls ECONOMY and COMPLETENESS, plus an additional constraint, SHORTEST, which ensures that, when multiple nested loops begin at the same point, the shorter one is taken first. But there is a way to get the same results without any constraints, and this is the approach I will pursue: the linearization algorithm I adopt is Idsardi & Shorey’s (2007) modified version of Dijkstra’s shortest path algorithm (Dijkstra 1959).

At the core of Idsardi & Shorey’s approach is the idea that we should dissociate precedence statements from the objects to which those statements refer. We can think of the set of segments represented in the precedence statements as the phonological equivalent of the numeration in syntax. While the numeration is just an unordered set, the precedence statements are organized in an ordered queue. By making this distinction, we reduce the problem of linearizing a phonological string to finding the shortest path through the set of connected vertices (= segments). The algorithm

always begins with the start symbol, #, and then scans the queue until it finds the highest precedence statement which begins at #. It then moves along the path specified by that statement, outputs the result, and de-prioritizes the newly-traversed path by moving it to the bottom of the queue. Next, starting from its new position, the algorithm again scans from the top of the queue until it finds a path which begins at its current location. This is repeated until the termination symbol, %, is reached. I illustrate a simple example of this procedure step-by-step with the word /kæt/ below.

(132) Vertices: {#, k, æ, t, %}

Initial queue:

- 1) # → k
- 2) k → æ
- 3) æ → t
- 4) t → %

Step 1 (begin at #)

Traverse path: # → k

Output: # → k

New queue:

- 1) k → æ
- 2) æ → t
- 3) t → %
- 4) # → k

Step 2 (begin at /k/)

Traverse path: k → æ

Output: # → k → æ

New queue:

- 1) æ → t
- 2) t → %
- 3) # → k
- 4) k → æ

Step 3 (begin at /æ/)

Traverse path: æ → t

Output: # → k → æ → t

New queue:

- 1) t → %

- 2) # \rightarrow k
- 3) k \rightarrow æ
- 4) æ \rightarrow t

Step 4 (begin at /t/)

Traverse path: t \rightarrow %

Output: # \rightarrow k \rightarrow æ \rightarrow t \rightarrow %

Algorithm halts.

In this particular example, since each vertex was the starting point for only one path in the queue, the order of the statements in the queue did not actually matter; I leave it to the reader to verify that any ordering of statements would have yielded the same output. But, as we are about to see, this is not the case when there are non-asymmetric precedence statements involved. Consider what would happen if we were to add one additional precedence statement, (t, k), to the queue. If it were added in the lowest-priority position, at the bottom of the original queue, the result of running the algorithm would still be /kæt/ because the algorithm would find (t, %) first and halt as a consequence, never traversing (t, k). But if we added this new statement *above* (t, %), something very different would happen:

(133) Vertices: {#, k, æ, t, %}

Initial queue:

- 1) t \rightarrow k
- 2) # \rightarrow k
- 3) k \rightarrow æ
- 4) æ \rightarrow t
- 5) t \rightarrow %

Step 1 (begin at #)

Traverse path: # \rightarrow k

Output: # \rightarrow k

New queue:

- 1) t \rightarrow k
- 2) k \rightarrow æ
- 3) æ \rightarrow t

- 4) $t \rightarrow \%$
- 5) $\# \rightarrow k$

Step 2 (begin at /k/)

Traverse path: $k \rightarrow \text{æ}$

Output: $\# \rightarrow k \rightarrow \text{æ}$

New queue:

- 1) $t \rightarrow k$
- 2) $\text{æ} \rightarrow t$
- 3) $t \rightarrow \%$
- 4) $\# \rightarrow k$
- 5) $k \rightarrow \text{æ}$

Step 3 (begin at /æ/)

Traverse path: $\text{æ} \rightarrow t$

Output: $\# \rightarrow k \rightarrow \text{æ} \rightarrow t$

New queue:

- 1) $t \rightarrow k$
- 2) $t \rightarrow \%$
- 3) $\# \rightarrow k$
- 4) $k \rightarrow \text{æ}$
- 5) $\text{æ} \rightarrow t$

Step 4 (begin at /t/)

Traverse path: $t \rightarrow k$

Output: $\# \rightarrow k \rightarrow \text{æ} \rightarrow t \rightarrow k$

New queue:

- 1) $t \rightarrow \%$
- 2) $\# \rightarrow k$
- 3) $k \rightarrow \text{æ}$
- 4) $\text{æ} \rightarrow t$
- 5) $t \rightarrow k$

Step 5 (begin at /k/)

Traverse path: $k \rightarrow \text{æ}$

Output: $\# \rightarrow k \rightarrow \text{æ} \rightarrow t \rightarrow k \rightarrow \text{æ}$

New queue:

- 1) $t \rightarrow \%$
- 2) $\# \rightarrow k$
- 3) $\text{æ} \rightarrow t$
- 4) $t \rightarrow k$
- 5) $k \rightarrow \text{æ}$

Step 6 (begin at /æ/)

Traverse path: æ → t

Output: # → k → æ → t → k → æ → t

New queue:

1) t → %

2) # → k

3) t → k

4) k → æ

5) æ → t

Step 7 (begin at /t/)

Traverse path: t → %

Output: # → k → æ → t → k → æ → t → %

Algorithm halts.

If we stipulate that the precedence relations established by SEARCH and COPY are always added to the top of the queue, the result will be exactly what we desire: an asymmetric string which realizes the m-links in preference to lexical material, and is economical. These properties result from how the algorithm operates; they do not need to be represented by constraints in the grammar.

There is another issue of linearization with which we need to grapple, namely how to account for the effects of Fitzpatrick's (2006) SHORTEST constraint. The effect of this highly-ranked constraint in his system is to ensure that when there are two loops which begin at the same point, the shorter one is taken first. The primary evidence for such a constraint comes from the interaction of two types of verbal reduplication in the Salishan language Lushootseed. The way to make a verb in Lushootseed distributive is to reduplicate up through the second consonant (i.e., CVC or CC; see Broselow (1983)), and there is also a process of 'out-of-control' reduplication that copies the first VC. Let us assume along with Fitzpatrick, at least temporarily, that the loops created by both of these processes are present in the representation at the same time.

Then the root *bali-* ‘to forget’ when undergoing both types of reduplication and prior to linearization would be:

$$(134) \quad \# \rightarrow b \rightarrow a \rightarrow l \rightarrow i \rightarrow \%$$

On the face of it, there are two ways to linearize this string. We could take the ‘inner’ (l, a) loop first and then the ‘outer’ (l, b) loop, which would produce *bal-al-bali*. This is the derivation that is preferred by SHORTEST. Alternatively, we could take the outer loop first and then the inner one, yielding *bal-bal-ali*. This derivation would violate SHORTEST. It so happens that the attested form in Lushootseed is *bal-al-bali*. I am not aware of any other cases of multiple reduplication in which two loops which begin at the same point, so as far as I know, the *bal-bal-ali*-type pattern is unattested and Lushootseed stands alone in exhibiting the *bal-al-bali*-type pattern.

As I see it—lacking any evidence for a minimally-differing language with forms like *bal-bal-ali*—there are two ways to obtain this result in a manner which is consistent with the theory developed in this dissertation. First, one could accept that both loops are present at the same time and find some property of the linearization mechanism which guarantees that the inner loop is taken first (i.e., reduce the ostensive constraint to an emergent property of the algorithm and/or representations). This might be attributable to some Third Factor principle, and would make reduplication akin to repeat notation in music, in which the innermost of two repeats is always performed first. Alternatively, one could argue that the order of affixes entering the derivation is always distributive first, then out-of-control, and the *bal-al-bali* pattern is attributable solely to this fact. I argue in what follows that the latter view is correct—in other words, that there could easily be a language with forms like *bal-bal-ali*.

The reason why Fitzpatrick argues that both loops are present in the representation at the same time is that, according to him, there are two attested hierarchical orderings of the distributive (DIST) and out-of-control (OOC) morphemes:

- (135) a. [DIST [OOC \sqrt{root}]]
 b. [OOC [DIST \sqrt{root}]]

Fitzpatrick claims that these two possibilities correspond to two distinct semantic types which he identifies. (135a) produces “semantics that could be paraphrased as something like ‘many X’s involved in random, ineffectual action,’” whereas with regards to (135b) “it seems that OOC has little or very subtle effects on the semantics of DIST forms” (Fitzpatrick 2006:20). Examples of the two types are given below.

	[DIST [OOC \sqrt{root}]]	[OOC [DIST \sqrt{root}]]
(136)	$saq^w \rightarrow saq^w-aq^w-saq^w$ ‘fly’ \rightarrow ‘many flying around’	$bali \rightarrow bal-al-bali$ ‘forget’ \rightarrow ‘to (suddenly) be forgetful’
	$g^wax \rightarrow g^wax^w-ax^w-g^wax^w$ ‘walk’ \rightarrow ‘a lot of walking around’	$g^wad \rightarrow g^wad-ad-g^wad$ ‘talk’ \rightarrow ‘talk (a lot), speak up’

I agree that there are clearly two different semantic classes here, but this does not necessarily correlate with a difference in scope between the two morphemes. It could be instead that there are two Aspect positions for the distributive, but both are lower than the out-of-control morpheme, which (following Davis et al. (To appear) on the related language St’at’imcets) is a modal. The forms with unpredictable semantics would be ‘lexical distributives’ with low attachment of the aspectual and semantics which must be learned. The forms with predictable semantics would result from higher attachment of the aspectual, but would still have OOC-over-DIST ordering and therefore take the same shape on the surface. From a syntactic point of view, this is a tenable conclusion: the distinction between these two types of distributives would

be parallel to the difference between two types of causatives in Tagalog, analyzed in similar terms by Travis (To appear). The causative morpheme in Tagalog is the prefix *pag-*, which combines with roots to create causatives, some of which are unpredictable, as shown below:

	<i>Root</i>	<i>Causative</i>
(137) a.	tumba ‘X fall down’	pag-tumba ‘Y knock X down’
b.	luwas ‘X go into the city’	pag-luwas ‘Y take X into the city’
c.	sabit ‘X be suspended’	pag-sabit ‘Y hang X’
d.	sabog ‘X explode’	pag-sabog ‘Y scatter X’
e.	sali ‘X join’	pag-sali ‘Y include X’
f.	— —	pag-luto ‘Y cook X’
g.	hiwa ‘X cut/slice Y’	pag-hiwa ‘X cut/slice Y’

It is particularly clear in Tagalog that *pag-* can occupy two different positions because the lexical causatives (with *pag-* in the lower position) can actually be causativized again. Only one copy of *pag-* can be pronounced in any given form, and usually this is the higher copy, as evidenced by it appearing to the left of another morpheme, *pa-*. However, when the Causee serves as a topic, the productive *pag-* drops, so there is a contrast between the productive causatives with only *pa-* and the lexical ones with *papag-*. (On iterated positions in the cartography of the TP domain, see also Brody & Szabolcsi (2003) and Butler (To appear).)

Returning to the Lushootseed case, as long as DIST is lower than OOC, we have some latitude in how to cash out the derivation. One option would be to perform distributive reduplication first, then linearize, which (again taking *bali* as our example) would produce *bal-bali*, and then to perform out-of-control reduplication to this, yielding *bal-al-bali*. Alternatively, it is possible to get the correct ordering with only one linearization step. Provided the out-of-control morpheme is added after the distributive one, the (l, a) link added by OOC will be added to the queue later

and therefore will be closer to the top than the (l, b) link added by DIST. A single run-through of the linearization algorithm will give the desired result.²²

This second possible analysis depends on the assumption with which we have been working thus far, namely that new precedence relations are added specifically to the very top of the queue. Idsardi & Shorey suggest that new precedence relations should not always be added with such high priority, but I will argue that they must invariably be added to the top of the queue. The crucial data here comes from cases in which reduplication and affixation both occur. To illustrate this, we'll again use the string /kæt/. Now consider a hypothetical language (I'll call it Katkatsinese) in which plurality is marked by complete reduplication of the stem plus suffixation of -s, yielding /kætkæts/. Crucially, the suffix is not reduplicated. This is trivial to achieve if the suffix is added after the reduplication has already been linearized, but let's assume for the sake of argument that this is not the case. Then, what is to stop (t, s) from being added to the queue higher than (t, k)? This would obviate the reduplication, and yield simply /kæts/. The solution to this, Idsardi & Shorey propose, is to say the following: suffixhood is the property of being "in competition with" the segment that is linked to %, and therefore, to target (in this case) the statement (t, %) for deprioritization. For this reason, (s, %) and (t, s) are added to the queue *right above* (t, %), rather than at the very top.

I argue that this is the wrong approach for a number of reasons. First, one

²²We might, of course, prefer one analysis to the other for independent reasons, depending on what we believe about the cyclicity of derivations. But I will set this issue aside until we discuss the phonological cycle in Chapter 5.

benefit of a theory like the one we have been pursuing is that it makes no distinction between reduplication and affixation, but Idsardi & Shorey's approach re-introduces a difference between these two processes. Second, there are cases in which a suffix *does* undergo reduplication along with the stem. One such case is the formation of distributive numerals in Yessan-Mayo (Foreman 1974):

- (138) pes -ri → pes-ri-pes-ri
 2 GEN 2.DISTR.GEN

An exactly parallel case from Arrernte is discussed by Breen & Pensalfini (1999) and Rainy (2006). In this language, an instrumental noun is formed from a verb by suffixing *-en̩* and (partially) reduplicating the stem + suffix:

- (139) a. at^wer → at^wer-en̩-er-en̩
 'fight' 'weapon'
 b. et̩ → et̩-en̩-et̩-en̩
 'poke' 'instrument for poking'

Insofar as both the suffixation-over-reduplication and reduplication-over-suffixation patterns are attested, one should not be too hasty to stipulate, as Idsardi & Shorey do, a procedure that will rule out the latter pattern. I suggest instead that the difference between the Katkatsinese pattern and the Yessan-Mayo/Arrernte pattern boils down to a difference in rule ordering (where 'rule' is taken to encompass SEARCH + COPY and linearization):

- | | | |
|-------|-----------------------|-----------------------|
| | Katkatsinese | Yessan-Mayo/Arrernte |
| (140) | 1. Form backward loop | 1. Add suffix |
| | 2. Linearize | 2. Linearize |
| | 3. Add suffix | 3. Form backward loop |
| | 4. Linearize | 4. Linearize |

This raises another question, namely whether linearization always applies immediately after the SEARCH and COPY operations which create a loop, or whether some

processes can apply while the representation is non-asymmetrical. This question is of some consequence if we want to represent *all* phonological rules in terms of SEARCH, as I will suggest later in this chapter: if the SEARCH algorithm has to run while there is a loop in the string, we would be justified in worrying whether the search might get ‘stuck’ in the loop and never terminate. Moreover, we might also hope for some principled way of constraining when linearization happens. As I see it, there are three options: (a) linearization always happens immediately after SEARCH and COPY concatenate two morphemes (a principled and unproblematic solution); (b) linearization is ordered along with all other rules (and the problem just mentioned will arise); (c) linearization happens once at the end of each phonological cycle (principled, but still problematic). Determining whether any rules have to apply between SEARCH/COPY and linearization can help us to decide this question.

Actually, the answer was already suggested in §4.3.4. Recall the case of overlapping Malay nasalization, repeated below:

- (141) Malay nasalization with reduplication (Raimy 2000a:16)
- | | | | | |
|----|------|------------|-----------|----------------------|
| a. | hamõ | ‘germ’ | hãmõ-hãmõ | ‘germs’ |
| b. | waŋĩ | ‘fragrant’ | wãŋĩ-wãŋĩ | ‘fragrant (intens.)’ |
| c. | aŋã | ‘reverie’ | ãŋã-ãŋã | ‘ambition’ |
| d. | aŋẽ | ‘wind’ | ãŋẽ-ãŋẽ | ‘unconfirmed news’ |

Raimy (2000a,b) argues that (in the absence of base-reduplicant identity, the possibility of which I will not entertain here) the only way to capture this case is to say that nasalization applies while there is still a loop connecting the last segment to the first, and that the existence of this loop is sufficient to trigger nasalization since it sets up a situation in which a nasal segment precedes the first vowel, albeit non-exhaustively.

A (seemingly) parallel case presented by Wilbur (1973) comes from Chumash. This language has a process which turns a voiceless consonant followed by /h/ into an aspirate, and does the same to geminate consonants. It also has reduplication of the first CVC of the stem and any ‘inner’ prefix (I assume outer prefixes such as *ma-* enter on subsequent cycles to a linearized form), serving to make a noun plural, an adjective intensive, or a verb repetitive (see Inkelas & Zoll (2005)). We see below that this process overapplies in reduplicated forms:

(142) Chumash aspiration with reduplication (Wilbur 1973:26)

- a. s-soyin s^hoy-s^hoyin ‘it is very black’
- b. ma-k-hatinet ma-k^hat-k^hatinet ‘my joints’

We must be very careful, however, not to equate the Malay and Chumash cases. The Chumash facts can be derived very simply by ordering the aspiration rule before reduplication, creating intermediate forms *s^hoyin* and *k^hatinet* going into the reduplicative process. This is true also of multiple other overapplication cases presented by Wilbur (1973, §3.8), including Tagalog and Javanese nasal assimilation and palatalization in Dakota. In fact, *all* the cases Wilbur discusses can be accounted for in this manner. This puts us in a somewhat uncomfortable position given that there are only two other cases of overapplication in the literature (which were not known at the time of Wilbur’s writing), namely the Malay case and one from the Ethiopian Semitic language Chaha which we will see momentarily. But the status of the Malay data is unclear, as Kenstowicz & Banksira (1999) and Raimy (2000b) note: despite being referenced in secondary sources since Kenstowicz (1981), this data “have not been reconfirmed or replicated... [and] may not present a real case of backcopying” (Raimy 2000b:542fn1).

The Chaha data, reported by Kenstowicz & Banksira (1999), is better documented but ultimately unconvincing. In Chaha, the relevant process is dissimilation of /x/ to [k] when a [+CONT, -SON] segment (i.e., /f, s, z, x, ʕ/) follows anywhere in the word.²³ In reduplicated frequentative imperfective and perfective verb forms, this process overapplies:

(143) Chaha dissimilation with reduplication (Kenstowicz & Banksira 1999)

	<i>Root</i>	<i>Imperfect</i>	<i>Perfect</i>	
a.	/sxɾ/	yɪ-t-sɪkəkəɾ	tə-skəkəkəɾ	‘act naughtily’
b.	/mxɾ/	yɪ-tɪ-mkəkəkəɾ	tə-mkəkəkəɾ	‘advise each other’
c.	/rxβ/	yɪ-tɪ-rkəkəkəβ	tə-rkəkəkəβ	‘show up’

In these cases, the triggering environment for the dissimilation is created only by reduplication making an /x . . . x/ sequence; there is no other [+CONT, -SON] segment in the root, so if dissimilation applied prior to reduplication, we would expect nothing to happen, and for all /x/ to surface as [x]. If instead dissimilation applied after reduplication, we would expect only the *first* instance of /x/ to become [k] since there is nothing subsequent to the second /x/ that would trigger its hardening. As a result of the unexpected outcome—both instances of /x/ surface as [k]—Kenstowicz & Banksira (1999) provide an analysis in terms of base-reduplicant identity, and Raimy (2000a) argues parallel to his analysis of Malay that this represents a case in which dissimilation applies to a looped representation when /x/ is followed by a [+CONT, -SON] segment and this environment need not be exhaustive. But there are two important things to note here. First of all, in both perfective and imperfective

²³As Kenstowicz & Banksira (1999) note, the underlying representation could also be an archiphoneme unspecified for [CONTINUANT]. The rule in question would then be feature-filling rather than feature-changing. This is not relevant for the present purposes.

frequentative forms (and non-frequentative perfectives, which also show [k] rather than [x]), the middle radical is actually geminated (=reduplicated). An independent process of degemination then turns /xx/ into [k] (Banksira 1997, Raimy 2000a). Thus, the vast majority of the forms which *prima facie* seem to exhibit overapplication of dissimilation are actually irrelevant; the [k]'s result from something entirely different. The idea that [k] in these forms arises from degemination rather than dissimilation is also supported by the fact that there are some forms, namely the frequentative imperative, which differ minimally from the frequentative perfective shown above in that they display [x] rather than [k]:

(144) Chaha imperative frequentative reduplication (Kenstowicz & Banksira 1999)

	<i>Root</i>	<i>Imperative</i>	
a.	/sxr/	tə-sxaxər	'act naughtily'
b.	/mxr/	tə-mxaxər	'advise each other'
c.	/rxβ/	tə-rxəxəβ	'show up'

In order to account for both the forms in (143) and those in (144), we need dissimilation to apply before reduplication (i.e., a counterfeeding order) but after gemination of the middle radical (i.e., a feeding order) to account for (143). We will develop this account further momentarily.

There are some other forms which also help to suggest a rule ordering analysis. These patterns occur when a biliteral root is required to assume a CVCCVC shape, or when it must conform to a template which requires three consonants. In the first case, both consonants are reduplicated (e.g., 12 → 1212), and in the second case, the second consonant is copied (e.g., 12 → 122). When the second radical is /x/, both copies of it surface as [k]; again, we do not expect the second /x/ to be hardened. This brings us to a second crucial fact about the Chaha case:

“Our perusal of Kenstowicz and Petros Banksira 1999 shows no words — reduplicated or not — in which a [k] is followed in a word by [x], or indeed any words in which the two different allophones co-occur. This distributional generalization easily follows if we assume a surface correspondence relation between velars. . . subject to a typical phonological agreement constraint, entirely independent of reduplication, which requires velar obstruents to agree in [continuant]. Constraints such as this are needed independently to handle everyday, nonreduplicative consonantal co-occurrence restrictions. . .” (Inkelas & Zoll 2005:178)

I have argued in an earlier chapter that constraints should not be part of phonological theory, but nevertheless I feel Inkelas & Zoll make an important point. To paraphrase, if [k] appears, then *all* instances of /x/ in the word surface as [k]. It is purely an accident that there are only ever two instances of /x/ in a word when there is reduplication, because of the familiar Semitic prohibition against /C₁C₁C₂/ roots, and because /C₁C₂C₂/ are by hypothesis underlyingly /C₁C₂/ with the second radical always being reduplicated to create a triliteral.

The observed data can be captured if we first make what I feel is a fairly intuitive distinction between two layers of morphophonological processes in this language: the ones which deal only with the root consonants (e.g., reduplicate 12 → 122, etc.), and those which add other material. This is essentially the same cut made by Raimy (2000a), but performed in a different manner: Raimy has all the loops being created at once, and then dissimilation then applying to the looped representation only in non-derived environments (where non-derived = no new segmental material added), which runs counter to the generalization that rule application is normally *blocked* in non-derived environments; see Chapter 5. But if we simply say that modifications to the root alone precede and are linearized *before* new morphological material is added, then there is the possibility of performing other phonological rules in between these

two layers of morphology.

The dissimilation facts will be correctly generated if we posit that the following steps:

- (145)
- a. Reduplication of root consonants
 - b. Linearization
 - c. Degemination
 - d. Dissimilation
 - e. Affixation
 - f. Linearization
 - g. /x/ → [k] if there is any other [k] in the word

Let's now run through three sample derivations, which will illustrate how this works. As our examples we will take the frequentative perfect, the frequentative imperative, and a biliteral root undergoing total reduplication. I assume that the SEARCH and COPY procedures which create the frequentative imperative and perfective are the same, apart from the initial gemination in the perfective:

- (146) Frequentative perfective/imperative formation
- a. Add (#, t), (t, ə), (ə, C₁)
 - b. Add (C₂, ə), (ə, C₃)
 - c. Add (C₂, a), (a, C₂)

The derivations for the frequentative perfect, frequentative imperative, and biliteral would be as follows, collapsing each instance of reduplication/affixation with the associated linearization for convenience.

	<i>Freq. perf.</i>	<i>Freq. imper.</i>	<i>Bilateral</i>
(147) (Root)	sxr	sxr	xt
Reduplication	sxxr	—	xtxt
Degemination	skr	—	—
Dissimilation	—	—	ktxt
Affixation	təskakər	təsxaxər	kətɣit
/x/ → [k]	—	—	kətkit

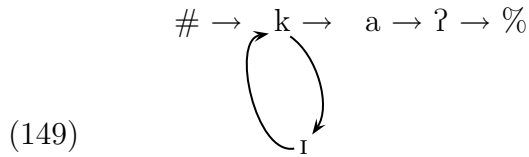
In short, rule ordering can also account for the Chaha case; nothing needs to happen to a looped representation in between reduplication/affixation and linearization. With the exception of the dubious Malay example, there is absolutely no evidence from overapplication that linearization must ever be separated from the application of SEARCH and COPY which created the non-asymmetric form.

There is a second class of phenomenon which has also been taken to require rules to apply to looped structures, however. The most well-known of these cases is from Akan. In this language, there is a process of palatalization which takes the dorsal segments plus /h/ to palatodorsals before [-LOW, +FRONT] segments. But here, we see that in some cases, palatalization applies transparently (as in (148a,b)), and in other cases (as in (148c,d)) it *under*-applies in reduplicated forms as compared to what would be the transparent application, shown in the ‘Expected’ column:

(148) Akan palatalization with reduplication (McCarthy & Prince 1995)

	<i>Actual</i>	<i>Expected</i>	
a.	dʒɪ-dʒe	dʒɪ-dʒe	‘receive’
b.	tɕɪ-tɕeʔ	tɕɪ-tɕeʔ	‘cut’
c.	kɪ-kaʔ	*tɕɪ-kaʔ	‘bite’
d.	hɪ-hawʔ	*ɕɪ-hawʔ	‘trouble’

This is a case of reduplication with fixed segmentism: copy the first consonant and insert /ɪ/ after it. Putting this in terms of adding precedence relations, (148c) is represented as:



Now it is apparent that while /k/ does precede the non-low front vowel /ɪ/, it also precedes the non-palatalizing vowel /a/: the environment is non-exhaustively/non-uniformly triggering for palatalization. But in contrast to Malay, which (purportedly) allows nasalization to apply in such a case, the palatalization rule does *not* apply here. It is just in the case where a consonant is exhaustively in a triggering environment, as in (148a,b), that palatalization applies. On the basis of this contrast between Malay and Akan, Raimy posits the Uniformity Parameter. If the Uniformity Parameter is set to ‘on,’ a rule is only triggered if the target segment is exhaustively in the triggering environment (as in Akan), and if it is set to ‘off,’ then a non-exhaustive environment is sufficient (as in Malay).²⁴

This tidy parametric explanation notwithstanding, it has been known since the 1960’s that the Akan case is perfectly well described with palatalization simply applying prior to reduplication (see discussion in Wilbur (1973)). The only case of underapplication which *cannot* be handled by rule ordering is from the Luiseño language and is described in detail by Wilbur (1973:18ff). This language normally has stress on the initial syllable, but certain suffixes trigger stress retraction. Subsequent

²⁴Idsardi & Shorey (2007) note that this correlates with the difference between universal and existential quantification: the ‘on’ setting can be thought of as a rule which applies if *all* arcs (precedence relations) connecting to a potential target match the structural description, whereas the ‘off’ setting is equivalent to a rule applying when *there exists* an arc which matches the structural description.

to this, there is syncope in the environment $\acute{V}C_CV$, and the second of two vowels in hiatus deletes.²⁵ Finally, there is a rule which turns $/\check{c}/$ into $[\check{s}]$ before a consonant or word-finally.²⁶ This $/\check{c}/ \rightarrow [\check{s}]$ rule is the subject of our concern, because it fails to apply to deverbals. These adjectives are formed by reduplicating the verbal root (here, *čara-* ‘to tear’) plus the adjectival suffix *-i*. I show this in the absolute, which has the ending *-č*; however, as we will see below, my account relies on the absolute ending not actually being present during the whole derivation. Below I give the expected and actual derivations of this adjectival form, as presented by Wilbur.

(150) Derivation of Luiseño deverbals

	<i>Expected</i>	<i>Actual</i>
Reduplication	čaračaraič	čaračaraič
Stress assign.	čáračaraič	čáračaraič
Stress retr.	čaráčaraič	čaráčaraič
Syncope	čaráčraič	čaráčraič
Hiatus res.	čaráčrač	čaráčrač
$/\check{c}/ \rightarrow [\check{s}]$	čaráčraš	*čarášraš

As we see above, the $/\check{c}/ \rightarrow [\check{s}]$ rule applies to the case ending, but unexpectedly *not* to the $/\check{c}/$ in the stem. The important thing to note here is that an analysis which has the $/\check{c}/ \rightarrow [\check{s}]$ rule applying to a looped derivation in Raimyan style is of no use. The environment for the application of this rule is only created by syncope, which must take place once the reduplicated form has already been linearized. Also, it is not as if this rule systematically fails to apply in all reduplicated forms; it applies regularly to

²⁵There are also processes of vowel shortening and raising which are not relevant to the forms described here. Shortening follows syncope, and raising is the final step. I omit these from the derivation shown below.

²⁶Wilbur (1973:21) provides arguments that $/\check{c}/$ is underlying, which I will not recap here. The same arguments would apply if $/\check{s}/$ were chosen as the underlying representation.

reduplicated noun and verb forms. For this reason, I suggest that adjective formation is exceptional in not triggering this particular rule. When case morphology enters on a subsequent cycle (anticipating Chapter 5), this rule now gets the chance to apply (and does so, affecting the case ending), only by this time the root can no longer be changed because of strict cyclicity.

The case for divorcing SEARCH and COPY from immediate linearization, then, rests only on the data from Malay, which “have proven difficult to confirm” (Kenstowicz & Banksira 1999:573; see also Raimy 200b). In the absence of another case which *must* be solved by applying a rule to a looped representation, I feel it is not justified to complicate the theory in order to account for this quite possibly spurious lone example. I suggest, then, that linearization always occurs immediately after SEARCH and COPY do their work. This has the effect of constraining the range of possible phonological phenomena considerably—specifically ruling out cases like the ostensive Malay pattern—and also eliminates a potential obstacle which would stand in the way of using SEARCH more broadly by ensuring that there will never be a situation in which the algorithm would have to traverse a loop.

To summarize the chapter up to this point, I first argued for Mailhot & Reiss’s (2007) view of harmony processes as applications of SEARCH and COPY algorithms driven by the need to fill in underspecified feature values. I then showed that this system is highly compatible with Raimy’s (1999 et seq.) analysis of reduplication, affixation, subtractive morphology, and templatic/non-concatenative morphology as involving the addition of (sometimes highly abstract) affixes; unifying the two theories requires only minor modifications to both. Based on my typological investigation of

the attested anchor points for reduplication and affixation, which yielded a restricted set of primitives used in these processes, I proposed that Mailhot & Reiss's SEARCH and COPY algorithms, with the addition of one extra variable, can be extended to account for all of these phenomena. The application of SEARCH and COPY are driven in these cases by the need to concatenate an affix with another string in the phonological workspace, and are facilitated by 'sticky ends' (variables) in the lexical representations those affixes. Finally, I showed that Idsardi & Shorey's (2007) modified version of Dijkstra's algorithm can be used to linearize the structures produced by SEARCH and COPY, and that loops are destroyed by linearization just as soon as they are created.

4.4 Generalized Search & Copy

In the remainder of this chapter, I will propose extending parameterized SEARCH and COPY to all phonological rules (I call these 'generalized' SEARCH and COPY), and introduce the third primitive operation, DELETE. As early as *SPE*, it was recognized that rule application could be seen as a search plus modification procedure (see also Mailhot & Reiss (2007:30)):

"To apply a rule, the entire string is first scanned for segments that satisfy the environmental constraints of the rule. After all such segments have been identified in the string, the changes required by the rule are applied simultaneously." (Chomsky & Halle 1968:344)

I want to emphasize the fact that describing rule application as a search-plus-modification procedure is completely compatible with the major results of rule-based phonological theory of the generative era; for example, the notion that rules apply serially and are extrinsically ordered. In fact, using SEARCH simplifies rules and preserves

important insights from autosegmental phonology while streamlining representations. The former can be seen clearly in the case of the ‘subscript zero convention,’ in which the symbol C_0 is used as an abbreviation for a string of zero or more consonants.²⁷ The following hypothetical rule, which will add a feature F to the final vowel in a word, makes use of this convention:

$$(151) \quad V \rightarrow [+F] / _ C_0\#$$

This rule is to be viewed as an abbreviation for an infinite set of simultaneously-applying rules as below:²⁸

$$(152) \quad \begin{array}{l} \text{a. } V \rightarrow [+F] / _ \# \\ \text{b. } V \rightarrow [+F] / _ C\# \\ \text{c. } V \rightarrow [+F] / _ CC\# \\ \text{d. } V \rightarrow [+F] / _ CCC\# \\ \text{e. } V \rightarrow [+F] / _ CCCC\# \\ \quad \quad \quad \vdots \end{array}$$

I agree with the argument which has been made more or less explicitly by Odden (1994) and others since the 1970’s that the subscript zero convention should be eliminated, and infinity should be re-interpreted as *locality*. That is to say, when two

²⁷See Reiss (2008b), which goes some distance towards developing a generalized SEARCH and COPY mechanism, for additional discussion of this issue. Reiss also proposes SEARCH-based accounts for several ‘Path-type rules’ (see §4.4.1 of the present work), including the Sundanese nasalization case which we will soon discuss.

²⁸Following Howard (1972:18), “The claim that these phenomena should be handled by infinite schema rather than by an abbreviation... is justified by the fact that the upper limit on the number of consonants is an arbitrary one contingent only upon the maximal number to be found in the strings of that language. If a word with four final consonants should be added to the language we would fully expect it to behave as predicted by [(152e)]. The fundamental fact here is that the number of consonants is entirely irrelevant to the operation of the rule.”

segments appear to interact at a distance, they are actually adjacent to one another on some abstract level. In recent years, this has been achieved representationally, by separating features onto separate planes (i.e., feature geometry combined with autosegmental tiers) and enforcing some type of constraint against crossing association lines; we saw this approach already in our discussion of harmony earlier in this chapter.

Adopting a SEARCH-based view of rule application allows us to maintain this basic result, but without appealing to autosegmental tiers, feature geometry, or constraints. In other words, the approach taken here is procedural rather than representational. More concretely, (151) can be replaced by the following SEARCH and COPY procedure, which is exactly the same as the procedure we used for morphophonology earlier in this chapter; the only difference is that since the feature [+F] docks on a single segment, there only needs to be one application of SEARCH and COPY. I take [+F] to be the initiator (ς) of SEARCH. In other words, I treat [+F] as a highly abstract affix (which other theories might call a ‘floating feature’) in need of being concatenated with the string in the phonological workspace.²⁹ Again, remember that SEARCH is starting from β —it cannot start from ς since ς is not in the string to be scanned—but I still consider ς to be the ‘initiator’ and assign it that variable since it is in some sense still the trigger of the SEARCH.

²⁹One might take exception with my use of ‘copy’ in this loose sense; the operation here is more akin to insertion than copying. But as Hornstein (2001) points out, ‘insertion’ is merely copying from the lexicon. I prefer to use the term ‘copy’ to emphasize parallelism with the other types of processes discussed throughout this chapter.

- (153)
- a. ς (initiator of SEARCH): [+F]
 - b. γ (target of SEARCH): First V
 - c. δ (direction of SEARCH): L
 - d. β (beginning point of SEARCH): %
 - e. COPY ς to γ

No matter how many consonants come between % and the last vowel in the word, SEARCH will converge on the correct target.

Another thing to note about the procedure in (153) is that, rather than COPY adding a feature *from* the target (γ) *to* the initiator (ς) as we have seen in both harmony and morphophonology, in this case COPY applies the other way around. This will require some modification to the COPY algorithm, as shown below:

- (154) COPY algorithm (bidirectional version)

Identify αF on γ_i and assign αF to ς_i if the set of conditions C on γ_i are satisfied or

Identify αF on ς_i and assign αF to γ_i if the set of conditions C on γ_i are satisfied.

Any conditions on COPY are still restricted to the γ variable. Additionally, note that in the morphophonology case, an entire segment is being copied into the precedence statement in which ς acts as a placeholder.³⁰ Consider the variable F in the

³⁰This runs counter to Principle 6 of Chomsky (1967:125):

- (1) PRINCIPLE 6: Two successive lines of a derivation can differ by at most one feature specification.

However, this generalization was clearly not meant to apply to morphological processes—and note that spreading of an autosegmental node subsuming multiple features also violates Principle

COPY algorithm, then, to stand for any prespecified set of features.

I argue that there is a principled distinction to be made here, one which Archangeli & Pulleyblank (1994) made in their theory of parametric rules. These authors proposed that all phonological rules can be described using combinations of four parameters: Function, Type, Direction, and Iteration. The parameters and their possible values are listed below.

- (155) a. Parameter: **Function**
 Values: {INSERT, DELETE}
- b. Parameter: **Type**
 Values: {PATH, F-ELEMENT}
- c. Parameter: **Direction**
 Values: {LEFT-TO-RIGHT, RIGHT-TO-LEFT}
- d. Parameter: **Iteration**
 Values: {ITERATIVE, NON-ITERATIVE}

Direction corresponds directly to the δ parameter on SEARCH. The Function parameter corresponds roughly to my distinction between COPY and DELETE operations, but note from our discussion of subtractive morphology that deletion of a segment/string can also be achieved via jump links; in other words, SEARCH plus COPY can mimic SEARCH plus DELETE. I will suggest shortly that DELETE, as dis-

6. Nor is it obvious to me in light of, for example, epenthesis of segments other than schwa, which presumably have more than a single feature value, how one could maintain this generalization. It is particularly hard to do so when one assumes little underspecification. See Vaux & Samuels (2003) for relevant arguments concerning consonant epenthesis.

tinct from jump-link creation, is a primitive operation which can be ordered with respect to SEARCH and COPY. We will discuss iterativity at various points throughout the rest of this chapter, and particularly in §4.4.4 when we tackle multiple rule application.

The Type parameter is what distinguishes rules spreading features which are already present on one segment to other segments in the string (Type: PATH) from those which introduce new features into the derivation (Type: F-ELEMENT). For example, a rule with the effect of the one in (151) would have the Type parameter set to F-ELEMENT because [F] was not present in the derivation prior to the rule's application. A harmony or assimilation rule, on the other hand, would have Type set to PATH because the rule creates copies of a feature which is already present.³¹ I would like to maintain the distinction between these two kinds of processes, which I will call Path-type and FE-type for convenience, using SEARCH.

4.4.1 Path-type rules

One of the fundamental insights of Mailhot & Reiss (2007) is that, as we discussed in §4.2.5, by turning the 'donor-recipient' conception of harmony on its head, a number of phenomena fall out of the theory for free. Such phenomena include the behavior of 'opaque' and 'transparent' vowels and blocking effects previously at-

³¹Strictly speaking, the difference between PATH and F-ELEMENT does not depend on whether the feature in question is already in the derivation; it is instead whether there exists a token of that feature *upon which the rule depends*. In other words, there could be a F-ELEMENT rule which inserts a feature that is already present on some segment in the string, so long as there is no relationship between the pre-existing and newly-added occurrences of that feature.

tributed to the No Line-Crossing Constraint. These results are achieved by making the *target/recipient* the initiator (in their terms, the ‘standard’ ς) of SEARCH. As Mailhot & Reiss (2007) note, multiple standards may converge on the same goal (γ); in other words, a single segment may donate a feature value to multiple recipients. This is to be preferred over the converse, which would be the case if the donor initiated the search. Multiple donors with conflicting feature values could converge on the same recipient, and there would have to be some mechanism for resolving this conflict.

As an example, let’s look at nasal spread in Sundanese. This language has a process which nasalizes all vowels to the right of a nasal consonant; the spreading is blocked by all oral consonants except /h/ and /ʔ/. Following Padgett (1995) and acoustic analyses by Cohn, these two consonants actually undergo nasalization themselves when in the appropriate environment (this is not shown in the transcription below).

(156) Sundanese nasal spread (Robins 1957, Cohn 1990)

- a. $\eta\tilde{a}\tilde{i}\tilde{a}n$ ‘to wet’
- b. $kum\tilde{a}h\tilde{a}$ ‘how’
- c. $m\tilde{i}\tilde{?}\tilde{a}sih$ ‘to love’
- d. $\eta\tilde{a}tur$ ‘to arrange’
- e. $m\tilde{a}wur$ ‘to spread’

Even though this may at first seem counterintuitive, let us posit a SEARCH with the following parameters to account for this process:

- (157)
- a. ς (initiator of SEARCH): $\forall X$ (i.e., all segments)
 - b. γ (target of SEARCH): $[\alpha NASAL]$
 - c. δ (direction of SEARCH): L
 - d. β (beginning point of SEARCH): ς
 - e. COPY $[\alpha NASAL]$ from γ_n to ς_n

This will make every segment look to its left for the closest segment specified for [NASAL], then copy that segment's value for [NASAL] onto the initiator of the search. We will obtain the correct results as long as COPY is incapable of overwriting feature values. In other words, COPY only succeeds if the standard is not specified for [NASAL]. Otherwise, it fails. Thus, this will be a typical 'feature-filling' rule.³²

Another interesting property of performing nasal spread in this way is that each segment can perform its SEARCH simultaneously, with the result being what has traditionally been termed 'iterative' application. To illustrate this, take the derivation of *kumãhã* 'how'. I assume that the underlying representation is /kumaha/. Then according to the SEARCH and COPY procedure outlined above, every segment will simultaneously initiate its own search, returning an ordered pair of the form (ς_i, γ_i) where γ_i is a segment which is specified for [NASAL]. I list these ordered pairs below (with the goal of the failed SEARCH from the leftmost segment represented as \emptyset). Recall that at this stage we assume that only /k/ and /m/ are specified for [NASAL], so these are the only possible goals for SEARCH.

(158) (k, \emptyset), (u, k), (m, k), (a, m), (h, m), (a, m)

Next, COPY applies. Since /k/ and /m/ are already specified for [NASAL], they cannot be affected. However, {u, a, h, a} are all unspecified for [NASAL] and will

³²Also note that, while Mailhot & Reiss (2007) did not use universal quantification in their original formulation of SEARCH and COPY for harmony phenomena, feature-filling harmony can be expressed with $\forall V$ or $\forall X$ as initiating SEARCH. Since there is no possibility of 'overwriting' feature values in the absence of combining COPY with DELETE, only underspecified segments will be valued by the COPY procedure associated with such a SEARCH.

receive a value for that feature from the goals of their respective searches. The result of this will be /u/ receiving /k/'s [-NASAL] and {a, h, a} receiving /m/'s [+NASAL].

In (157), the target of SEARCH was narrowly specified, leading to termination only on consonants specified for [NASAL]; COPY had no restrictions on it. Now let's look at what happens in the converse situation: when the target of SEARCH is quite broadly specified and there are conditions on COPY. Consider what pattern would be produced by the following:

- (159)
- a. ς (initiator of SEARCH): $\forall C$
 - b. γ (target of SEARCH): X
 - c. δ (direction of SEARCH): R
 - d. β (beginning point of SEARCH): ς
 - e. COPY [α VOICE] from γ_n to ς_n if γ_n is [+CONS].

This would initiate a SEARCH from each consonant to the nearest segment to its right. If that segment is a consonant, the initiator will receive the voicing specification of the target. In other words, this combination of SEARCH and COPY will produce regressive voicing assimilation in clusters. Because every consonant's search terminates at the segment immediately adjacent to it, the assimilation is inherently local or 'non-iterative.' If the segment to the right of the initiator is a vowel, COPY fails so nothing happens; the initiator cannot be affected by a consonant which is non-adjacent to it, unlike in the nasalization case, where there was no limit on the distance over which the SEARCH algorithm could travel, and therefore over which [NASAL] could spread.

Depending on one's view of underlying representations, one might want to make the argument that the assimilation process described above is feature-changing, not feature-filling. That is to say, all consonants should be underlyingly specified for

[VOICE], and regressive assimilation should be able to ‘overwrite’ those values. Even if this is not the correct analysis of this particular case, it is undeniable that feature-changing rules do exist and must be accounted for in any phonological theory. On the other hand, we would also like to capture the noted generalization that the feature-filling case is the more frequent state of affairs (see Kiparsky 1985, Archangeli & Pulleyblank 1994). I argue that this is because feature-changing processes are inherently more complicated, involving the application of the operation DELETE between SEARCH and COPY; DELETE has the ability to remove feature values. For example, if all consonants were underlyingly specified for [VOICE], the procedure in (159) could be modified as follows:

- (160)
- a. ς (initiator of SEARCH): $\forall C$
 - b. γ (target of SEARCH): X
 - c. δ (direction of SEARCH): R
 - d. β (beginning point of SEARCH): ς
 - e. DELETE [α VOICE] from ς_n if γ_n is [+CONS].
 - f. COPY [α VOICE] from γ_n to ς_n .

The conditions placed on COPY in (159) have been instead been stated on DELETE, but the procedure remains the same otherwise. This rule will ensure that two adjacent consonants agree in voicing regardless of whether the leftmost of the pair is specified for [VOICE] or not.

Another use of DELETE is to remove a feature from the target of SEARCH after COPY has applied; this is the ‘spread-and-delink’ paradigm. Take, for example, the case of high tone spread in Tonga. In this language, a lexical H-tone spreads to all vowels to its left, then deletes from the original location. (Another rule then inserts a low tone on the toneless syllables; we will discuss this in the following section.)

(161) Tonga H-spread & deletion (Archangeli & Pulleyblank 1994:292)

	<i>Underlying</i>	<i>Surface</i>	
a.	imakáni	ímákàni	‘news, affairs’
b.	imusimbí	ímúsímbì	‘girl’

The SEARCH, COPY, and DELETE processes which generate H-tone spread and deletion are shown below.

- (162)
- a. ς (initiator of SEARCH): $\forall V$
 - b. γ (target of SEARCH): H (i.e., V with H tone)
 - c. δ (direction of SEARCH): R
 - d. β (beginning point of SEARCH): ς
 - e. COPY H from γ_n to ς_n .
 - f. DELETE H from γ_n .

In short, DELETE can apply either before COPY (to ς) or after COPY (to γ). A feature-changing spread-and-delink rule would therefore contain two applications of DELETE.

Before turning to the topic of FE-type rules, I would like to address one question about DELETE which may be in the reader’s mind: why should there be a primitive operation DELETE when SEARCH and COPY can create a jump link which has the effect during linearization of deleting a segment or string of segments? (Recall the analysis of subtractive morphology shown in (112) of §4.3.4). One way to think of this is as parallel to the difference in syntax between the deletion of uninterpretable features and the deletion of movement copies at the interfaces: one can delete a feature value only, while the other can delete an entire syntactic/phonological feature bundle (i.e., a segment or a word/phrase). Of course, one must be careful not to push this analogy too far—in recent Minimalist syntax it is typically thought that uninterpretable features are unvalued features, and therefore that valuation and deletion go hand in hand; moreover, a newly-valued feature *must* delete in syntax whereas in

phonology I am claiming that DELETE normally does *not* apply—but as a heuristic I feel the parallel has some value.

4.4.2 FE-type rules

We now turn to FE-type rules, the ones which involve the addition of new features into the derivation rather than ‘donation’ of a feature present on the goal to the standard. As I have already argued, FE-type rules should be seen as akin to affixation; the initiator of SEARCH in such a rule is essentially a very small affix—a feature bundle smaller than a segment—looking for a host.³³ As such, this affix is the initiator of SEARCH (starting from within the string in the workspace: $\beta = \{\%, \#\}$), and COPY applies to affix’s feature value(s) to the goal(s) of this SEARCH, subject to any conditions which may be on COPY. The reason why FE-type rules differ from Path-type rules in this respect is simple: the segments which are ultimately affected by the rule cannot initiate SEARCH because there is nothing in the string to search *for*. But in all other respects, FE-type rules are just like Path-type rules. For instance, both can be feature-filling or feature-changing (i.e., they may include DELETE or not), and both can cause changes to a single segment (apply locally/non-iteratively) or multiple ones (apply iteratively).

Admittedly, maintaining a difference between Path-type and FE-type rules may appear to be in violation of Ockham’s Razor and general Minimalist principles, which

³³An interesting result of construing FE-type rules in this way is that it allows us to capture exchange rules (recall §3.2.2) as arising from an FE-type rule with the inserted element being simply a negative operator—which, as we will see in §4.4.3, finds other uses within the SEARCH and COPY framework.

would seem to suggest that all phonological rules should be of a single type. This concern notwithstanding, I feel there are compelling reasons why it is advantageous to make the distinction I have suggested, and why making this distinction in fact leads to a parsimonious theory. First, let us not lose sight of the fact that reducing reduplication, affixation, subtractive and templatic morphology, metathesis, and ‘garden-variety’ phonological rules to three simple operations plus a linearization procedure is already a significant step towards a minimal, and Minimalist, theory of phonology. Second, Path-type and FE-type rules are hardly *de novo* categories; as we have already seen, the distinction is a familiar one from Autosegmental Phonology (see especially Archangeli & Pulleyblank (1994)).

Furthermore, the formulation of FE-type rules requires only two minimal changes to the SEARCH and COPY schema we established for morphophonological processes. We already saw the first change, namely that the direction of COPY is from ς to γ rather than the other way around.³⁴ The new type of COPY is illustrated by the earlier example of placing a feature [F] on the final vowel in a word, which I repeat below:

- (163)
- a. ς (initiator of SEARCH): [+F]
 - b. γ (target of SEARCH): First V
 - c. δ (direction of SEARCH): L
 - d. β (beginning point of SEARCH): %
 - e. COPY ς_n to γ_n

To see the other modification which is necessary, consider again the Tonga data from the previous section. We have already seen how the H-tone spreading and

³⁴Adding the ς -to- γ COPY operation in addition to the γ -to- ς type could be seen as increasing the symmetry of the theory, a potentially welcome consequence.

delinking shown in these forms can be obtained via a Path-type rule. This will generate the following intermediate representations:

- (164) Tonga H-spread & deletion (Archangeli & Pulleyblank 1994:292)
- | | <i>Underlying</i> | <i>Intermediate</i> | <i>Surface</i> | |
|----|-------------------|---------------------|----------------|-----------------|
| a. | imakáni | ímákani | ímákàni | ‘news, affairs’ |
| b. | imusimbí | ímúsímbi | ímúsímbì | ‘girl’ |

Now we would like to see how L tones—(at least some of) which cannot be present underlyingly since they will replace deleted H—fall into place. I will suggest that the low tone is the initiator of SEARCH into the string, and allow one small modification to the goal (γ) parameter of SEARCH: just as we allowed universal quantification in the standard (ς) parameter to yield ‘iterative’ Path-type rules, we will now allow universal quantification in the γ parameter as well. This will allow SEARCH to identify all segments of a particular type (in this case, all vowels) and COPY to place low tone on all of them. (Since DELETE is not involved, this application of COPY will be feature-filling and not disrupt the H-tones.) The parameters for the L-insertion process are shown below, though note that β and δ could take any number of values; my selection of leftward search from $\%_0$ is arbitrary.

- (165)
- a. ς (initiator of SEARCH): L (i.e., L tone)
 - b. γ (target of SEARCH): $\forall V$
 - c. δ (direction of SEARCH): L
 - d. β (beginning point of SEARCH): $\%$
 - e. COPY ς_n to γ_n .

This additional use of the universal quantifier and the reversal of the COPY operation appear sufficient to account for all the differences between Path-type and FE-type rules.

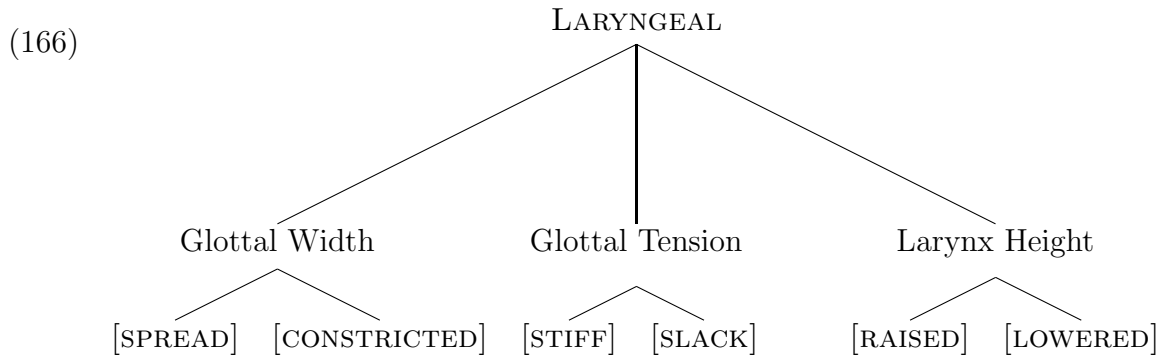
I would now like to discuss some further properties of SEARCH and COPY which are necessary to account for phonological phenomena in general, and to appropriately constrain the possible range thereof. I will discuss two such issues: first, conditions on COPY (§4.4.3), and secondly, multiple rule application (§4.4.4).

4.4.3 Quantification & conditions on copy

Throughout this chapter we have primarily been concerned with SEARCH. However, the conditional nature of COPY—COPY α F *if* condition C is met—also merits attention. In order to approach this issue, I would like to recap arguments made by Reiss (2003b,c) against feature geometry.³⁵ These arguments will lead us to adopt Reiss' alternative, 'feature algebra,' which we will translate into conditions on COPY.

Early theories of generative phonology held all phonological features to be on a par, with no structure internal to the featural system. Since the mid-1980's (e.g., Clements (1985)), though, many phonologists have argued for a hierarchically structured feature system (feature geometry). There are some ways in which groupings of features are obvious, either on articulatory or auditory bases. For example, many phonologists have posited a LARYNGEAL node. Avery & Idsardi (2001) give the following internal structure for it:

³⁵This issue is orthogonal to the question of whether features are innate or emergent, and it bears repeating that Reiss explicitly rejects the emergent feature hypothesis (see, e.g., Hale & Reiss 2008:52ff).



The evidence for feature geometry comes from two different places. First, there are groupings like Glottal Width which are inevitable from an anatomical or acoustical standpoint: the glottis can either be spread or constricted, and that's it; the features associated with these properties must be in complementary distribution purely for physical reasons, so the node Glottal Width does not really do any work in phonology. This is illustrated by a similar situation involving vowel height, or tongue root specifications, or backness: physically, no segment can be both [+HI] and [-HI], or [ATR] and [RTR], or [FRONT] and [BACK] (or [+BACK] and [-BACK]). Encoding these non-co-occurrence restrictions in the feature system is merely a redundant re-statement of biological fact.

More interesting to me is the evidence adduced in support of feature geometry which comes from the typology of sound patterns. For example, Odden (1991) noticed that [BACK] and [ROUND] often seem to pattern/spread together, and on this basis he proposed the node Color, subsuming these two features. This essentially creates a featurally natural class by brute force. But as we have discussed in Chapter 3, one of the major lessons to take away from Mielke (2008) is that positing more and more features/nodes in order to make more phonologically active classes expressible in featurally natural terms is barking up the wrong tree. Trying to do this will only

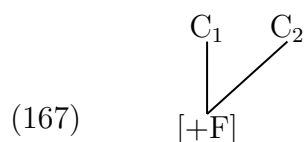
result in a dramatic increase in *ad hoc* features/nodes. What we really need is a way to refer to any arbitrary group of segments.

Reiss (2003b) makes this point in a different way. He focuses on the typology of rules which delete a vowel between certain consonants. There are some vowel deletion rules, such as the one in Biblical Hebrew which takes /ka:tab-u:/ to [ka:θvu:], which apply only when the flanking consonants are (underlyingly) non-identical: syncope does not occur in, for instance, /sa:bab-u:/, which surfaces as [sa:vavu:] after spirantization. ‘Anti-gemination’ processes like this one have been explained in terms of blocking by a universal constraint called the Obligatory Contour Principle (OCP; Leben 1973).

But there is an alternative, which was raised and then rejected by Yip (1988): write the rule $A \rightarrow \emptyset / B_C$ as per usual, and then add the condition that $B \neq C$. Odden (1988) and Reiss (2003b,c) counter that Yip was too quick to dismiss this option. As Odden points out, antigemination is but one piece in the typological puzzle. Not only are there syncope rules which exhibit antigemination effects and those which apply blindly regardless of whether they create geminates, there is also a third type which applies *only* in the case where the flanking consonants are identical, producing what Odden dubs ‘antiantigemination.’ The same, Odden shows, is true of vowel insertion rules: there are some which apply only when the flanking consonants are identical, some which apply blindly, and some which apply only if the flanking consonants are non-identical. (We will see concrete examples of such processes shortly.)

The condition in which the two consonants must be identical can be described as

a condition on rule application or as a constraint referring to structures like the one below, in which two segments are linked by sharing the same feature value:



While feature-value identity can be conveniently expressed in autosegmental notation as above, Reiss (2003b,c) makes the point that because the autosegmental/feature geometric approach does not use variables, it cannot account for rules which require that two segments differ by any arbitrary feature, or any from among a particular subset of features. In order to account for such rules, Reiss proposes a system of ‘feature algebra’ which incorporates variables and quantifiers. The basis of this theory is that what we call a segment (here, C_1 or C_2) is an abbreviation for a feature matrix, which we represent in the following manner:

(168) Segments as feature matrices (Reiss 2003c:222)

$$C_1 = \begin{bmatrix} (\alpha F_1)_1 \\ (\beta F_2)_1 \\ (\gamma F_3)_1 \\ \vdots \end{bmatrix} \quad C_2 = \begin{bmatrix} (\delta F_1)_2 \\ (\epsilon F_2)_2 \\ (\zeta F_3)_2 \\ \vdots \end{bmatrix}$$

F_i denotes a feature, such as [NASAL] and Greek letter variables denote the value (\pm) that feature F_i has for a given segment. The subscript outside of a pair of parentheses containing αF_i denotes the segment in question; thus, these subscripts are always 1 for C_1 and 2 for C_2 .

With these representations, it is still possible to represent the equivalent of (167), where segments C_1 and C_2 have the same value for feature F_n :

$$(169) \quad [(\alpha F_n)_1] = [(\beta F_n)_2]$$

The case in which two segments have different values for F_n can be represented in exactly the same fashion, substituting \neq for $=$. This takes care of the case in which two segments must differ in their values for a *particular* feature, but it can also be extended to account for the non-identity condition: the case in which two segments must differ in terms of *some* feature value, but it does not matter which. This is expressed using the existential quantifier, as below, where \mathbb{F} is the set of features.

(170) NON-IDENTITY CONDITION (preliminary version)

$$\exists F_i \in \mathbb{F} \text{ such that } [(\alpha F_i)_1] \neq [(\beta F_i)_2]$$

We can in fact think of the above statement as a special case of a more general one in which F_i belongs to some set of features $\mathbb{G} \subseteq \mathbb{F}$. Going back to the identity case, the same logic is applicable. The existential quantifier is not relevant here—it is never the case that a rule applies only when two segments share a value for any arbitrary feature—but if we use the universal quantifier instead, it becomes possible to require that two segments agree in their values for an arbitrary set of features. The requirement of total identity (segments must share *all* feature values) is the special case in which $\mathbb{G} = \mathbb{F}$.

(171) IDENTITY CONDITION

$$\forall F_i \in \mathbb{G} \text{ such that } [(\alpha F_i)_1] = [(\beta F_i)_2]$$

In fact, Odden (To appear:21-22fn16) points out that we are better off thinking of *all* the possible conditions as variants on (171):

“Reiss proposes that both universal and existential quantifiers are required, to formulate the Non-Identity Condition— $\exists F_i \in G$ s.t. $[(\alpha F_i)_1] \neq [(\beta F_i)_2]$ — and the Identity Condition— $\forall F_i \in G$ s.t. $[(\alpha F_i)_1] = [(\beta F_i)_2]$. This formalism predicts two unattested conditions, Variable Partial Identity— $\exists F_i \in G$ s.t. $[(\alpha F_i)_1] = [(\beta F_i)_2]$ where at least one feature must be the same— and Complete Nonidentity— $\forall F_i \in G$ s.t. $[(\alpha F_i)_1] \neq [(\beta F_i)_2]$ where all features must be non-identical. Reiss proposes a functional explanation for the nonexistence of the latter two classes. It is worth pointing out that this can also be formally explained. Exploiting DeMorgan’s Laws, the Identity Condition [sic; should be *Non-Identity* condition—BDS] can be equivalently expressed as $\neg \forall F_i \in G$ s.t. $[(\alpha F_i)_1] = [(\beta F_i)_2]$. Given that, Identity and Non-Identity are a single proposition $\forall F_i \in G$ s.t. $[(\alpha F_i)_1] = [(\beta F_i)_2]$ or its negation. If the formal theory only employs the notion of feature identity, not non-identity, and only employs universal quantifiers, not existential quantifiers, then all and only the attested classes of identity conditions can be formalized.”

I will take Odden’s suggested formulations of the Identity and Non-Identity Conditions as the starting point for my translation of the feature algebra approach into conditions on COPY.³⁶ The Identity and Non-Identity Conditions with universal quantification are repeated below:

(172) IDENTITY CONDITION

$$\forall F_i \in G \text{ such that } [(\alpha F_i)_1] = [(\beta F_i)_2]$$

(173) NON-IDENTITY CONDITION

$$\neg \forall F_i \in G \text{ such that } [(\alpha F_i)_1] = [(\beta F_i)_2]$$

³⁶This reformulation of the Non-Identity Condition takes the force out of Baković’s (2005) criticism of Reiss’ proposal to the effect that the latter does not rule out the unattested Variable Partial Identity and Complete Non-Identity Conditions.

With this foundation in place, we can now discuss the importance of these two conditions for a SEARCH-and-COPY model of phonology. I will illustrate this with two examples: syncope in Afar, which is subject to the Non-Identity Condition, and syncope in Yapese, which is subject to the Identity Condition.

First, let us look at the Afar case. The data, originally from Bliese (1981), have been treated by McCarthy (1986), Yip (1988), Reiss (2003b,c), and Baković (2005). The alternations for which we are trying to account are the following:

(174) Afar syncope

	<i>Underlying</i>	<i>Surface</i>	
a.	digib-e	digbé	‘he married’
b.	xamil-i	xamlí	‘swamp grass’
c.	danan-e	danané	‘he hurt’
d.	xarar-e	xararé	‘he burned’

Descriptively, the second vowel in a word deletes, providing the flanking consonants are not completely identical (the vowel must also be unstressed). In the framework developed here, the syncope rule will be expressed as a SEARCH and COPY operation which creates a jump link:

- (175) a. ς (initiator of SEARCH):
 $\varsigma_i \rightarrow \varsigma_j$
 b. γ (target of SEARCH):
 γ_i : Second C
 γ_j : First C
 c. δ (direction of SEARCH): R
 d. β (beginning point of SEARCH):
 β_i : #
 β_j : γ_i
 e. COPY γ_i to ς_i and γ_j to ς_j

This will delete the peninitial vowel regardless of the identity of the flanking consonants, which is not what we want; we need to apply the Non-Identity Condition

to COPY. In order to do this, we can capitalize on the fact that the consonants which must be non-identical are γ_i and γ_j by adding the following condition on COPY:

$$(176) \quad \text{COPY } \gamma_i \text{ to } \varsigma_i \text{ and } \gamma_j \text{ to } \varsigma_j \text{ if } \neg \forall F_i \in \mathbb{F} \text{ such that } [(\alpha F_i)_{\gamma_i}] = [(\beta F_i)_{\gamma_j}]$$

This will give the correct result for the forms in (174). However, more careful inspection of the Afar data reveals that the situation is a bit more complicated. Syncope does not happen when the second syllable is closed:

$$(177) \quad \begin{array}{llll} \text{a.} & \text{digibté} & *\text{digbté} & \text{'she married'} \quad (\text{cf. digbé 'I married'}) \\ \text{b.} & \text{wagerné} & *\text{wagrné} & \text{'we reconciled'} \quad (\text{cf. wagré 'he reconciled'}) \end{array}$$

One way to express this would be to initiate a third iteration of SEARCH (call it a 'subsidiary SEARCH,' as we did in the case of reduplication) beginning at γ_j and searching to the right for the first segment it encounters. We could then incorporate the target of this search, γ_k , into the conditions on COPY:

$$(178) \quad \text{COPY } \gamma_i \text{ to } \varsigma_i \text{ and } \gamma_j \text{ to } \varsigma_j \text{ if } \neg \forall F_i \in \mathbb{F} \text{ such that } [(\alpha F_i)_{\gamma_i}] = [(\beta F_i)_{\gamma_j}] \\ \text{and } \gamma_k \text{ is [-CONS]}$$

A similar approach can account for syncope in Yapese (Jensen 1977, Odden 1988, Reiss 2003b,c), which only applies if the flanking consonants are homorganic and the first consonant is word-initial;³⁷ this is a Partial Identity Condition case.

	<i>Underlying</i>	<i>Surface</i>	
(179)	a. ba puw	bpuw	'it's a bamboo'
	b. ni te:l	nte:l	'take it'
	c. rada:n	rda:n	'its width'

³⁷Or postvocalic. Let us abstract away from this because it will bring up the issue of how rules apply to multiple targets within a word when one application may potentially destroy the environment (or create a new environment) or another, a complication we have not yet discussed. See §4.4.4 below.

We can describe this with the following SEARCH and COPY procedure:

- (180) a. ς (initiator of SEARCH):
 $\varsigma_i \rightarrow \varsigma_j$
- b. γ (target of SEARCH):
 γ_i : First C
 γ_j : First C
- c. δ (direction of SEARCH): R
- d. β (beginning point of SEARCH):
 β_i : #
 β_j : γ_i
- e. COPY γ_i to ς_i and γ_j to ς_j if $\forall F_i \in \{\text{CORONAL, DORSAL, LABIAL}\}$ such that $[(\alpha F_i)_{\gamma_i}] = [(\beta F_i)_{\gamma_j}]$

Thus, the feature algebra approach can easily be applied to a SEARCH and COPY implementation of rule application. It so happens that the rules which are discussed in conjunction with the OCP, and therefore with the Identity and Non-Identity Conditions, are FE-type processes (namely epenthesis and syncope). However, as we saw with vowel harmony, conditions on COPY are also applicable to Path-type processes, and it is quite possible that the feature-algebraic conditions discussed in this section may also be useful for describing Path-type processes as well. I leave this matter to future research. However, I am not aware of any Path-type processes which need a subsidiary SEARCH to state their conditions on COPY; we will make use of this generalization in the next section.

4.4.4 Multiple rule application

The final issue which I would like to discuss is how rules apply to multiple targets within a single word. One answer to this question has already been suggested in our discussion of Path-type rules: the COPY operation applies simultaneously to all

targets which SEARCH has identified. This simultaneous-application approach, as opposed to the directional-iterative application required by autosegmental spreading, goes back to *SPE*. I repeat here the relevant passage:

“To apply a rule, the entire string is first scanned for segments that satisfy the environmental constraints of the rule. After all such segments have been identified in the string, the changes required by the rule are applied simultaneously.” (Chomsky & Halle 1968:344)

The ramifications of this view have been discussed at length in the phonological literature of the past several decades. The most direct consequence of the view expressed in the above passage (call it the simultaneous-application approach) is that

“[G]iven an input string to a rule of the form $A \rightarrow B / C_D$, all *A*s in the context *C_D* will be converted to *B* regardless of whether they remain in that context after the rule has been applied elsewhere in the string. It is the original input string that determines the application of the rule to any point in the string. Application of the rule at any one point in the string cannot prevent (bleed) the application of the rule at any other point in the string. In addition... application at any one point in the string may never create the conditions that make possible the application of the rule at another point in the string. A feeding interaction between applications is prohibited.” (Kenstowicz & Kisseberth 1977:178)

However, when one adds the complication of cyclicity (a matter to which we will return in the next chapter), the possibility of sequential application is re-introduced. Howard (1972:20) summarizes the theory:

(181) *SPE* position on multiple rule application

- a. Application of a rule to two different segments in a string may be sequential provided that (1) the rule is cyclic and (2) the two segments fall within the domain of different cycles.
- b. Application of a rule is simultaneous under all other circumstances.

- c. Schemata abbreviating infinite sets of rules are applied simultaneously.
- d. Except under the conditions provided in (a), rules may not apply to their own outputs.

We have already dismissed possibility (c) above: infinite schemata are now handled by SEARCH. However, it remains to be seen whether (b) and (d) hold—in other words, if rules can ever feed or bleed themselves on the same cycle. The simultaneous-application approach has been challenged on the basis that, indeed, such cases are attested. It is not clear to me, though, that the role of cyclicity has been fully appreciated in the putative counterexamples. For instance, Odden (2005) presents a rule of H-tone lowering in Karanga Shona which he claims provides evidence for left-to-right iterative application of the following rule:

$$(182) \quad \begin{array}{l} \text{H} \quad \rightarrow \text{L} / \text{H-H} \\ \text{[+prefix]} \end{array}$$

In other words, in a sequence of three consecutive high tones, (at least) the first two of which belong to prefixes, the middle one lowers. The relevant data are shown below; in all cases, the last morpheme is the root.

(183) Karanga Shona H-tone lowering

	<i>Underlying</i>	<i>Surface</i>	
a.	á-ká-tórá	á-ka-tórá	‘he took’
b.	á-ká-mú-tórá	á-ka-mú-tórá	‘he took it’
c.	nda-ká-mú-tórá	nda-ká-mu-tórá	‘I took it’
d.	ha-á-zá-ká-mú-tórá	ha-á-za-ká-mu-tóra	‘he didn’t take it’

Odden is correct that forms like *haázakámutóra* present a problem for a simultaneous-application analysis, *if* the word is treated as a single cycle. If that were the case, all but the first and last H tones would be deleted. However, under the assumption that

(at least some of) the prefixes enter on separate cycles—an assumption which I feel is justified for tense and agreement morphemes like these, anticipating Chapter 5—the problem disappears. We can formulate the rule as follows:

- (184) a. ς (initiator of SEARCH): \forall H (i.e., V with H tone)
 b. γ (target of SEARCH): First V
 c. δ (direction of SEARCH): R
 d. β (beginning point of SEARCH): ς
 e. DELETE H from γ_n if γ_n is H

As in the Afar case, we need to add a second condition—here on DELETE—which is based on a second iteration of SEARCH beginning at γ_n and searching to the right for the first vowel it encounters. The second condition on DELETE, then, is that the result of this subsidiary SEARCH must also bear H-tone.

Most of the other cases of which I am aware can be analyzed in a similar fashion, since they all involve strings of morphemes; that is, they all fall under (181a). However, there are some genuine examples of alternating patterns in a monomorphemic context. One is vowel reduction in Macushi (Hawkins 1950, Kenstowicz & Kisseberth 1977, Odden 2005), which applies to certain unstressed vowels. This process must iterate from the left, and bleeds itself; a vowel does not reduce if it is preceded by a reduced vowel.

- (185) Macushi vowel reduction

	<i>Before reduction</i>	<i>After reduction</i>	
a.	wanamari	wənaməri	‘mirror’
b.	u-wanamari-ri	əwanəmarəri	‘my mirror’
c.	u-manari-ri	əmanəri	‘my cassava grater’

One very interesting property of truly iterative processes such as this one (as diagnosed by the characteristic alternations they produce by self-bleeding) is that they are of a particular type:

“Propagating rules produce a pattern that is alternating or nonalternating. Segmental and tone rules are nonalternating: they apply to produce maximal effects, in feeding (class I) or nonbleeding (class II) order. Stress, glide and vowel deletion rules are alternating: they apply in bleeding order (class III).” (Jensen & Stong-Jensen 1976)

We can, I argue, view this distinction between Classes I & II and Class III rules as equivalent to the Path- vs. FE-type distinction, lending further credence to the idea that the mechanisms which produce these two types of processes are (partially) distinct. First consider the Class I/Class II rules. These are processes like harmony/assimilation and tone spread—in other words, Path-type rules. On the view presented here, it is not really accurate to say that these rules are self-feeding; this is the primary difference between the SEARCH-based and autosegmental accounts. We view the iterative (as opposed to non-iterative or strictly local) nature of such rules as stemming from the fact that multiple standards can converge on the same target; there is no need for sequentiality to achieve the desired results. So the relevant fact here is really that Path-type rules *cannot bleed themselves*. This can be seen as a direct result of simultaneous application, a conclusion also reached by Howard (1972). On the other hand, the Class III rules such as glide formation and vowel deletion are in our system FE-type rules. (So, too, may rules of stress placement/pitch accent/boundary tone be, as suggested in the above quote.) We would also predict, if Class III can be equated with FE-type rules, that the ones which produce vowel reduction (by adding an affix $\varsigma_i \rightarrow \emptyset \rightarrow \varsigma_j$ so as to ‘bypass’ the lexical vowel; see Raimy (2000a:31ff)) and epenthesis would also exhibit self-bleeding. This prediction appears to be borne out by cases like the Macushi vowel reduction rule described above.

Two important questions immediately arise if what I have suggested is correct. First, why can only FE-type rules, but not Path-type rules, bleed themselves? Second, if FE-type rules can bleed themselves, why can't they feed themselves? I suggest the answer to the first question is that COPY, and any subsidiary SEARCH associated with it, applies sequentially rather than simultaneously as we have tacitly assumed up to now. The reason why this has not become important until now is primarily that it is impossible to tell with Path-type rules whether COPY (and DELETE, for that matter) applies to multiple targets simultaneously or not. To illustrate this, let's look again at Sundanese nasal spread.

- (186) Sundanese nasal spread (Robins 1957, Cohn 1990)
- a. $\eta\tilde{a}\tilde{i}\tilde{a}n$ 'to wet'
 - b. $kum\tilde{a}h\tilde{a}$ 'how'
 - c. $m\tilde{i}\tilde{r}\tilde{a}s\tilde{i}h$ 'to love'
 - d. $\eta\tilde{a}t\tilde{u}r$ 'to arrange'
 - e. $m\tilde{a}w\tilde{u}r$ 'to spread'

We posited the following SEARCH and COPY procedure:

- (187)
- a. ς (initiator of SEARCH): $\forall X$
 - b. γ (target of SEARCH): $[\alpha \text{ NASAL}]$
 - c. δ (direction of SEARCH): L
 - d. β (beginning point of SEARCH): ς
 - e. COPY $[\alpha \text{ NASAL}]$ from γ_n to ς_n

Take the derivation of *kumãhã* 'how.' SEARCH will return the following pairs of standards and goals:

- (188) $(k, \emptyset), (u, k), (m, k), (a, m), (h, m), (a, m)$

Since $\{u, a, h, a\}$ are all unspecified for $[\text{NASAL}]$, they will receive values from the goals of their respective searches. But since this valuation/COPY process depends

only on the [NASAL] value of γ_n , which does not change at any point in the COPY process as a whole (even in a spread-and-delink process, DELETE necessarily happens to γ *after* COPY has applied to all ς), it makes no difference at all whether the copying occurs all at once or in any order.

The situation is different for FE-type rules because of the possibility of a condition on COPY requiring a subsidiary SEARCH. In such cases, the success of the COPY process for a segment γ_n depends not just on the identity of ς_n and γ_n of the main SEARCH, but also on the γ_{n+1} of the subsidiary SEARCH. If the main SEARCH identifies multiple (potential) targets—i.e., if γ_n involves the universal quantifier—then the target of the subsidiary SEARCH may itself be the target of the *main* SEARCH. This is where the potential for self-bleeding emerges, if COPY and subsidiary SEARCH are sequential.

When there is universal quantification in the SEARCH procedure, meaning the rule can target more than one location in the string, SEARCH first identifies all targets (this, as we have assumed all along, is also iterative) before COPY applies at all. Assume that COPY then applies to each pair returned by the main SEARCH one by one (in the order in which they are identified, $\gamma_0 \dots \gamma_n$); subsidiary SEARCH is done in the same manner. Let's take as an example a hypothetical rule of H-tone insertion which applies to all vowels which are not preceded by another H-tone. This process will be characterized by a SEARCH which scans the string from left to right, identifying all the vowels in the word. So for the string shown below in (189), the standard/goal pairs in (190) will be generated:

(189) # \rightarrow C \rightarrow V₁ \rightarrow C \rightarrow V₂ \rightarrow C \rightarrow V₃ \rightarrow %

(190) $(\varsigma_i, V_1), (\varsigma_i, V_2), (\varsigma_i, V_3)$

Next there is the COPY operation which copies ς_i (i.e., H tone) to each of the vowels. This is subject to a condition enforced by a second, subsidiary application of SEARCH which happens prior to each separate application of COPY. This searches from each γ identified in the main SEARCH and looks to the left for the nearest vowel; if this vowel bears H tone, then COPY does not apply. Step by step, the process looks like this:

- (191) a. Main SEARCH: identify $(\varsigma_i, V_1), (\varsigma_i, V_2), (\varsigma_i, V_3)$
 b. COPY for γ_i :
 Subsidiary SEARCH finds no vowel to the left.
 COPY applies, yielding $CV_1CV_2CV_3$.
 c. COPY for γ_j :
 Subsidiary SEARCH finds V_1 .
 COPY fails to apply.
 d. COPY for γ_k :
 Subsidiary SEARCH finds V_2 .
 COPY applies, yielding $CV_1CV_2CV_3$.

In short, it is because of subsidiary SEARCH, which brings along the potential to condition COPY/DELETE on segments which may be affected by earlier instances of COPY/DELETE, that FE-type rules can bleed themselves. Whether COPY applies simultaneously or sequentially in Path-type processes—or indeed, in FE-type processes which do not involve both universal quantification and subsidiary SEARCH—is impossible to determine, because the results will be the same either way. For the sake of simplicity and parallelism, though, it is perhaps best to assume that COPY is always iterative.

As for the second question, why FE-type rules can feed but not feed themselves, the answer has two parts. First, note that any interaction between multiple appli-

cations of the same rule is only possible when there is universal quantification in SEARCH; otherwise, SEARCH only generates a single (ζ, γ) pair. The other thing to consider is what would have to happen for an FE-type rule *with* universal quantification to feed itself. In order for this to happen, we would need one application of COPY to create a new application environment which will be found by a subsequent subsidiary SEARCH. In other words, we would need COPY to insert a particular feature value (call it [+F]) and a condition on COPY which depends on that same feature value's presence. But now we have gotten ourselves into a bind: such a rule could not apply in the first instance unless there was *already* an instance of [+F] in the derivation to satisfy the condition on (the first application of) COPY. But note that as soon as we have new tokens of [+F] depending on a token of [+F] which pre-exists SEARCH and COPY, by definition we are dealing with a Path-type or spreading rule, not an FE-type or feature insertion rule. Thus, the observation that Class III (FE-type) rules do not feed themselves, as noted by Jensen & Stong-Jensen (1976), is explained. Again, as I have already noted, on our view it is not really the case (as Jensen & Stong-Jensen argued) that Path-type rules feed themselves, either; the appearance of iterative spreading is created by the fact that multiple standards (the ultimate targets of the spreading) can simultaneously converge on a single goal (the donor of the spread feature).

This concludes our discussion of how to implement (morpho-)phonological rules using SEARCH, COPY, and DELETE. Now that we have some understanding of how these three primitive operations work together to produce the range of attested phonological and morphophonological processes, the next logical step is to investigate how

these operations interact with phonological representations on a larger scale. In other words, we turn to looking at the domains within which these operations can apply: specifically, in the next chapter we address how these domains are constructed and how they constrain the operations developed here.

Chapter 5

Phonology at the Interface

5.1 Introduction

This chapter takes us from the representations and operations developed in the previous chapters to the syntax-phonology interface. Because any theory of the interface is necessarily dependent on the current state of affairs in syntactic theory, which is constantly in flux, this chapter is necessarily somewhat speculative. I merely attempt here to offer proof of concept for a particular type of approach, with the caveat that the details may well have to change as our understanding of syntax and the architecture of grammar progress.¹ For this reason, I focus primarily on the logic of the arguments for the theory I present here, which I call ‘phonological derivation by phase’ (PDbP). This theory makes it possible to combine the best parts of Lexical Phonology (Kiparsky 1982), Distributed Morphology (Halle & Marantz 1993), and Derivation by Phase (Chomsky 2001), and re-opens the possibility of a direct

¹Specifically, my theory depends on the inventory of phase heads, and on the particular structures and movements involved in the constructions which I analyze—nominal compounds, for instance. At present, as I try to demonstrate throughout the chapter, research indicates that the structures which I require are not *ad hoc*, but supported on independent syntactic and semantic grounds.

reference conception of the syntax-phonology interface (Kaisse 1985, Odden 1990, Cinque 1993), obviating the need for prosodic boundary construction in the phonology. The basis for this theory is the notion that phonology is cyclic and therefore inescapably derivational. I further argue, following Marvin (2002), that this is the *direct consequence* of cyclic transfer (i.e., phasality) in syntax.

With this model in place, I illustrate how the phonological operations described in the previous chapter are triggered by applications of Merge in the narrow syntax and can be viewed as interface-driven repair strategies necessitated by ill-formed objects being transferred to phonology. I present arguments that the domains of phonological rule application, both above and below the word level, come for free when we assume Distributed Morphology and a phasal syntax. Specifically, phonological processes and operations such as linearization of looped structures get the chance to apply at each application of Spell-Out, and may apply across two adjacent Spell-Out domains but no more. This follows from the Phase Impenetrability Condition (Chomsky 2001). I claim that morpheme-level phases can replace Lexical Phonology's hierarchy of strata, and that clause-level phases can replace the prosodic hierarchy. These arguments are supported with analyses of segmental and suprasegmental (e.g., tone and phrasal stress assignment) processes from languages such as English, Kinande, Korean, and Basque.

This chapter also includes discussion of the status of some phonologically null syntactic objects (e.g., phonetically null vocabulary items and unpronounced copies) with respect to the domains of phonological rule application in an attempt to reconcile the findings of Truckenbrodt (1999), Seidl (2001), and An (2007a,b). I also discuss the

distribution of pronounced items that seem to be sensitive to prosodic factors, such as the ‘second-position clitics’ found in the South Slavic languages, among others.

5.2 Phonological derivation by phase

Throughout the generative era, several cyclic models of phonology have been proposed. The first of these was Chomsky et al. (1956), which introduced the phonological cycle, a crucial component of Chomsky & Halle (1968), and adopted in syntax by Chomsky (1965).² In phonology, this concept was later implemented as the ‘strict cycle’ of Kean (1974) and Mascaró (1976). The tradition of Lexical Phonology (& Morphology) begun by Kiparsky (1982) and Mohanan (1982) developed the idea of cyclicity further, building on Pesetsky (1979). Lexical Phonology classifies morphemes into a number of ordered strata or levels, each of which constitutes a domain associated with a set of phonological rules, plus a final set of ‘post-lexical,’ ‘non-cyclic’ rules.³ (We will discuss the differences between lexical/cyclic and post-lexical/non-cyclic rules in §5.2.3.) Since the late 1980’s, Lexical Phonology has come under heavy criticism, with Gussmann (1988) even proclaiming the “death knell” of Lexical Phonology with the advent of the first published book on the subject, Mohanan (1986). We will evaluate a number of these criticisms in the remainder of this chapter. However, we should note at this juncture that Gussman’s report of Lexical Phonology’s death proved premature, as research in this vein has continued,

²See Freidin (1999), Lasnik (2006) for a history of the cycle’s various incarnations in syntax.

³I will make reference to numerous Lexical Phonology concepts in this chapter; for a proper introduction, see the works cited above, the papers in Kaisse & Hargus (1993), or McMahon (2000).

with numerous studies making headway towards defining the lexical strata in various languages, and more theoretical debates over issues such as the typology of cyclic vs. non-cyclic and lexical vs. post-lexical rules and the correct characterization of principles such as the Strict Cycle Condition. Lexical Phonology has also yielded two Optimality Theoretic frameworks, the LPM-OT of Kiparsky (2000) and the Stratal OT of Bermúdez-Otero (To appear).

Recently, a new movement in phonological theory has emerged, attempting to combine the insights of Lexical Phonology with Distributed Morphology (Halle & Marantz 1993) and the concept of ‘derivation by phase’ in syntax, developed by Chomsky (2001, 2008). The theory presented here, phonological derivation by phase (PDbP), falls under this umbrella, as it takes as a starting point the conceptual argument laid out in the foundational work by Marvin (2002:74): “If we think of levels in the lexicon as levels of syntactic attachment of affixes, we can actually say that Lexical Phonology suggests that phonological rules are limited by syntactic domains, possibly phases.”

From a Minimalist standpoint, a model of grammar with synchronous cycles across the various modules is highly desirable.⁴ Indeed, it is this is the “best-case scenario” according to Chomsky (2004:107):

⁴There is a large—and growing—body of literature which argues that phases are required (follow from virtual conceptual necessity) to regulate syntax’s interfaces with the semantic and phonological components. It is beyond the scope of this dissertation to discuss the implications of derivation by phase for syntax and interpretation, but see, for instance, Boeckx (2008) on how phases facilitate ‘wild-type’ or ‘free’ Merge and a conjunctivist semantics of the type proposed by Pietroski (2005 et seq.).

“Assume that all three components [narrow syntax (NS), semantics (Σ), & phonology (Φ)] are cyclic, a very natural optimality requirement and fairly conventional. In the worst case, the three cycles are independent; the best case is that there is a single cycle only. Assume that to be true. Then Φ and Σ apply to units constructed by NS, and the three components of the derivation of <PHON, SEM> proceed cyclically in parallel. [...] When a phase is transferred to Φ , it is converted to PHON. Φ proceeds in parallel with the NS derivation. Φ is greatly simplified if it can ‘forget about’ what has been transferred to it at earlier phases; otherwise, the advantages of cyclic computation are lost.”

Not only may this solution be computationally efficient, it also allows us to recognize the important contributions of cyclic models of phonology such as Chomsky et al. (1956), Kean (1974), Mascaró (1976), Kiparsky (1982), and Mohanan (1982), *inter alia*. For instance, all attempts to account for phonological opacity effects (i.e., counterbleeding and counterfeeding interactions of the attested types) in a monos-tratal theory suffer from serious empirical or technical problems (see Vaux 2008 and references therein for discussion). Furthermore, the model proposed here relies on a cycle that is not proprietary to phonology. This insulates the approach from one family of recurring criticisms of Lexical Phonology, that its levels were poorly motivated and allowed to proliferate in an unconstrained manner (see, e.g., Itô & Mester 2003). In PDbP, by contrast, evidence for the cycle should come from syntax and semantics in addition to (morpho)phonology. There can be no *ad hoc* stipulation of cycles/levels if a phonological analysis must be responsible to such external evidence; conversely, phonological phenomena should be able to provide evidence which bears on syntactic analysis.

Let us now turn to the question of what form such a model should take, from the phonological point of view. I take as my guide the following quote from Cole

(1995:108):

“A theory of phonology that can account for the phenomena attributed to cyclicity must include (1) a subtheory of domains which can construct domains on the basis of morphological structure, though not necessarily isomorphic to that structure, within which certain phonological rules may apply; (2) a condition... which restricts certain rules from applying in monomorphemic environments; and (3) a mechanism for modeling the interaction that can occur between rules applying in cyclic domains and those applying in the larger domains defined by word and phrase structure.”

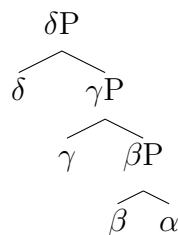
In the text to follow, I describe how PDbP achieves each of these three desiderata.

5.2.1 Phases & Spell-Out

Before going any further, we should clarify how the basic phase architecture works.

Consider a syntactic tree like the one below.

(192)



This is a static picture of a syntactic derivation at a particular point in time. Let's follow that derivation step by step. Conventional wisdom states that elements are merged in the syntax two at a time, from the bottom of the tree to the top.⁵ The first stage in the construction of (192), then, is a syntactic object β merging with another syntactic object α . This creates a set, $\{\beta, \alpha\}$. Depending on certain properties (which we may set aside for present purposes), either β or α will ‘project,’

⁵For justification, see Chomsky (2008), which grounds binary Merge and the requirement that Merge be ‘to the edge’ in basic principles of computational efficiency.

that is, one of them will be selected as the head of the phrase under construction. We represent this state of affairs as $\{\beta \{\beta, \alpha\}\}$. (In (192), merely for convenience, I label the new projection as β P, i.e., a β Phrase.) The derivation proceeds in this fashion, creating $\{\gamma \{\beta \{\beta, \alpha\}\}\}$, and so forth.

At certain points, the derivation is punctuated by the introduction of an element which bears the property of being a ‘phase head.’ Again, the factors that decide what is and what isn’t a phase head are not directly relevant to us. What is crucial is that phase heads initiate what is called Transfer or Spell-Out, sending a chunk of the completed derivation to the semantic and phonological systems. Specifically, the complement of a phase head is the chunk that gets transferred, at the point when another phase head enters the derivation. Upon transfer, the ‘spell-out domain’ (transferred chunk) is rendered opaque to further syntactic operations. This is formalized in the Phase Impenetrability Condition:

(193) PHASE IMPENETRABILITY CONDITON (Chomsky 2001)⁶

For $[ZP Z \dots [HP \alpha [H YP]]]$: The domain of H is not accessible to operations at ZP, but only H and its edge.

Let us now assume for illustrative purposes that each of $\alpha, \beta, \gamma, \delta$ in (192) are all phase heads. Then the steps of the derivation yielding that are as follows.

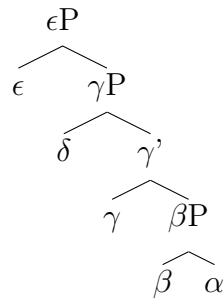
⁶There are two versions of the Phase Impenetrability Condition, the original from Chomsky (2000) (sometimes called *PIC*₁) and the newer version presented here, from Chomsky (2001) (*PIC*₂). One difference between the two formulations is crucial to us: under *PIC*₁, transfer was triggered immediately by the introduction of a phase head α , whereas under *PIC*₂, transfer of α ’s complement domain is delayed until a second phase head β enters the derivation.

(194) Derivation of (192)

- a. Merge (β, α) : α accessible to β .
- b. Merge $(\gamma, \beta\text{P})$: β accessible to γ . α transferred.
- c. Merge $(\delta, \gamma\text{P})$: γ accessible to δ . βP transferred.

However, in syntax the situation is virtually never like this, with a sequence $\alpha, \beta, \gamma, \delta$ all phase heads, and all simplex (non-branching, terminal) nodes. Typically (or perhaps even necessarily; see Richards 2006a), phase heads and non-phase heads alternate with one another, so the chunks being transferred are larger than depicted in (194). Let's consider a more realistic situation in (195) below. Assume that only γ and ϵ are phase heads:

(195)



The derivation will proceed as follows:

- (196)
- a. Merge (β, α) : α accessible to β .
 - b. Merge $(\gamma, \beta\text{P})$: β, α accessible to γ .
 - c. Merge (δ, γ') : γ accessible to δ .
 - d. Merge $(\epsilon, \gamma\text{P})$: δ, γ accessible to ϵ . βP transferred.

Here are some general guidelines which can be seen by inspecting (195)-(196): a phase head's complement (such as βP in (195)) is accessible only to the phase head

(γ) and any non-branching specifiers within the same phrase (δ). A phase head (γ) is accessible up to the next c-commanding phase head (ϵ). A complex specifier or adjunct forms its own spell-out domain (Uriagereka 1999); it is ‘constructed in a separate workspace’ from the rest of the structure.⁷

One important clarification is necessary in order to enable us to make broader use of the Phase Impenetrability Condition. In narrow syntax, ‘accessible to operations’ essentially means eligible for movement (i.e., Internal Merge or Re-Merge), and able to participate in Agree. For phonological purposes, I will move forward under the assumption that an ‘accessible’ string of phonology is visible to SEARCH and can be modified by COPY and DELETE. Now let us assume that phase impenetrability holds in phonology, so each phonological string becomes inaccessible subsequent to the transfer of another string to the phonological component. This is the fundamental assumption upon which the analyses of stress assignment proposed in Marvin (2002) rest, and upon which I will build in the remainder of this chapter. By preventing ‘reaching back too far’ into the derivation, the Phase Impenetrability Condition derives the effects previously attributed to the deletion of morpheme

⁷In the discussion to follow, I assume that Uriagereka’s conception of Multiple Spell-Out (i.e., complex specifiers and adjuncts are spelled out alone) and Chomsky’s phase framework are compatible. One means for accomplishing this is suggested in recent proposals by Narita (2009) and Boeckx (2008), who argue that only simplex syntactic objects can undergo Merge: complex objects introduced on a left branch must therefore be reduced to simplex objects before they can be integrated with the main derivational spine. This is achieved by the transfer of all but the head of the mergee. That is to say, complex specifiers and adjuncts must be headed by phase heads. Note that Newell (2008), in her discussion of the phase as a phonological domain, also makes the assumption that Uriagereka-style and Chomsky-style spell-out domains can be simultaneously entertained.

boundaries ('bracket erasure') at the end of every cycle (Siegel 1974, Mohanan 1982), opacifying the results of earlier ones. In other words, a rule can only affect something on its own cycle and/or the previous one, nothing more. The solution adopted here is similar in spirit to Pesetsky (1979) and the Lexical Phonology tradition following it: word-building operations and phonological rules interleave, and the Phase Impenetrability Condition prevents modifying previous cycles after they are built. I will expand on this shortly.

But first, if we are going to pursue this type of theory, we must identify what is a phase head, and therefore what is a spell-out domain. This is an actively evolving area of syntactic theory, but the picture presented here is fairly mainstream. Chomsky (2001 et seq.) takes C and transitive *v* to be phase heads; Legate (2003), Marvin (2002), Marantz (2008), and others argue that *v* must be a phase head in unaccusative and passive constructions as well. Crucially, T is not a phase head. Svenonius (2004), Bošković (2005), and Ott (2008), among others, argue for D as a phase head, and I will follow them here. McGinnis (2001) adds the High Applicative (ApplH) to this list. Other questions remain open, such as whether P is also a phase head (see Abels 2003). It is my hope that the present work will open the door for phonological effects to shed some light on these unresolved matters, but for the time being, I will limit the examples used for illustrative purposes here to the less controversial cases.

Phasal domains are also identifiable within words. Parallel to *v*, Marantz (2001) establishes $\{n, a\}$ as phase heads. In Distributed Morphology terms, following Marantz (1997), these elements are the categorial heads to which a-categorial roots must merge, and derivational affixes also belong to these classes. Marvin (2002) and Di Sciullo

(2004, 2005) argue on multiple independent grounds that the Phase Impenetrability Condition holds for these ‘morphological phases.’

It is interesting at this juncture to compare this inventory of phase heads to Chomsky & Halle (1968:366)’s rule of phonological phrase-building. In *SPE*, a # boundary is automatically inserted “at the beginning and end of every string dominated by...one of the lexical categories ‘noun,’ ‘verb,’ ‘adjective,’ or by a category such as ‘sentence,’ ‘noun phrase,’ ‘verb phrase.’” PDbP provides a new and principled way of understanding why precisely *these* objects should constitute phonological domains.

The strongest claim made by the PDbP approach (call it the ‘strong PDbP thesis’), and the one to which I will adhere, is that spell-out domains are the *only* domains that phonology needs. In other words, this is PDbP’s answer to Cole’s (1995) desideratum (1), quoted in the previous section: both the levels of Lexical Phonology and the constituents of the prosodic hierarchy come for free when we assume Distributed Morphology and a phasal syntax: phonological domains are directly imposed by morphosyntactic structure, and phonology need not erect any boundaries. It has been recognized for at least forty years (i.e., at least back to *SPE*) that phonological domains correspond—in some fashion—to morphosyntactic ones. If the correspondence is not one of exact congruence, then phonology must construct (or adjust) boundaries. But if the correspondence *is* exact, then phonology can simply ‘read’ the structures it is given. Theories that assume exact correspondence subscribe to the ‘direct reference’ conception of the syntax/phonology interface; see Kaisse (1985), Odden (1990), Cinque (1993). In recent literature, it is common to read that direct reference cannot

be correct because there are mismatches between syntactic and phonological domains. This is the position held by proponents of ‘indirect reference’ theories such as Selkirk (1984), Nespor & Vogel (1986), Truckenbrodt (1995), Seidl (2001), and many others. If PDbP is correct, there is no need to abandon direct reference for an indirect theory. In fact, the situation is even better: phonology doesn’t have to ‘read’ syntactic boundaries, it just applies to each chunk as it is received.

5.2.2 How derivations proceed

Now that I have described the basic skeleton of PDbP, let me put some flesh onto its bones. The story of PDbP properly begins at the syntax-to-phonology transfer. According to the Distributed Morphology hypothesis of Late Insertion, phonological content enters the derivation at Spell-Out, and requires licensing by functional heads (‘f-morphemes,’ like our phase heads *v*, *n*, *a*). The phonological component sits waiting until the first application of Spell-Out is triggered by the introduction of a phase head. (Recall this will correspond to the introduction of the *second* phase head in the syntactic derivation, but it will transfer the complement domain of the *first* phase head.) Further operations take place at each of these levels, filtering out illicit structures and preparing the spelled-out content for the semantic component and the phonological one. Most importantly, on the way to phonology, the hierarchical syntactic structure is linearized, and it is this linear string which the phonological component receives. Sensitivity to linear adjacency rather than dominance is a hallmark of morphological and phonological processes, as opposed to syntactic ones. I will have more to say about linearization throughout this chapter, especially in §5.5

on phonologically null syntactic objects. Another important event that happens between narrow syntax and phonology is copy deletion; typically only one copy in a chain created by Internal/Re-Merge can be pronounced. This deletion process will be elaborated upon later in this chapter, during our discussion of second-position clitics. But for the time being, we'll consider what happens when these preparatory operations at the interface are complete and a string arrives in the phonological workspace.

I take it as given that derivations cannot and should not be able to 'look ahead' to subsequent steps. I also take it as given that sometimes ill-formed phonological objects are created. Phonological operations can therefore be seen as 'interface-driven repair' in the sense of Calabrese (1995, 2005). I have already discussed several of these repairs: valuation of underspecified features, flattening of looped structures, and so forth. Now we will specify more precisely *when* these repairs occur. The simple answer is: immediately, and because of the Phase Impenetrability Condition, never later.⁸ We are forced to adopt this view if there is no look-ahead. For instance, the phonological system cannot know that the phonotactically-illicit structure in the current cycle will be rendered licit by a suffix which has not yet been transferred. Its only choice is to make the necessary repairs immediately, just in case.⁹

⁸I should clarify that by 'immediately' I mean 'within that cycle.' I maintain that phonological rules must be extrinsically ordered within a cycle: in other words, that their ordering must be learned (see Bromberger & Halle (1989) and Vaux (2008) for arguments to this effect).

⁹To enforce this, Piggott & Newell (2006:16) propose a 'codicil' to the Phase Impenetrability Condition which they call Phase Integrity: "Conditions on the well-formedness of prosodic categories are imposed on all elements that emerge within a phase α , if the elements are solely within phase

In principle, the first time repairs can happen, then—the first time phonological operations can apply—is when a phase head sends its downstairs neighbor’s complement to be transferred to the phonology. But on the basis of empirical evidence, we know that this first potential cycle of phonological rule application does not actually happen (see also the discussion surrounding (197) later in this chapter). Consider the case in which the first string to enter the phonological component is a root (transferred by virtue of being the complement of *v*, *n*, or *a*). If repairs applied at the stage when only the root was visible in the phonology, then no root would ever be able to escape phonological rules, even when it appears in an affixed form. This is not true: \sqrt{metr} escapes schwa-insertion when it is part of *metric* (in which case \sqrt{metr} is the complement of the adjectival head, *-ic*). Otherwise, we would expect something like [mirərɪk] rather than [mɛtrɪk]. So the first actual phonological cycle must not occur until the phonology receives a *second* string.¹⁰

Why might this be? It has long been recognized that all cyclic phonological processes are dependent on there being two cycles’ worth of strings in the workspace. This is the general force of the Strict Cycle Condition.

α.” This formalizes the prohibition against look-ahead, but I feel it is unnecessary to posit such a constraint, since the impossibility of looking ahead is an inherent property of stepwise derivations in general.

¹⁰Newell (2008) achieves the same result—the delay of the first cycle—by arguing that morpheme-level phases are spelled out differently from clause-level ones: the root and the innermost categorial head are spelled out together.

(197) STRICT CYCLE CONDITION (SCC; here from Kenstowicz 1994:208)¹¹

A cyclic rule may apply to a string x just in case either of the following holds:

- a. (SCC_1) The rule makes crucial reference to information in the representation that spans the boundary between the current cycle and the preceding one.
- b. (SCC_2) The rule applies solely within the domain of the previous cycle but crucially refers to information supplied by a rule operating on the current cycle.

As Kenstowicz (1994:208) explains, “The first rule to apply on any cycle must apply by case (a) [SCC_1], since a rule can apply by case (b) [SCC_2] only if some preceding rule has applied on the current cycle. But if the first application on any given cycle goes by case (a), then it also follows that no cyclic rule may apply on the innermost cycle of a derivation — for lack of a cyclic boundary.”

From my perspective, the absence of this potential first cycle is particularly striking. I believe it is driven by a commonly-recognized interface requirement, namely the requirement that the phonological component generate a fully-ordered linear string. This is commonly believed to be a condition on the syntax-phonology interface, but I argue that it also restricts the phonological system’s output to the Sensory-Motor

¹¹The biclausal, disjunctive version here forms the basis for most phonological discussions of strict cyclicity and ‘derived environment’ effects. However, it is worth noting that syntacticians may have in mind a different version of the Strict Cycle Condition, which was first proposed for syntax by Chomsky (1973:243): “No rule can apply to a domain dominated by a cyclic node A in such a way as to affect solely a proper subdomain of A dominated by a node B which is also a cyclic node.”

system (hence the requirement that looped precedence relations be linearized on each cycle in the phonology, as discussed in §4.3.5). A phonological derivation can only converge, therefore, as long as the newly-introduced string can concatenate¹² with the string on the previous cycle. Thus, the very first thing that happens on every phonological cycle is that the previous string is concatenated with the newly-introduced one. This is particularly obvious in the case where a bound affix is being introduced; it must concatenate with a host or the derivation will crash. But if I am correct, this is not only true of affixes.¹³ There must always be one accessible element in the derivation so that concatenation can occur on the next cycle. During the first step there is only one element at all, which means that it must remain accessible when the next transfer happens.

All phonological processes are in a sense parasitic on this fact, since concatenation opens the door for SEARCH to enter the previous cycle—which, by the Strict Cycle Condition, it *always* does in cases of successful rule application. If phonology applied to one morpheme at a time, it would quickly run itself out of business. Setting the issue of concatenation aside, think of what would happen if phonological rules applied on the first cycle, to a root all by itself, with the root becoming opaque prior to transfer of the second cycle. *Every* derivation starting with that root would result in

¹²This concatenation operation is not the same as Merge in syntax: Merge combines two objects symmetrically, whereas Concatenate combines two objects asymmetrically. Iterative applications of Concatenate yield a flat linear structure, but iterative applications of Merge yield a nested hierarchical structure. See Samuels & Boeckx (2009).

¹³This amounts to a rejection of the Typed Linearization Hypothesis and the Sufficient Ordering Hypothesis of Embick (2003).

it taking the exact same shape. Since roots would not participate in any observable alternations, they all would simply be learned as-is, and there would be no need for repairs at all, since all stored forms would be fully licit.¹⁴ In short, a form is stored as it appears on the surface (on the phonemic level), unless that form participates in visible alternations. Such alternations are necessarily dependent on concatenation with another morpheme, which is why underspecification must always be resolved by looking *back* into the derivation. In the absence of look-ahead, an underspecified morpheme cannot simply wait and hope that a potential valuator will enter the derivation, preventing it from crashing. (Recall our discussion of underspecification in Chapter 3.)

My conclusion, then, is that the first *actual* phonological cycle applies on the second occurrence of transfer, namely to the phase head and its complement. At this stage, the phase head and complement are visible to each other, so SEARCH can look into them, and COPY and DELETE can modify them. But even if the phase head is phonologically null, its complement nevertheless becomes completely opaque to future instances of SEARCH on subsequent cycles (i.e., upon the transfer of the next spell-out domain to phonology).

This process is iterated. Every time another string is sent to the phonology, some set of phonological rules gets the chance to apply: when conditions on SEARCH are newly satisfied by the material entering in that phase, the rule can modify the visible material from the previous phase. Moreover, since $\{n, v, a\}$ are all phase heads, every

¹⁴Cf. Lass & Anderson (1975:231): “If any phone appears in a non-alternating form, it must be lexical in that form. No segment then which appears in non-alternating forms may not be lexical.”

derivational affix triggers a phonological cycle.¹⁵ This is a desired result, as Orgun (1996) has argued on the basis of vowel raising and elision interactions in Qashgar Uyghur that each affix must have its own phonological cycle (see Nevins & Vaux (2008), Vaux (2008) for further illustrations of this point). In light of such data, it seems the problem with Lexical Phonology was not that it had too many levels, but rather that there cannot be a fixed number of levels in a given language, period. Having one cycle per morpheme also yields as a theorem the Natural Bracketing Hypothesis:

(198) NATURAL BRACKETING HYPOTHESIS (Brame 1972)

A substring ψ of a string ϕ is a domain of cyclic rule application in phonology only if it shows up elsewhere as an independent word sequence which enters compositionally into the determination of the meaning of ϕ .

Note in this connection that ψ may ‘accidentally’ not be a word, as in many *-ation* nouns that are not paired with corresponding *-ate* verbs. The ease with which these are backformed (*conversate*, *orientate*, *constellate*) gives weight to this hypothesis. At first glance, the last part of the Natural Bracketing Hypothesis—that ψ must figure *compositionally* into the meaning of ϕ —does not appear to follow by what we have said thus far. However, Marvin (2002) provides several examples of minimal

¹⁵It is interesting to note that this entails the possibility of a language that has ‘no phonology’ (or more accurately, no lexical rules) because it has no derivational affixes. This is precisely what Sandler (2008) has claimed for Al-Sayyid Bedouin Sign Language, in which the only morphological process is compounding, and only in compounds is a “kernel of proto-phonology” in the form of assimilation beginning to emerge.

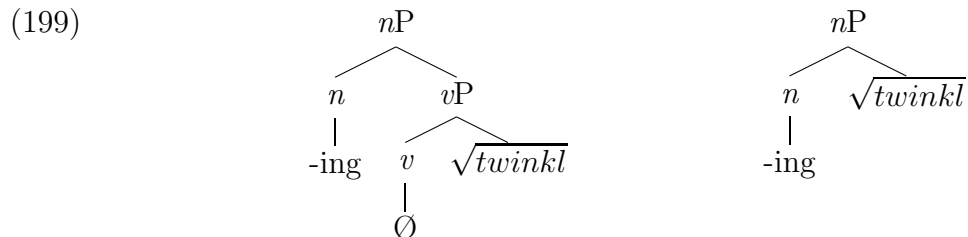
pairs in which one member is semantically compositional and phonologically regular, while the other member is semantically opaque and phonologically irregular. These include *twink*[kəl]ing (gleaming) vs. *twin*[kl]ing (an instant) and *shor*[tən]ing (making shorter) vs. *shor*[tn]ing (fat used in cooking). Marvin explains this phenomenon as follows, in keeping with the proposal by Marantz (1997) (see also Embick & Marantz (2008), Embick (2008)):

“[T]he attachment site of category-forming affixes is relevant for both meaning and pronunciation. If an affix is attached directly to the root, the meaning of the whole can be idiosyncratic (unpredictable). This follows from the fact that the root meaning itself is unpredictable and encyclopedic knowledge has to be evoked in order to negotiate the meaning of the root in the context of the category-forming head. If an affix is attached on top of the root that already has a category-forming affix attached, the meaning of the whole is predictable from the meaning of the upper affix and the unit it attaches to, because the meaning of this unit, comprising the root and lower category-forming affix, has already been negotiated at this point.” (Marvin 2002:31)

Thus, the regular forms in the minimal pairs I mentioned above are made by *-ing* attaching to a *v*(P) that has been constructed from a root plus a *v* head, which entails that the root is repaired (by schwa-insertion) prior to the introduction of *-ing*. The opaque forms, by contrast, involve (in Marvin’s view) direct affixation of *-ing* to the a-categorial root, or (in my view) may be stored directly in the lexicon as a simplex form.¹⁶

¹⁶Which of these options is ultimately correct does not affect the outcome of the derivation. If the opaque forms are stored as simplex forms, one might argue that the derivationally-produced forms of *lightning* and *shortening* consist of $\sqrt{\text{light}}/\sqrt{\text{short}}$ plus a *v* head *-n* and then the *n* head *-ing*. Either way, the first phonological cycle will operate over the root and its sister: *lightn-* and *shortn-* in the predictable cases but *lightning* and *shortening* in the opaque cases. Thus, the desired contrast is still achieved.

I illustrate this graphically in (199) below with the case of *t*, following Marvin's posited structures.



This example also illustrates another important difference between Lexical Phonology and PDbP. In Lexical Phonology, one would have said that *-ing* exhibits ‘double membership,’ the ubiquity of which was seen by some as problematic (Aronoff 1976): *-ing* acts as a Level 2 (word-level) suffix in the transparent cases and as a Level 1 (stem-level) affix in the opaque cases (or again, we might argue that the opaque forms are stored as such in the lexicon). In PDbP, there is no notion of a morpheme belonging to a particular level at all. There are simply affixes that subcategorize for different types of complements (*v*, *n*, *a*, $\sqrt{\quad}$, final stress, animate, etc.). The rampant double membership that troubled Aronoff is no problem for PDbP, because it merely reflects the fact that a given affix can appear in different structural positions relative to a root; that is, it can subcategorize for more than one type of complement. (It would in fact require an additional stipulation, and run counter to much empirical evidence, to rule out this possibility.) If a root has already been rendered opaque by the Phase Impenetrability Condition prior to the introduction of a given affix, the affix will not be able to change that root, and it will thereby act (in that particular instance) as though it belongs to Level 2. Otherwise, if the affix attaches directly to the root without any intervening phase head, it can (if, following Halle & Vergnaud (1987), it is marked as being able to do so) effect changes on the root, such

as stress re-assignment, which in Lexical Phonology is taken to be the major defining characteristic of English Level 1 affixes.¹⁷

The fact that all derivational affixes are phase heads, the Phase Impenetrability Condition, and the binarity of branching conspire to produce exactly Lexical Phonology's Affix Ordering Generalization (Selkirk 1982): only one affix—the innermost—may (potentially) belong to Level 1, because a root can only have one sister. All other affixes will find the root inaccessible and must therefore act as though they belong to Level 2.¹⁸ In the case where the root merges first with a null categorial head, any affixes which may attach will all act as though they are in Level 2. Another morphological consequence of derivational affixes being phase heads is that they undergo vocabulary insertion on different cycles and thus cannot undergo fusion, which explains an observation attributed to David Perlmutter to the effect that there are no portmanteaux derivational affixes; see Embick (2008) for discussion.

¹⁷Gussenhoven (1991b) confirms that it is still necessary to mark affixes as triggering stress-assignment rules or not; there are some affixes, like *-ize*, which undergo Level 1 segmental phenomena but behave irregularly when it comes to stress. Thus, the difference between affixes that can and cannot affect stress does not reduce to a difference in attachment position in the model proposed here.

¹⁸Newell (2008) makes this distinction in a different way, by treating affixes which do not change the category of the prior structure as 'morphological adjuncts.' This misses the generalization that *no* affix, whether it is category-changing or not, can affect the root if it is not the innermost; also, the idea of an adjunct in morphology is suspect, requiring the assumption that both pair-Merge (i.e., adjunction) and set-Merge (the normal, symmetric case) exist at this level. I will not discuss this issue any further here, but see Samuels & Boeckx (2009) on the relevance of the pair- vs. set-Merge distinction for phonology.

This says nothing, however, about the controversial ‘loop’ of Halle & Mohanan (1985), which allows for the interleaving of compounding (posited to be in its own stratum, Level 3) and Level 2 affixation. My opinion is that much ado has been made about nothing. A compound is, after all, a word just like any other: its internal structure has been opacified so it is treated as an atomic unit by later operations. So it would actually take special pleading to *rule out* affixation applying to compounds. From the affix’s point of view, all that is visible is a single item. This, again, is an effect of categorial heads being phase heads. The Phase Impenetrability Condition prevents the affix from seeing any deeper than the outermost phase head, the one which determines the final categorial membership of the compound.

5.2.3 Cyclicity & lexical rule application

Let me now explain more about the nature of rules in PDbP, their specific location in the grammar, and how they come to apply. It should already be very apparent, as I have stressed throughout this dissertation, and particularly in Chapter 2, that the theory advanced here is meant to be Minimalist in character. One notion essential to Minimalism is the idea that the lexicon is the source of all the differences among languages, often expressed as the idea that ‘all parameters are lexical,’ dating back to Borer (1984). Syntax itself is universal (see Boeckx (To appear) for lucid discussion), and in the previous two chapters I described what I believe is the universal core of phonology. But, as Otto Jespersen famously said (as quoted by Chomsky 1995b), “no one ever dreamed of a universal morphology.” Idiosyncrasies can, and should, be expressed as properties of lexical items.

When I introduced SEARCH and COPY, I mentioned that every affix must be lexically specified with the parameters on the SEARCH and COPY operations it will initiate in order to attach to its host. I also follow Marvin (2002), who argues that in PDbP affixes are specified as potentially triggering stress re-assignment or not; whether that potential is realized depends on the affix's position in a particular instance (i.e., whether it is sequestered from its target because of phase impenetrability). There are surely more lexically-specified rules than just these. For instance, in Irish, there are three homophonous possessive pronouns: the third person singular masculine, third person singular feminine, and third person plural are all pronounced as [ə]. However, each has a different effect on the initial consonant of the following noun (possessee), as shown in (200) below with the noun *cat* 'cat':

- (200) a. [ək]at 'her cat' (no mutation)
 b. [əx]at 'his cat' (lenition)
 c. [əg]at 'their cat' (eclipsis)

It is hard to imagine how this effect could be captured, if not by rules specific to the individual possessive pronouns.

At the same time, there are some rules that seem to apply at particular points in a derivation, regardless of what morphemes are involved. The most notable of these are in Lexical Phonology termed 'post-lexical' rules, meaning they apply across (some) word boundaries. It has long been noted that such 'late' rules have different properties from the earlier, cyclic lexical rules (see Coetzee & Pater (To appear) for a recent overview).¹⁹ While lexical rules are subject to exceptions on a morpheme-

¹⁹These differences run parallel to the observation made by Di Sciullo (2004, 2005): morphological

by-morpheme basis in precisely the manner illustrated by (200), post-lexical rules are exceptionless from a morphological point of view. However, they can be sensitive to syntax, as has been recognized since Chomsky et al. (1956). This, of course, is exactly what PDbP is meant to capture. Post-lexical rules also tend to be gradient, and to be sensitive to performance factors such as speech rate/style/register and lexical frequency.

I maintain the distinction between these two rule types by arguing that all phonological rules obey the Phase Impenetrability Condition, but in one of two different ways. Lexical rules must obey the Phase Impenetrability Condition at both the morpheme level (phase heads *n*, *a*, etc.) and the clausal level (phase heads *v*, C, D, etc.); we will return in §5.3.2 to the ambiguous status of *v* with regard to the morpheme-/clause-level distinction. Post-lexical rules apply once a sequence of morphemes has been turned into an atomic unit and obey the Phase Impenetrability Condition only at the clausal level. The way I use the terms lexical and post-lexical here roughly corresponds to the distinction between ‘concatenation’ and ‘chaining’ rules in Pak (2008).

Upon observing a particular alternation, how are we to determine whether it results from a lexical or a post-lexical rule? As we have already noted, descriptively they differ in a number of ways. For instance, lexical rules are subject to lexical exceptions. Taking this to its logical conclusion, and in keeping with the Minimalist desire to place all idiosyncrasies in the lexicon, I adopt the *SPE* position that indiselection is similar to but more restricted than syntactic selection. Lexical rules are similar to (in fact, often the same as) lexical rules but more restricted.

vidual lexical entries can be specified as [-rule X]. One might think of there being an ordered list of rules that are associated with each lexical category, which are automatically called up by individual lexical items belonging to that category unless otherwise specified. Post-lexical rules, in contrast, are germane only to functional categories: they are properties of the clause-level phase heads *v*, C, D, etc., and apply to the whole complement domain of the head on which they are carried, down to the next phase head. (It is possible, as we will see in the case of Basque presented later in this chapter, for clause-level phase heads to host lexical rules also.) Unlike lexical rules, post-lexical rules only apply to *one* (clause-level) spell-out domain. As we will see in §5.4 (primarily §5.4.4), this creates a situation in which a lexical rule can span a boundary between two clause-level domains while a post-lexical rule cannot.²⁰

Viewing the lexical/post-lexical distinction in this way explains several things that were established in Lexical Phonology. First, there is no way for post-lexical rules to have lexical exceptions if the string is already opacified in this way when they apply. Also, the model I have suggested turns the cyclic/post-cyclic dichotomy on its head and in doing so, helps to explain some cyclic effects. In Lexical Phonology, one major debate concerned whether the classes of lexical and post-lexical rules could be identified with the classes of cyclic and post-cyclic rules, respectively (for an overview see McMahon 2000 §2.3). Let me try to make explicit all the restrictions on cyclic rules captured by the Strict Cycle Condition (197) and then explain how these properties

²⁰This is another way in which the theory presented here differs from that of Pak (2008). For Pak, concatenation rules are not only limited to two morpheme-level domains which are within a single clause-level spell-out domain; we will see examples later in which it is important that lexical rules can apply across *any* two adjacent morphemes even if they span a clause-level phase boundary.

can all be viewed as consequences of PDbP. Cyclic rules are expected to obey the following:

(201) Conditions on cyclic rules

- a. No rule can operate on the first (root-level) cycle.
- b. A rule on cycle n cannot alter any structure contained within cycles prior to $n-1$, or refer to any structure on cycles after n .
- c. A rule on cycle n that only refers to structure introduced on cycle n is not permitted.
- d. A rule on cycle n that refers to a structure contained within cycles n **and** $n-1$ may apply.
- e. A rule on cycle n that affects only structure properly contained within cycle $n-1$ can only apply if cycle $n-1$ was already modified by an earlier rule on cycle n (conforming to (d) above, or an earlier rule conforming to (e), itself licensed by a rule conforming to (d)).

Earlier in this chapter we already discussed the impossibility of rules applying to material which is contained within a single cycle (i.e., (201c)): such a rule would *always* apply, so the affected form would just be learned in the post-application form. We need not explicitly rule out the possibility of such processes—they will just never be posited by the learner. We also already discussed (201a). In short, the Strict Cycle Condition allows only rules by means of which one morpheme is modified on the basis of properties of *a different* morpheme (i.e., (201d, e)). Rules cannot look ahead, and what's more, two morphemes which affect each other must be on adjacent cycles, as (201c) makes clear. PDbP takes this type of opacity to be a characteristic property of

linguistic derivations, as reflected by the Phase Impenetrability Condition. This raises a very important issue, which is that the Strict Cycle Condition is only claimed to hold for cyclic rules. This makes sense when one considers that the cycles over which the Strict Cycle Condition was designed to hold were essentially morpheme-level ones—so we equate lexical rules with cyclic rules, as many in the Lexical Phonology literature have. In sum, the Phase Impenetrability Condition and general properties of the language acquisition process produce all of the effects that have at certain points been attributed to the Strict Cycle Condition or alternatives/amendments such as the Elsewhere Condition, the Bracket Erasure Convention, and Structure Preservation (see Kiparsky (1973), Siegel (1974), Mohanan (1982), Borowsky (1990), *inter alia*).

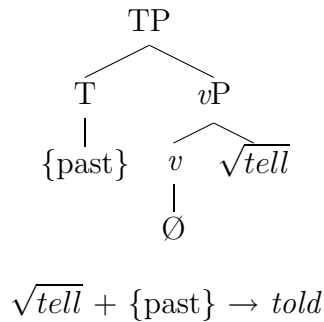
5.3 Apparent phase impenetrability violations

Now that I have laid the foundations of how I see PDbP as working at the morpheme level, I want to discuss several cases which Marvin (2002) claims are exceptions to the Phase Impenetrability Condition. I argue that all of these cases are actually unproblematic for the model presented here, in which the Phase Impenetrability Condition restricts the application of all rules.

5.3.1 T-root interactions

One alleged problem for the Phase Impenetrability Condition raised by Marvin concerns interactions between T and a verbal root. I will illustrate this first with irregular past tense forms in English. The structure under consideration is as follows:

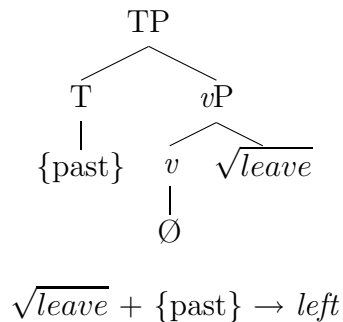
(202)



“The problem occurs with the English Past Tense forms of irregular verbs, where the temporal feature E, R_S (or [+past]) on Tense can change the pronunciation of the root as in [(202)] despite the fact that Tense is two phases above the root and given Phase Impenetrability Condition...should not be able to affect the Spell-Out of the root, which is spelled out at *vP*.” (Marvin 2002:165)

One could also formulate this problem in the opposite direction: not only can the presence of a particular tense affect the exponence of the root, the presence of a particular root can affect the exponence of T, selecting *-t* rather than the usual *-ed*, as shown below:

(203)



First, note that it is simply mistaken that T is “two phases above” the root. It is in fact in the spell-out domain immediately after the root’s: the root is spelled out by virtue of being in the complement domain of *v*, and T (along with *v*) is spelled out by virtue of being in the complement domain of C. Thus, the root is accessible to modification by T. This clarification eliminates another ostensive Phase

Impenetrability Condition violation of exactly the same type, involving present tense stress retraction in Slovenian (Marvin 2002:161ff). Embick (2008:31) notes that the following generalization seems to hold about interactions between a root (or non-cyclic head) and another non-cyclic node:

- (204) A non-cyclic (i.e. non-category-defining) head X can see a Root in spite of intervening cyclic node x , but this seems to happen only when x is non-overt. This is the situation in the English past tense, where the phonologically null v head does not prevent the T[past] head from having its allomorphy conditioned by the identity of the Root.

Embick hypothesizes that the cyclic node involved must be null to license this type of allomorphy because the null v node is “pruned” during linearization, creating linear adjacency between the root and the non-cyclic node T; vocabulary insertion then takes place at the stage where this local relationship has been established. For example, the presence of a root \sqrt{sing} causes T to be realized as $-\emptyset$ and also triggers a readjustment rule which changes the vowel in *sing* to *sang*. Pruning appears to be non-obligatory, however, and the circumstances under which pruning takes place remain to be investigated. I will have nothing more to say about this matter, but the important point here is that the Phase Impenetrability Condition does *not* rule out T-root interactions, as claimed by Marvin. The theory turns out to be not quite restricted enough, rather than too restrictive.

Even though the problem with *tell-told* and *leave-left* turns out to be illusory, this discussion brings up an interesting related issue: what about true suppletion of the type \sqrt{go} plus {past} = *went*?

“Canonical cases like those mentioned above—*be*, *go*, etc.—are *light verbs*: members of the functional vocabulary. Marantz (1995) and others have emphasized that within a theory with some late insertion, restricting suppletion to the functional vocabulary is an important desideratum. In this type of theory, suppletion is simply contextual allomorphy, but with ‘free-standing’ verbs etc. rather than with affixed morphemes. Thus, the fact that the element being realized is a verb—i.e., a kind of *v*—makes it more noticeable than other types of allomorphy, but the mechanism for handling these effects, involving competing [vocabulary items], are the same whether the object in question is an affix or a ‘stem.’” (Embick 2008:56)

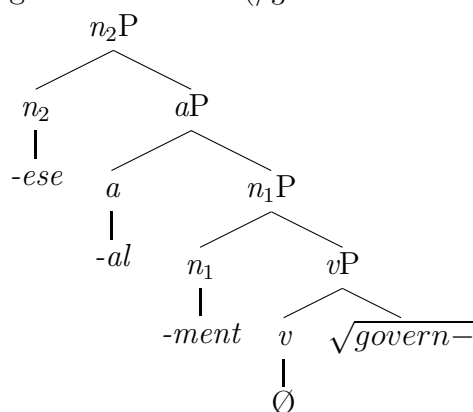
In short, the structure for generating *went* is different from the one that generates *told*, *left*, or *sang*. GO and BE are instantiations of *v*, not roots. Since T and *v* are in the same Spell-Out domain, they undergo vocabulary insertion on the same cycle (though it is still bottom-up within a cycle, so insertion occurs at *v* first, then T). T is present at the stage when *v* is given phonological content, so true suppletion is possible for light verbs; it is possible to insert a particular allomorph of *v* in the context of a particular T. But since the root (to become a verb in the presence of *v*) and T are *not* in the same Spell-Out domain, only phonological modification is possible for lexical verbs. Vocabulary insertion for the root node takes place before T is visible. It is then possible on the next cycle, when *v* and T enter, for a readjustment rule to modify the root. The Phase Impenetrability Condition permits this because the two domains are adjacent. Since the vocabulary item for *v* is inserted before that of T even on the same cycle, the spelling out of *v* can be sensitive to the *presence* of T and its morphological features, but not its phonological content. Conversely, since the root is visible to T, it can affect which exponent of T is selected for insertion.

5.3.2 Post-lexical stress assignment rules

The second group of putative exceptions to the Phase Impenetrability Condition discussed by Marvin (2002:50ff) involve stress assignment. Specifically, she cites three examples from English: the Compound Stress Rule, the Rhythm Rule, and the assignment of primary stress. We will discuss these in turn, beginning with primary stress assignment.

The problem with primary stress is illustrated by the interaction of stress assignment and vowel reduction in the word *governmentalese*, discussed by Marvin (2002:51ff). We will walk through this derivation step by step, as it appears in Marvin's analysis, ignoring the metrical grid above Line 1 for the moment. The structure of this word is shown in (205) below:

(205) *góvernmentálese* (/gʌvərnməntəlɪz/)



The relevant metrical rules, as in Halle & Vergnaud (1987) and reproduced by Marvin, construct feet as follows. First, there are two Edge-Marking Rules. The RLR version inserts a right parenthesis to the left of the rightmost syllable on Line 0 if that syllable contains a short vowel. The LLR version, which applies only if the RLR version has not, inserts a left parenthesis to the left of the rightmost syllable. The Edge-Marking Rules apply to a specified part of the English lexicon, and when

they do apply, they are ordered before the Main Stress Rule. The Main Stress Rule applies across the board. It inserts a right parenthesis so as to group the rightmost two asterisks in a binary foot if the final syllable is light; otherwise, if the final syllable is heavy or if the word is too short, then a unary foot is constructed. In the example below, I show feet erected by the Edge-Marking Rules with square brackets to keep them separate from those erected by the Main Stress Rule, which are notated with parentheses. The sequence marked as $\langle P^* \rangle$ in the Main Stress rule indicates either \rangle or $[$, created by the earlier application of an Edge-Marking rule, plus an asterisk.

(206) EDGE-MARKING RULES

a. RLR Edge Marking

$$\emptyset \rightarrow \rangle \text{ in env. } *_* \#\# \text{ Line 0}$$

Condition J: Final asterisk projects short vowel.

b. LLR Edge Marking

$$\emptyset \rightarrow [\text{ in env. } *_* \#\# \text{ Line 0}$$

(207) MAIN STRESS RULE

a. $\emptyset \rightarrow ($ in env. $_* * \langle P^* \rangle$ Line 0

Condition K: Second asterisk projects vowel in a light rime.

b. $\emptyset \rightarrow ($ in env. $_* \langle P^* \rangle \#\#$ Line 0

These rules feed a non-cyclic (post-lexical) rule of vowel reduction shown below.

(208) VOWEL REDUCTION RULE (non-cyclic)

$$[-\text{HIGH}] \rightarrow /ə/ \text{ if } \{\text{Line 1: } \emptyset\}, \{\text{Line 0: } *\}$$

Since the Edge-Marking Rules and Main Stress Rule are both cyclic, they first

apply to the chunk *govern-Ø* ($\sqrt{\quad} + v$). At this stage, the Main Stress Rule applies, creating a binary foot. The head of the foot projects to Line 1.²¹

(209) Line 1 *
 Line 0 (*]*
 gov ern

Next, *-ment* is spelled out and added to the structure. However, this suffix (like *-ing* and *-ness*) does not trigger either the Edge-Marking Rule or the Main Stress Rule. The derivation proceeds, yielding *gouvernement*:

(210) Line 1 *
 Line 0 (*]* *
 gov ern ment

The next step is the introduction of *-al*, which does trigger the cyclic stress rules. At this stage, *govern* is inaccessible because of phase impenetrability, but *-ment* remains accessible; on this cycle the RLR Edge-Marking Rule and the Main Stress Rule both apply. The head of the newly-created foot projects to Line 1. This yields *gouvernemental*.

(211) Line 1 * *
 Line 0 (*]* (*]*
 gov ern ment al

Then the cyclic affix *-ese* enters the derivation, again triggering the Main Stress Rule, producing *gouvernementalèse*, with the head of the new foot projecting to Line 1.

²¹Marvin (2002) omits showing the brackets created by the RLR Edge-Marking Rule on *govern*, for reasons which are not clear to me. Halle (1998) is explicit about the necessity of this rule applying to the verb *gouverner*, to a number of unsuffixed adjectives, and to adjectives in *-al* which show the same stress pattern as *gouvernemental*, such as *ànecdótal*, *sùicídal*, *sàcerdótal*, *àsteróidal*.

(212)	Line 1	*		*		*
	Line 0	(*]*	(*]*	(*
		gov	ern	ment	al	ése

Finally, vowel reduction applies across the board. Marvin uses this example to demonstrate that the Line 1 stresses from prior cycles cannot be ‘forgotten’ or ‘erased’ as the derivation proceeds, because it is precisely the vowels that have projected to Line 1 which escape vowel reduction. (This much must be true if we wish to stick with a more *SPE*-like derivation, avoiding the erasing and re-building of Halle & Vergnaud (1987); Marvin gives several compelling arguments to this effect.) However, vowel reduction (a post-lexical rule) cannot apply until spell-out of the last phase in the prosodic word, i.e., n_2P . For instance, if it applies on the cycle at which *government* is created, it will reduce the vowel in *-ment* to schwa, and this will carry over to the next cycle, giving the wrong results for *governmental* with a stressed full vowel in *-ment*. Thus, vowel reduction must wait until the final cycle to apply to the whole word, in apparent violation of the Phase Impenetrability Condition.

The question of which Line 1 stress will receive Line 2 (primary) stress is also relevant here, as Marvin also points out:

(213) PRIMARY STRESS ASSIGNMENT

- a. góvern
- b. góvernment
- c. gòvernmental
- d. gòvernmentalése

As should be evident from (213), the rightmost Line 1 stress is normally promoted,

but this, too, cannot happen until the whole word is completed. (This is normally attributed to the Nuclear Stress Rule or something like it.²²) What forces this interpretation is the fact that *-al* is a ‘pre-stressing’ suffix which causes primary stress to fall on *-ment*. If we assume that a Line 2 asterisk has already been assigned to *gov-* at this point, phase impenetrability ought to prevent removing that asterisk, or even making it visible such that *-ment* can ‘one-up’ it.

I proposed earlier in this chapter that post-lexical rules apply within a single clause-level spell-out domain. For a noun like *governmentalese*, this will typically be the complement of D. The only problem, then, is why $\sqrt{\text{govern}}$, as the complement of *v*, does not constitute its own clause-level domain. That is, *v* could plausibly belong to both the clause-level and morpheme-level sets, but here it does not trigger post-lexical rules. Furthermore, it cannot be that *v* only triggers post-lexical rules in a given configuration, because it stands in a head-complement relation with a root no matter whether it is inside a word like *governmentalese* or in a $[_{TP} T [_{vP} v [_{VP}]]]$ clausal structure (as in (202) below). One possible solution to this paradox becomes apparent when we consider that the typical diagnostic for the presence of *v* is the associated presence of an argument. There is no argument identifiable in *governmentalese* or similar words in which *v* ostensibly appears—but there is clearly *some* phase head enforcing the Phase Impenetrability Condition in these words, as the *twinkling-twink[ə]ling* example in (199) makes obvious. So if it is not a *v*, then what is it? The answer is suggested by Baker (2003§2.9), who has argued that a VP actually contains

²²See Kahnemuyipour (2004) and discussion later in this chapter for phase-based alternative to the Nuclear Stress Rule which is compatible with the theory presented here.

an AP: the AP denotes a property, and the V is a BE-operator on top of the AP, thus creating an unaccusative (with *v* a CAUSE or similar operator on top of the BE-operator, producing transitivity). I suggest along these lines that what has previously been called a word-internal *v* is actually an *a*, which means that the morpheme-level and clause-level phase heads are now disjoint sets; I depart from Baker by calling the BE-operator *v*, in line with more recent thinking on unaccusatives, since it introduces an argument. This does not change the number of morpheme-level domains within words like *governmentalese*, but it has the desired effect of preventing post-lexical rules triggered by clause-level phase heads from applying to a domain internal to derived words.

The two other stress assignment rules which Marvin discusses are the Compound Stress Rule and the Rhythm Rule. I give examples of the alternations covered by these two rules below:

(214) RHYTHM RULE

- a. thirtéén
- b. thirtèèn mén

(215) COMPOUND STRESS RULE

- a. tréal-and-érror
- b. tréal-and-érror mètthod

First, it is important to note that both of these phenomena are limited in their domains of application. For instance, the Rhythm Rule applies only within a phonological phrase (see Gussenhoven 1991a, Inkelas & Zec 1995, *inter alia*), and the same

seems to be true for German and Masoretic Hebrew, which have similar processes (see Liberman & Prince 1977). The Compound Rule has been argued to be lexical, specifically applying to formations created on Stratum 2 (see, e.g., Gussenhoven 1991b).

I argue that (214)-(215) are two manifestations of the same phenomenon, and tractable in a way that respects phase impenetrability. The key to my analysis comes from four experimental studies: two on compound stress, Kunter & Plag (2007) and Kösling & Plag (2008), and two on the Rhythm Rule in Connell & Arvaniti (1995). The upshot of the experimental findings is that at least some speakers do perceive stress shifts/reversals such as the ones shown in (214)-(215), but judgments are very unstable and context-dependent (i.e., judgments on shift are unclear when *thirtéen* is presented in isolation, particularly when subjects are not given a choice of reporting that both syllables are stressed equally). Furthermore, not all shifts are the result of stress clash (Horne 1993), so the Rhythm Rule as it has traditionally been construed—either as iambic reversal of the first two stresses or deletion of the middle stress in a series of three—cannot be maintained. I therefore follow Shattuck-Hufnagel (1995), who argues that the perception of stress shift is caused by the juxtaposition of pitch accents determined at the phrasal level with word-level stresses (which are not actually altered). That is to say, the percept of *thirtèen mén* emerges from the interaction of two phenomena: first, the word stresses (*thirtéen mén*), and second, the high tone which docks on the first stressable unit in the phrase, *thir-*. This meshes well with the findings of Kunter & Plag (2007) for compound stress: they show that the perceived primary stress in English noun-noun compounds is often signaled by

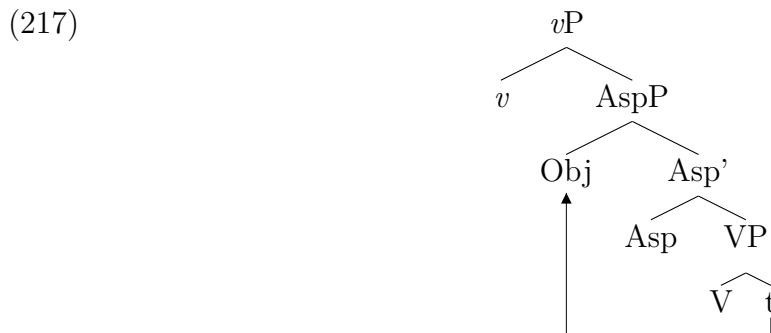
pitch accent alone. Of course the next question is how placement of this pitch accent occurs, which I will discuss below and in §5.4. The bottom line for present purposes is that the Rhythm Rule and Compound Rule are not independent; they both stem from a single source, namely the assignment of a pitch accent by a determiner head.

That this pitch accent should be associated with D-elements is suggested by the fact that Rhythm Rule (and compound stress) effects are, as far as I can tell, limited to DPs; all the examples of which I am aware (for example, cited by Liberman & Prince (1977), Halle & Vergnaud (1987), Gussenhoven (1991a)) are limited to the DP domain: *the Saginaw Michigan Gazette*, *one-twenty Jay Street*, *Boston Symphony Orchestra anniversary*, *law degree requirement changes*, *good-looking lifeguard*, etc. What I will ultimately suggest is that we can describe the effects previously attributed to the Rhythm Rule and the Compound Rule (and also Initial Accent Deletion; see Gussenhoven (1991a)) without violating the Phase Impenetrability Condition. This is achieved with an analysis parallel to the phase-based accounts of sentential stress which have been explored by authors including Kahnemuyipour (2004), Adger (2006), Ishihara (2007), and Kratzer & Selkirk (2007). In short, the insight behind these accounts is that sentential stress can and should be computed over syntactic representations, not by a rule within the phonological component. Here I will sketch out Kahnemuyipour's proposal for sentential stress assignment and then extend it to account for Rhythm Rule effects and compound stress.

One of the basic facts which Kahnemuyipour seeks to explain is that, in a neutral context, sentential stress falls on the object in both SVO and SOV languages. The following examples in English and Persian are from Kahnemuyipour (2004:88).

- (216) a. John bought a book.
 b. Ali ye ketaab xarid.
 Ali a book bought
 ‘Ali bought a book.’

Kahnemuyipour adopts an antisymmetric syntax, with the object moving to the specifier of AspP (an agreement projection between VP and *v*P) in both SVO and SOV languages; the verb moves to *v* to yield SVO but stays *in situ* for SOV. Stress assignment is very simple: it applies to the highest element in a Spell-Out domain. For Kahnemuyipour, phase heads are C and *v*; however, unaccusative and passive *v* are not.²³ In transitive sentences in both SOV and SVO languages, the object is the highest in *v*’s spell-out domain (complement) and therefore gets stressed: the verb has raised out of the domain in SVO languages, and while it remains within the domain in SOV languages, the object is higher.



²³As I mentioned earlier in this chapter, the status of *v* in various contexts remains controversial. If Kahnemuyipour’s analysis is correct, then perhaps phonology provides evidence which could potentially help shed some light on this issue. One could conceivably argue in the face of conflicting data, though, that *v* is a phase head in all contexts but does not trigger stress assignment in unaccusatives or passives. And Kahnemuyipour must in fact admit that an unaccusative/passive *v*P constitutes a phase when modifiers are added to it; see discussion in Kahnemuyipour (2004), pp. 137ff.

In the higher phase, the subject in Spec,TP receives stress as the highest element within C's complement domain. The prediction made here is correct—the subject does in fact receive stress along with the object in both SVO and SOV languages. But the theory does not immediately account for the fact that the object (or whatever highest element, such as an adverb, is stressed in *vP*) receives the main stress and the subject is only secondarily stressed (data from Kahnemuyipour (2004:154)):

(218) a. $\frac{\text{John}}{2}$ saw $\frac{\text{Mary}}{1}$.

b. $\frac{\text{Ali}}{2}$ $\frac{\text{ye ketaab}}{1}$ xarid.

Looking at sentences which involve clausal complementation, it becomes clear that the correct generalization about primary vs. secondary stress is that the lowest stress (i.e., the one in the first spell-out domain in a bottom-up derivation) is primary.

(219) a. $\frac{\text{John}}{2}$ told $\frac{\text{Mary}}{2}$ that $\frac{\text{Jane}}{2}$ saw $\frac{\text{Bill}}{1}$

b. $\frac{\text{Ali}}{2}$ be Maryam $\frac{\text{goft}}{2}$ ke $\frac{\text{Mina}}{2}$ $\frac{\text{qazaa}}{1}$ mi-xaad

Ali to Maryam said that Mina food want.3SG.DUR

'Ali said to Mary that Mina wants food.'

As Newell (2008) notes, if there is no look-ahead in the system, then assigning primary stress to the lowest phase is the only way to ensure that primary stress does in fact get assigned.

It would take us too far afield to discuss the wide range of cases in Persian, English, and German which Kahnemuyipour treats. However, later in this chapter I will refer to one further contrast between English and Persian with regards to stress assignment

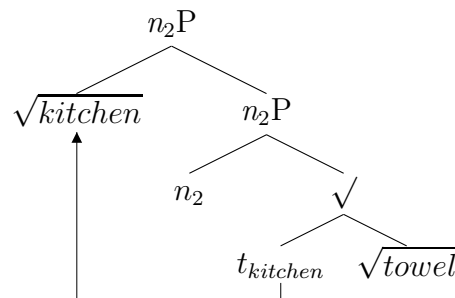
during our discussion of the Empty Complement Condition (242), which I introduce in §5.4.4. For now, though, the point I would like to make is that Rhythm Rule effects and compound stress stem from exactly the same rule as Kahnemuyipour's stress assignment, but in the DP domain rather than CP or *v*P. In other words, these effects provide evidence that D should be added to Kahnemuyipour's list of functional heads which assign stress to the highest element in their complement domain; D acts just like the other clause-level phase heads in this respect.

First I will verify that, with the addition of the pitch accent assigned by D, we can generate the stress pattern of *thírtèn mèn*. I assume that *thirteen* is normally assigned stress on both syllables, and that the primary stress perceived on its second syllable when it is pronounced in isolation is a result of phrasal stress or a phrase-final pitch accent. The derivation for *thírtèn mèn* goes as follows: first, *men* is spelled out alone as the complement of *n*. Next, the complement of D is spelled out, including *thirteen* sitting in NumP, between the *n* and D projections. On this cycle, *thirteen* is stressed, and so is *men*. I argue that the leftmost stress in the highest element in D's complement is accented, just as Kahnemuyipour has argued for *v*P and CP. The result is that, with this added accent, *thir-* is now more prominent than the other syllables. The nuclear stress and/or phrase-final pitch accent (normally a low tone, in a basic declarative sentence) may also wind up on *men*, depending on the context and sentence type, so the end result may be either *thírtèn mèn* or *thírtèn mén*.

Positing pitch accent assignment by D also seems to produce the correct result for nominal compounds. Little work has been done on the syntax of primary nominal

compounds,²⁴ particularly long ones, within the type of morphosyntactic framework PDbP assumes. This is very unfortunate considering the Compound Rule is exemplified by long strings of nouns like *teachers union president election*, *kitchen towel rack*, *law school language exam*, and so forth (e.g., Halle & Vergnaud 1987:271ff). Nevertheless, we can still confirm that the correct accentuation—which differs somewhat from what I presented in (214), as we are about to see—can be generated without violating the Phase Impenetrability Condition. I will assume that, at least for the nominal compounds at issue here, we are dealing with root incorporation (Wiltschko 2009). The structure I posit for two-member compounds is therefore as follows:

(220)



I will illustrate how stress assignment in a three-member nominal compound like *kitchen towel rack* could work. This compound can be interpreted in two ways: as a rack for kitchen towels (left-branching), or as a towel rack that is in the kitchen (right-branching). The stress pattern of the compound has been claimed to differ accordingly (see Chomsky & Halle 1968, Halle & Vergnaud 1987):

- (221) a. [[kítchen tòwel] ràck]
 b. [kítchen [tówel ràck]]

Kösling & Plag (2008) have undertaken what to my knowledge is the first large-scale study of stress in three-member nominal compounds. They analyze a large

²⁴As opposed to compounds which contain a derived verbal element, such as *truck driver*.

spoken corpus according to a number of structural, semantic, and acoustic measures, and find a considerable amount of variation in the stress patterns of compounds. However, the direction of branching makes a fairly good prediction about where stress will fall in a compound. They give an analysis in terms of pitch accent:

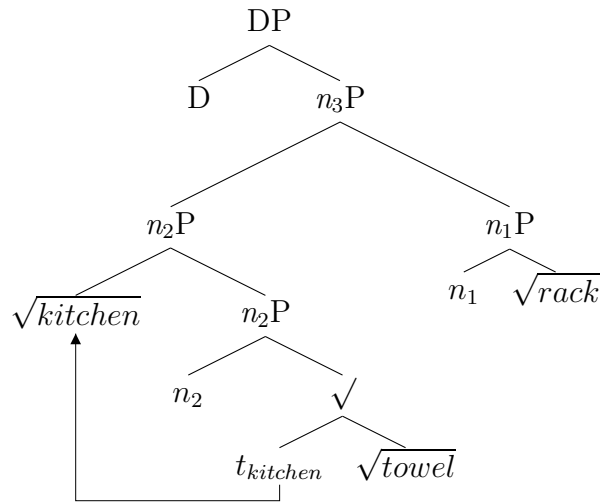
“Based on the typical pitch curve we found for left-branching compounds [...] we might argue that in general there is a single pitch accent assigned to constituent N1 [the leftmost member in the compound] with no pitch accents assigned to N2 and N3. This is indicated by a high pitch on N1 which decreases towards constituent N3. For right-branching compounds, we found that constituent N1 and constituent N2 in general do not differ in pitch. This strongly indicates that in right-branching compounds a pitch accent is assigned to N1 and N2.” (Kösling & Plag 2008:28)

This is precisely what my account predicts, under reasonable assumptions about the syntactic structures involved. In short, it appears as though the pitch accent assigned by D applies only to the leftmost member of a compound in a left-branching nominal compound, but in a right-branching one, it can apply to the *two* leftmost members.

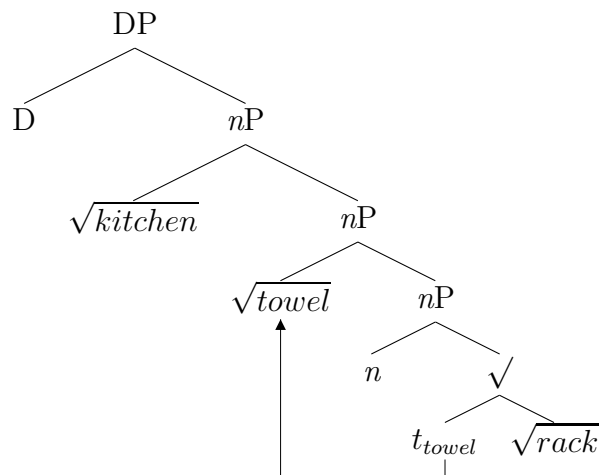
An arboreal representation of each bracketing is given below in (222). In (222a), corresponding to (221a), we see that *kitchen towel* is assembled as in (220), with subsequent movement of *kitchen* into the specifier of the n_2 P. This entire structure is then adjoined to *rack*'s n_1 P. In (222b), corresponding to (221b), *towel rack* is composed as in (220), again with movement of one of the roots into the specifier position, and then *kitchen* is adjoined to this n P.²⁵

²⁵Since so little work has been done on three-member compounds, the structures posited here must be seen as a tentative proposal. For the account of compound stress presented here to work, all that is strictly necessary is for *kitchen* to be the uniquely highest object in the complement of D

(222) a.



b.



We will now look at stress assignment in these structures, beginning with (222a). First, *rack* is spelled out alone. The next cycle, the first on which stress will be assigned, includes *rack* and the null *n*; *rack* will receive stress. The next spell-out domain is the complement of n_2 , *towel*. This word receives regular trochaic stress, as does *kitchen*. Next, D (and its edge, if any) are spelled out. *Kitchen* has the only stress visible to D, and it receives the pitch accent from D so it ends up with the most stress in the left-branching compound, and for *kitchen* and *towel* to be equidistant from D (and the highest objects in its complement) in the right-branching compound.

prominent stress in the compound. This derives the desired stress pattern, [[kítchen tòwel] ràck].

For the structure in (222b), there is only one cycle of rule application: *rack* is spelled out alone but as always, no rules can apply until the second transfer. On the next cycle, when the complement of D is spelled out, both *kitchen* and *towel* are introduced. Both of these words will receive trochaic stress as before, and *rack* will also again receive stress. Finally, D assigns its pitch accent. At this stage there are two elements in the edge of *nP*, visible to (or equidistant from) D: *kitchen* and *towel*. Accentuation by D can therefore apply to both *kitchen* and *towel*, yielding the desired result of pitch accent on both N1 and N2, in Kösling & Plag's terms.

The last point which I would like to make about this account of Rhythm Rule effects and compound stress is that, like Kahnemuyipour's (2004) account of sentential stress, it crucially relies on the notion of hierarchy: the pitch accent is assigned to the *highest* element in the phase head's complement. As I have tried to make clear throughout the present work, hierarchy is a syntactic notion with no place in phonology. The conclusion I draw from this, along with Féry & Ishihara (To appear) and Scheer (In press), is that intonation is assigned in the syntax. Only syntactic structure, not any properties of the phonological material which will eventually be assigned to that structure, is necessary to compute the intonation. The phonological system merely interprets the directions which it is given by the syntax. In the case of the phenomena which we have been considering, the syntax assigns what will ultimately be interpreted as a pitch accent to each element in D's complement; in the phonology, this pitch accent is placed via SEARCH and COPY (more specifically, an

FE-type rule) on the leftmost stressed element in each word. Note that *leftmost* is a linear notion, and therefore properly within the domain of phonology.

5.4 Prosody without hierarchy

In the previous sections, I focused on how PDbP captures some of the fundamental insights of Lexical Phonology, while improving upon it considerably in many respects. However, this is but one way in which PDbP changes the picture of phonology and its interfaces. The model developed in the previous sections can also lead us to a new understanding of phrase-level phonology, one that involves no boundary construction and eliminates the prosodic hierarchy. As Scheer (2008b:145fn1) notes, the relationship between Lexical Phonology and Prosodic Phonology has always been unclear, leading to claims that Lexical Phonology is redundant and should be eliminated (e.g., Selkirk 1984, Inkelas 1990). In this section I hope to show that PDbP can cover all of the territory that was uneasily divided between Lexical Phonology and Prosodic Phonology.

5.4.1 Prosodic Hierarchy Theory

I will not seek to give a comprehensive primer in Prosodic Phonology/prosodic hierarchy theory here; I direct the reader to Inkelas & Zec (1995), on which I base the brief introductory discussion below, for an overview. Since Selkirk (1978), and in classic works on prosodic hierarchy theory such as Selkirk (1984) and Nespor & Vogel (1986), a hierarchy of phonological constituents has been identified. The most standard of these are (from smallest to largest, or weakest to strongest) the phono-

logical word (ω), the phonological phrase (ϕ), the intonational phrase (I-phrase), and the utterance (U).

It is commonly (though not exceptionlessly) thought that this hierarchy of constituents obeys the conditions in (223)-(224) (Selkirk 1984, Nespor & Vogel 1986):

(223) STRICT LAYERING HYPOTHESIS

A given nonterminal unit of the prosodic hierarchy, X^P , is composed of one or more units of the immediately lower category, X^{P-1} .

(224) PROPER CONTAINMENT

A boundary at a particular level of the prosodic hierarchy implies all weaker boundaries.

That is, the prosodic hierarchy is non-recursive (though see Dobashi (2003) and Truckenbrodt (1995, et seq.) for arguments to the contrary), and no levels can be ‘skipped.’

The evidence for prosodic constituents falls into three major classes: (a) phonological rules for which they serve as domains of application, (b) phonological processes which occur at their edges (primarily suprasegmental, e.g., boundary tones), and (c) restrictions on syntactic elements relative to their edges (e.g., second position clitics). This is reminiscent of Selkirk’s (1980) categories of domain span rules and domain limit rules; domain span rules correlate with (a), while domain limit rules correlate with (b). In this chapter we will discuss examples of each of these types, plus the type in (c).

The fundamental hypothesis of prosodic hierarchy theory is that the constituents suggested by these converging lines of evidence correlate with, but are not isomorphic to, syntactic constituents. For this reason, it is (proponents of the prosodic hierarchy

claim) necessary to erect and adjust boundaries in the phonology, on the basis of syntactic information. Two general schools of thought have emerged on how this construction is undertaken: the relation-based mapping approach represented by Nespor & Vogel (1986), and the edge- or end-based mapping approach represented by Selkirk (1986) and, in Optimality-Theoretic terms, Truckenbrodt (1995, 1999). I briefly summarize below how ϕ is constructed in each of these theories.

(225) Relation-based ϕ -construction (Nespor & Vogel 1986:168ff)

a. ϕ domain

The domain of ϕ consists of a C [clitic group] which contains a lexical head (X) and all C s on its nonrecursive side up to the C that contains another head outside of the maximal projection of X.

b. ϕ construction

Join into an n-ary branching ϕ all C s included in a string delimited by the definition of the domain of ϕ .

c. ϕ restructuring (optional)

A nonbranching ϕ which is the first complement of X on its recursive side is joined into the ϕ that contains X.

(226) End-based ϕ -construction (Truckenbrodt 1995:223)

A language ranks the two following universal constraints:

a. ALIGN-XP, R: ALIGN (XP, R; ϕ , R)

For each XP there is a ϕ such that the right edge of XP coincides with the right edge of ϕ .

b. ALIGN-XP, L: ALIGN (XP, L; ϕ , L)

For each XP there is a ϕ such that the left edge of XP coincides with the left edge of ϕ .

Dobashi (2003) shows how the theories in (225) and (226) make different predictions with regards to the syntactic structure in (227):

(227) $[_{IP} NP_{Subj} Infl [_{VP} V NP_{Obj}]]$

The relation-based model in (225) will construct (228a), and if the optional restructuring rule applies, (228b). The end-based model in (226), if ALIGN-XP, R outranks ALIGN-XP, L, will construct only (228b).²⁶

(228) ϕ boundaries for (227):

- a. $(NP_{Subj})_{\phi} (Infl V)_{\phi} (NP_{Obj})_{\phi}$
- b. $(NP_{Subj})_{\phi} (Infl V NP_{Obj})_{\phi}$

The two prosodic hierarchy models therefore agree on one thing, namely that the subject must always be phrased separately, which a great deal of literature on prosodic phrasing in SVO languages has shown is generally true (see Dobashi 2003, Ch. 2). However, they differ as to whether it is possible to phrase the object alone as well. The fact that prosodic hierarchy theory (in whatever its guise) predicts such a restricted set of prosodic constituents (“domain clustering”) is often cited as an advantage. Inkelas & Zec (1995:548) write, “in making these predictions, the Prosodic Hierarchy Theory

²⁶An OT implementation of end-based ϕ construction, the Strict Layer Hypothesis, and Proper Containment requires many more constraints than just the ALIGN family, such as WRAP-XP, NON-RECURSIVITY, EXHAUSTIVITY, LAYEREDNESS, & HEADEDNESS. See Truckenbrodt (2007) for an overview.

distinguishes itself dramatically from so-called direct access theories... in which each individual phonological rule may specify its own unique syntactic conditions. There is no expectation in such theories of any convergence or mutual constraining effect among rule domains.” In the remainder of this section, I attempt to show that, within a phase-based model of grammar, direct reference is actually more constrained and more accurate in its predictions than prosodic hierarchy theory while being far more parsimonious in its assumptions.

From my perspective, though, Inkelas & Zec are missing the real difference between direct and indirect reference theories, which has nothing to do with restrictiveness. Paraphrasing Seidl (2000), both sides acknowledge that there are phonologically-relevant domains at the phrasal level. The difference is that direct reference theories state these domains in terms of syntactic primes, while indirect theories state them in terms of phonological primes. This is not a matter of mere preference: adopting indirect reference violates the modular architecture of grammar. For indirect reference theories, prosodic constituents are constructed from a syntactic representation, as should be obvious from (225)-(226). And yet, for Optimality-Theoretic approaches which use constraints like the ones in (226),

“prosodic structure is created by ALIGN and WRAP constraints *in the phonology*, i.e. the constraints at hand being interspersed with purely phonological constraints in the same constraint hierarchy. Mapping between morpho-syntax and phonology, which is what ALIGN and WRAP do, is a process that needs to be able to interpret morpho-syntactic structure — something that is impossible on modular grounds when sitting in phonology.” (Scheer In press fn 14)

In short, Scheer makes the point that if we want to maintain that phonological representations do not include syntactic information, then the indirect mapping approach

is not viable (see also Scheer (2008b), especially §7.4).

Many other arguments against the prosodic hierarchy exist, particularly in light of Bare Phrase Structure (Chomsky 1995a), in which it is impossible to refer to syntactic projections (i.e., XP), as both relation- and edge-based approaches must (see Dobashi 2003:10ff). I will not attempt to recap these arguments here. The analyses presented in Seidl (2000, 2001) and the conceptual arguments in Scheer (2008b) are to my mind particularly devastating for prosodic hierarchy theory, and I encourage the reader to consult these works. I will limit myself to one very simple argument here: as I have already mentioned, the reason why indirect reference theories exist in the first place is that there are allegedly mismatches (incongruencies) between syntactic structure and phonological domains. One famous mismatch, already noted in *SPE*, is shown in (229). Brackets represent clause boundaries and parentheses represent I-phrase boundaries.

- (229) Syntax: This is [the cat that caught [the rat that stole [the cheese]]]
 Phonology: (This is the cat) (that caught the rat) (that stole the cheese)

However, syntactic theory has changed a great deal since these observations were made, and a phase-based approach to syntax fares much better when it comes to approximating both syntactic and phonological phenomena. In fact, I believe it fares so well that there are no longer mismatches, and the argument for indirect reference therefore disappears. For example, one diagnostic for the purported ‘phonological’ parse is the stresses on *cat*, *rat*, and *cheese*. But we have already seen in our discussion of Kahnemuyipour (2004) that these stresses derive from a rule which accents to the

highest element in each clause-level spell-out domain.²⁷ Scheer (2008b) notes the fact that mismatches of this type disappear when the relevant phenomena are described in terms of boundaries instead of domains—but I argue in this chapter that they also disappear when we use the right domains.

5.4.2 Direct reference & phase domains

Ultimately, we can only have a truly direct reference theory (and thereby adhere to the strong PDbP thesis) if a spell-out domain corresponds *always* and *exactly* to ϕ , and furthermore if ϕ is the unique level of phonological domain necessary above the word level. A quote from the final chapter of Dobashi (2003) summarizes this goal nicely:

“[I]f the mapping to Φ occurs as the syntactic derivation goes on, and if phonological rules apply as the mapping takes place, it would be unnecessary to create a p-phrase [= ϕ] in Φ . That is, the phonological rules apply when a phonological string is mapped to Φ , and such a phonological string becomes inaccessible when another phonological string is mapped to Φ later in the derivation. If so, the p-phrase is unnecessary. That is, the apparent p-phrase phenomena are reduced to the derivational properties of syntax and the cyclic mapping to Φ .” (Dobashi 2003:223)

The purpose of this chapter is to give proof of concept for a theory that, in pursuing this goal, eliminates the recourse to projections/labels, as dictated by Bare Phrase Structure. This pared-down syntax/phonology interface should be the null hypothesis given that Spell-Out is precisely the operation that connects syntax and phonology. Also aligned with this goal of phase-based direct reference are Sato (2006,

²⁷See also Newell (2008) for a different phase-based approach to this mismatch, treating the *that*-clauses as adjuncts.

2008) and Pak (2008), though my theory differs from theirs in several respects, as I will discuss in §5.4.3.

Let us first consider what prediction a phase-based system makes about ϕ -construction, compared to prosodic hierarchy theory. Dobashi (2003) shows that there are essentially four types of attested SVO languages:

(230) Typology of ϕ -domains in SVO languages (Dobashi 2003:38)

- a. $(S)_\phi (V)_\phi (O)_\phi$
- b. $(S)_\phi (V)_\phi (O)_\phi$ or
 $(S)_\phi (V O)_\phi$ if O is non-branching
- c. $(S)_\phi (V O)_\phi$
- d. $(S)_\phi (V O)_\phi$ or
 $(S V)_\phi$ if S is non-branching

Conspicuously missing from this typology is $(S V)_\phi (O)_\phi$ phrasing. The type of language in (230a) is exemplified by French, as shown in (231), and the Anlo dialect of Ewe. Italian falls under (230b); see (232). Kimatuumbi (233) represents (230c), and Kinyambo (234) is of the type in (230d).²⁸ All the examples below are taken from Dobashi (2003), §2.2; I indicate the phonological phrasing with brackets.

²⁸Dobashi takes only v and C to be phase heads. He notes that if the verb raises to v , as is commonly assumed, this will result in the subject and verb phrased together, and the object in its own ϕ domain. This prediction differs from that of prosodic hierarchy theory (recall (227)), and is undesirable from a typological perspective. Dobashi's answer to the mismatch between the *prima facie* predictions of the phase-based model and the typology in (230) is to modify the spell-out procedure. He argues that the leftmost element in a spell-out domain is actually not spelled out with the rest of the phase, but instead hangs back to establish the ordering of its phase with respect

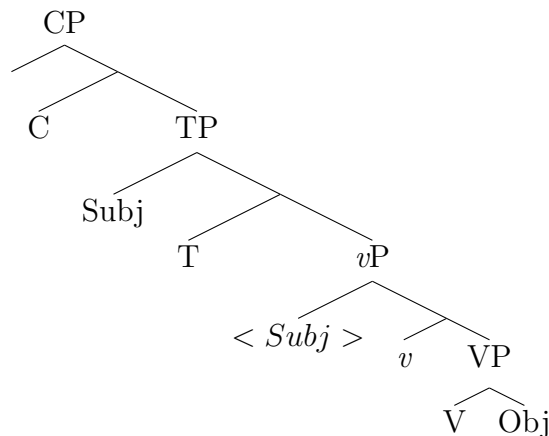
- (231) [L' immigré] [envoyait] [un paquet] [à sa famille] (French)
 the immigrant sent a package to his family
 'The immigrant sent a package to his family'
- (232) a. [Venderá] [questo leopardo] [in dicembre] (Italian)
 sell.FUT.3SG this leopard in December
 'He will sell this leopard in December'
- b. [prenderá] [tordi] or [prenderá tordi]
 catch.FUT.3SG thrushes
 'He will catch thrushes'
- (233) a. [Mamboondó] [aawúile] (Kimatuumbi)
 Mamboondo die.PST.3SG
 'Mamboondo died'
- b. [naamwéeni nchéngowe Maliíya]
 see.PST.1SG husband Mary's
 'I saw Mary's husband'
- (234) a. [abakozi báka júna] (Kinyambo)
 workers help.PST.3PL
 'The workers helped'

to the next phase; only then is it transferred. This has the effect of delaying spell-out of V (the leftmost element in *v*'s complement domain) and the subject (the leftmost element in C's complement domain), resulting in the desired separate ϕ domains for the subject, verb, and object. This captures (230a) and part of (230b). Languages like (230c,d) are claimed to have V-to-T movement and object raising to Spec,*v*P such that nothing remains in the lowest spell-out domain; for this reason, the verb and object are phrased together. Only the second options in (230b,d) still elude the theory. To account for these, Dobashi proposes a rule of restructuring which combines two ϕ -domains if one of them fails to meet the requirement that each ϕ minimally contains two prosodic words. Languages that exhibit the alternations in (230b,d) allow restructuring, while (230a,c) do not. The model proposed here improves upon Dobashi's because it eliminates restructuring and leaves the spell-out mechanism unchanged.

- b. [abakozi bakúru] [bákajúna]
 workers mature help.PST.3PL
 ‘The mature workers helped’
- c. [okubon’ ómuntu]
 see person
 ‘To see the person’

Now we’ll see how to generate this typology in PDbP. We start with the basic assumption, as introduced earlier in this chapter, that a phase head’s complement domain (minus what has already been spelled out) is transferred to the phonology as a unit, which for clause-level phase heads corresponds to ϕ in prosodic hierarchy theory. Rather than assuming along with Dobashi and Sato that D is not a phase head (or, along with Pak, that it does not necessarily trigger Spell-Out), let us instead say that D *is* a phase head and always triggers Spell-Out when it is present, but it need not be present in every language or for every argument (see §5.4.3).²⁹ Take an SVO sentence like the one below:

(235)



The phase heads are C, *v*, and D (if present). Say the verb raises to *v*, or to T. In either case, the object will be phrased alone, by virtue of being the only element

²⁹That D is a phase head has already been important to the analysis of English stress in §5.3.2, and will also figure prominently in our discussions of other phrase-level phenomena.

remaining in the complement domain of the phase head v . The subject will typically not be phrased together with the verb, even though no phase head (on the main derivational spine) intervenes between them: if the subject branches, it is a complex specifier, which is spelled out on its own (Uriagereka 1999); it will also be phrased alone if it is a full DP. This allows for the possibility of phrasing a non-branching subject with the verb, provided it is not a full DP (or even if it is, and the N moves into the edge of DP), as is the case in Kinyambo. Following Dobashi, I argue that $(V O)_\phi$ phrasing in Kimatuumbi and Kinyambo results from object raising, with the same provisos. (The difference between these two types of languages and the applicative structures involved will be discussed in more detail in later sections.) It is also possible to capture the $(S)_\phi(V O)_\phi$ phrasing that occurs with non-branching objects in Italian if the verb raises to T and the object also raises; this will again reduce the variation to the NP/DP structure.

If this theory is correct, it makes explaining domain span rules very simple: they are simply post-lexical rules that apply to the the complement domains of clause-level phase heads. By tying spell-out domains directly to prosodic phrasing, we also derive the Maximal ϕ Condition of Richards (2004, 2006b):

(236) MAXIMAL ϕ CONDITION

A prosodic phrase ϕ ($\dots\omega$, etc.) can be no larger than a phase.

The same works for domain limit rules, or phenomena that take place at the edges of ϕ -domains. As we have just established, the domains in question are the complement domains of clausal phase heads. There are two ways, then, that the edges of these domains can be marked. A post-lexical rule on the phase head could

do the job: this way it is possible to mark either edge, since a post-lexical rule sees the entire domain as a single string without any internal boundaries. The other alternative is to appeal to a lexical rule on the phase head. Because lexical rules are responsible to the Phase Impenetrability Condition at the morpheme level, this will only work for rules that mark the left (least-embedded) edge of the complement; the right edge of what is accessible to a lexical rule (i.e., the morpheme-level domain) will typically not extend all the way to the right edge of the clause-level domain. We would also expect that these two types of domain limit rules could be distinguished on the basis of whether or not they exhibit the properties characteristic of post-lexical rules: exceptionlessness with respect to lexical items, optionality, and so on (recall §5.2.3).

It would be a massive undertaking to show that phase domains suffice for every rule with a claimed domain of ϕ , but I attempt to support this hypothesis later in this chapter by using vowel assimilation in Lekeitio Basque and obstruent voicing in Korean as case studies, giving detailed syntactic analyses of the environments in which each apply. I demonstrate that the Phase Impenetrability Condition predicts the application and blocking contexts of both processes, both above and below the word level, in exactly the way I have just described.

5.4.3 Differences from other phase-based theories

PDbP shares in many of the successful predictions made by other theories which take the spell-out domain to delimit phonological rule application. However, it differs from previous models in several crucial respects. Perhaps the closest relative of PDbP

is the theory proposed by Pak (2008). As Pak notes, other phase-based theories suffer from a number of shortcomings. Some concentrate only on sub-word-level phenomena (e.g., Marvin (2002)) while others posit modifications to syntactic domains at the interface or in the phonology that prevent them from abandoning indirect reference (Seidl (2000, 2001), Dobashi (2003), Ishihara (2007), Kratzer & Selkirk (2007)). Many also fail to account for phonological rules that apply to smaller domains than (clause-level) phases or domains that are not syntactic constituents (Dobashi (2003), Sato (2006, 2008)); see discussion of these and more in Pak (2008), Chapter 2.

Pak argues that there are three types of phonological rules: those which apply to two consecutive morphological words as they are linearized ('concatenation rules'), those which apply when these pairs of words are themselves linearized into a string that is precisely one clause-level spell-out domain long ('chaining rules'), and those which apply when these spell-out domains are linearized with respect to one another ('late-linearization rules'). Of these, only the third are sensitive to speech rate and register information, which can cause the merging or splitting of chains and therefore affect the domains of rule application.

Following Embick & Noyer (2001 et seq.), Pak posits a number of different stages of linearization/concatenation at the syntax/phonology interface: one that linearizes morphemes within a word, a subsequent one that linearizes two adjacent words (never crossing spell-out domain boundaries), a third that linearizes these pairs of words into a single chain (again, within spell-out domain boundaries), and a fourth that linearizes these chains. If we accept that *v* triggers transfer, then we have already seen cases from English in §5.3.1 which show this restriction to be false: T and a

verbal root interact, across *v*. Also, the ‘Concatenation statements’ which state the linearized pairs of words, and over which concatenation rules are claimed to apply, inherently overlap. For example, in her discussion of Luganda, for the phrase *ebi-kopo bya-mu-gaanda wa-Walusimbi* ‘Walusimbi’s brother’s cups,’ Pak (2008:210ff) gives the following Concatenation statements, represented here as ordered pairs: (ebikopo, bya), (bya, muganda), (muganda, wa), and (wa, Walusimbi). If concatenation rules apply to these pairs separately, it is unclear what to do about the dual representations of *bya*, *muganda*, and *wa*, which will presumably undergo different operations since they are in different environments. Enforcing the Phase Impenetrability Condition at the morpheme level as items are concatenated into a single string, eliminating this pairwise step, produces a similar rule-application domain locality effect (with the differences mentioned above), but without this representational problem.

The concatenation rules themselves in Pak’s system are also similar to, yet differ in critical ways from, lexical rules in the theory presented here. Pak claims that concatenation rules apply only to two consecutive words within a single (clause-level) spell-out domain, never across words in different domains. As I mentioned above, if *v* triggers Spell-Out, this linearization scheme would incorrectly rule out T-*v*- $\sqrt{\text{ }}$ interactions and other attested phenomena. This means that, contra Pak, a lexical rule can apply across a clause-level spell-out domain boundary which is impenetrable to post-lexical rules; this occurs when a lexical rule triggered by an element in the edge of a clause-level phase applies to something in its complement. Such situations are, I argue, responsible for the overlapping domains which caused Seidl to claim that indirect reference is necessary. I address this claim in §5.4.4.

Another important advantage of the PDbP conception of lexical rules over Pak's theory of concatenation rules is that the former rely on the notion of morpheme-level phase heads, while the latter refer to morphological words. The notion that phases, and the Phase Impenetrability Condition, can explain a great deal about how lexical rules apply is one of the primary insights of Marvin (2002), and one which I think is well worth maintaining. It is precisely this which gives us the best of Lexical Phonology while avoiding many of its downsides, and which allows for a unified account of lexical rule application/blocking both within polymorphemic words and across word boundaries. Moreover, the way lexical rules are treated as being associated with particular lexical items allows us to eliminate reference to syntactic information such as morphosyntactic feature content, node labels, and c-command in phonological rules. For example, rather than formulating a rule that says "do X to a word c-commanded by [-FINITE] T," we simply have a rule on [-FINITE] T that initiates a SEARCH into the accessible domain (i.e., the previous morpheme-level spell-out domain, which it necessarily c-commands), and then a COPY and/or DELETE operation applies to the results of that SEARCH. The result is the same, but this way the operations take place in a linear space, free of hierarchical notions and free of vocabulary that is not germane to phonology.

As I have already mentioned, Pak's chaining rules correspond to what I have been calling post-lexical rules in that they apply once to a phase-sized string. For Pak, though, only C necessarily triggers Spell-Out; *v* and D may do so in some languages, but this matter is set to the side.³⁰ This move effectively severs the phonological cycle

³⁰A similar position, that *v* is not a phase head in Inuktitut, is held by Compton & Pittman

from the syntactic one, which undermines the argument from computational efficiency for tying phonological domains to syntactic ones in the first place. It is also unclear why C should enjoy such a privileged position, if the notion of *phase* (not, e.g., *proposition*) is what is important to phonology. In contrast, in PDbP, all clause-level phase heads are treated on a par. Certain phase heads may not be relevant to particular languages: D and ApplH may not be part of every language's inventory, for example. We expect that independent syntactic and/or semantic evidence will converge with phonological evidence to corroborate this. For example, Bošković (2005, et seq.) and Despić (2008) argue that a number of facts about Serbo-Croatian morphology, syntax, and semantics are explained if D is absent from this language. The absence of D should likewise be reflected in the observed phonological phrasing. To the best of my knowledge, this is correct; there is no evidence that objects are phrased separately in Serbo-Croatian, which would necessarily occur if D were present. Similar arguments will be made for high/low applicatives in Bantu in the following sections. If, like Pak, we simply stipulated on the basis of phonological evidence alone whether D triggers Spell-Out in a given language, such a correlation would not be expected.

The category of 'late-linearization rules' for which Pak argues is intriguing and worthy of future research, as are the mechanisms of chain-splitting and chain-merging which she claims yield the register- and speed-dependent variation in the application domains in this type of rule. Since there are clearly so many performance factors and

(2007). However, they note that this position is not necessary; if the observed inverse morpheme order in the language is caused by movement, then it could be that *v*'s complement domain is spelled out, but empty. For the same reasons cited in the main text, I would adopt the latter analysis for Inuktitut.

sociolinguistic variables at play here, I leave this area for future research, with the hope that more investigation into the range of possible split/merger points will shed new light on this source of variation.

5.4.4 From minimally indirect to direct reference

In the previous sections we discussed how phenomena that have typically been described as ϕ -level can be accounted for within a phase-based framework, and without the construction of any boundaries in phonology, via a combination of procedural and representational approaches. In this section, I answer the objection to direct reference raised by Seidl (2000, 2001), who argues that, while phase domains are indeed very important to phonological computations, they do not suffice to cover all the attested phenomena. Seidl argues for a “minimally indirect” interface theory, in which phonology has access to two sets of syntactically-determined domains: first, the ‘early parse’ or ‘morphosyntactic parse’ consisting of spell-out domains, and second, the ‘late parse’ or ‘phonological parse’ which may include re-bracketing of clitics with their hosts, and the projection of boundaries at the edges of θ -marking domains. Rules which refer to this late parse would seem to preclude the possibility of a truly direct interface. For reasons of space I cannot describe and re-analyze all the data covered by Seidl here, but I will try to give evidence that the PDbP system of allowing lexical rules to span two morpheme-level domains but confining post-lexical rules to one clause-level domain can accommodate both ‘early-parse’ and ‘late-parse’ rules.

One example Seidl gives of the early-/late-parse dichotomy comes from Oyo Yoruba, and specifically the overlapping domains of high-tone deletion and ATR harmony in

this language. The data presented in (237) is reported in Akinlabi & Liberman (2000) and (238) is from Akinbiyi Akinlabi (p.c.); both are reported by Seidl (2000:67ff). I give a few simple cases of the tone deletion process below: underlying high tone on an object clitic deletes when it follows a verb that also carries a high tone. Thus, the high tone appears on the clitics *mí* ‘me’ and *wá* ‘us’ in (237a,b) since they follow a verb with low tone (*kò* ‘divorced’), but the high tone deletes on the clitics in (237c,d), following a verb with high tone (*kó* ‘taught’).

- (237) a. *ó kò mí* → *ó kò mí*
 he divorced me
- b. *ó kò wá* → *ó kò wá*
 he divorced us
- c. *ó kó mí* → *ó kó mi*
 he taught me
- d. *ó kó wá* → *ó kó wa*
 he taught us

Note that the adjacent high tones on the subject and verb in the preceding examples are not repaired. However, when we add ATR harmony to the picture, the opposite is true: the subject clitic harmonizes with the verb, but the object clitic is unaffected.

- (238) a. *ó kó wá* → *ó kó wa*
 he taught us
- b. *ó lé wá* → *ó lé wa*
 he chased us

Because of this domain overlap, Seidl argues that the tone deletion rule applies on the early phase-based parse within the spell-out domain of VP, while harmony

applies to the late parse, after the subject has cliticized to the verb, and the object is parsed in its own domain.

I agree with Seidl that high-tone deletion takes place within the spell-out domain to which the verb and object both belong; in my account it is a post-lexical rule. However, there is no reason that harmony needs to be computed over a different parse. I propose instead that subject clitics are harmonic (underspecified for [ATR]), while object clitics are not. A simple SEARCH and COPY operation initiated by the subject pronoun (i.e., a lexical rule) will take care of the harmony. The fact that high-tone deletion is exceptionless but harmony has lexical exceptions—for instance, only singular subject clitics participate (Seidl 2000:71fn28)—lends support to my view. Seidl instead takes the position that late-parse rules tend to have exceptions, in contrast to early rules, but this does not follow from any independent principle of her theory.³¹

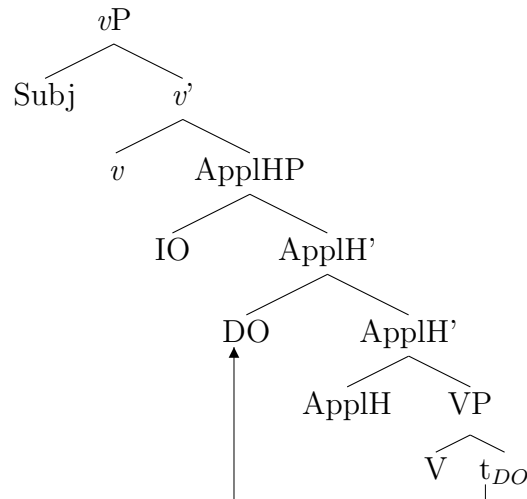
Many of the rules that Seidl argues are sensitive to θ -marking domain boundaries come from Bantu languages. She details the syntactic differences between ‘symmetric’ Bantu languages such as Kinande and ‘asymmetric’ ones such as Chicheŵa, then argues that the differences in the boundaries projected at the edges of the θ -marking domains in these two types of languages yield different domains for rules such as tone

³¹Seidl claims that whether or not θ -marking domain boundaries are projected is dependent on speech rate/register, which can account for optionality in the application of late rules. However, this cannot account for the type of systematic pattern that Oyo Yoruba vowel harmony exhibits, with the plural subject pronouns never participating. Furthermore, as we saw earlier in this chapter, *post-lexical* rules—corresponding to Seidl’s *early* rules—are the ones which are known to exhibit sensitivity to extra-grammatical variables.

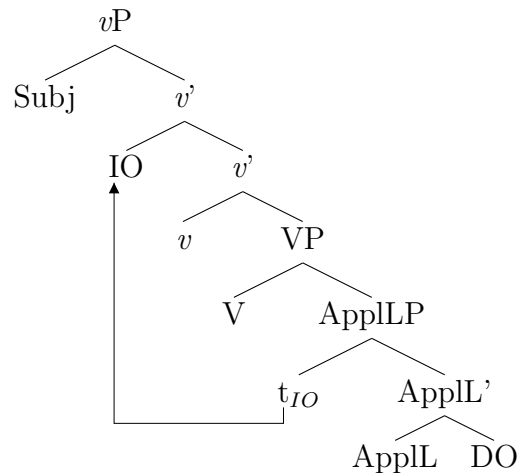
sandhi. Symmetric languages exhibit a single domain with the indirect object (IO) and direct object (DO) phrased together, while asymmetric ones show two domains, (IO)(DO). Seidl says this results from the DO raising to the specifier of ApplP in symmetric languages but remaining *in situ* in asymmetric ones; in either case, the edge of the θ -marking domain, VP, projects a phonological phrase boundary, but if the DO has raised, there is nothing left in that domain so everything gets phrased together.

McGinnis (2001) shows that reference to θ -marking domains is unnecessary; the phrasing facts immediately follow from spell-out domains when we appreciate that the difference between symmetric languages and asymmetric ones, following Pylkkänen (2002), is that the former have a high applicative (ApplHP) between *v*P and VP, while the latter have a low applicative (ApplLP) within VP. Furthermore, the high applicative head is a phase head, while the low applicative head is not (this will be important when we analyze Basque, but is not crucial here). For McGinnis, then, both types of languages exhibit movement to check a phase-EPP feature: the DO raises to the edge of the ApplHP phase in a symmetric language while the IO remains *in situ*, and the IO moves to the edge of the *v*P phase in an asymmetric language while the DO remains *in situ*. This is shown below in (239)-(240), modified slightly from McGinnis (2001:27).

(239) High applicative (symmetric)



(240) Low applicative (asymmetric)



The symmetric case in (239) will have both objects in a single spell-out domain: nothing remains in ApplH' 's complement domain, but both objects sit in ApplHP' 's edge and will be spelled out together as the complement of v . In the asymmetric case, (240), the DO is in v' 's complement but the IO is in its edge, so they will be spelled out separately.

Seidl makes a further assumption that the subject and verb move out of vP in both types of language, with the subject landing in Spec,TP and the verb also moving

into the TP domain. This makes a good prediction for asymmetric languages—the subject, verb, and IO will all be spelled out together with the DO separate, and it generates the correct S V IO DO word order. For symmetric languages, though, neither leaving the subject and verb in *vP* or raising them predicts the single (S V IO DO) domain which Seidl claims is necessary. Whether or not the subject and verb remain in *vP*, we still get (S V)(IO DO). I will illustrate with Seidl’s example from Kinande that, by adding one additional parameter to the way post-lexical rules apply in PDbP, it is possible to reconcile a two-domain analysis with this type of data.

Kinande shows lengthening of the penultimate vowel in a domain. Lengthening does not affect the verb if it has an object after it, nor does it affect the first object in a ditransitive. The high tone found on the penult is also absent when an object follows.³²

(241) Kinande penultimate vowel lengthening (Hyman & Valinande 1985)

- a. er-rí-tú:m-a (→ *er-rí-túm-a)
IV-INF-send-FV
- b. er-rí-tum-a valiná:nde (→ *er-rí-tú:m-a valiná:nde)
IV-INF-send-FV Valinande
‘to send Valinande’
- c. tu-ká-βi-túm-ir-a omúkali valiná:nde (→ *omúka:li valiná:nde)
we-PST-T-send-BENE-FV woman Valinande
‘We have just sent Valinande to the woman’

In short, in order to capture the distribution of these two processes we need a

³²In the following Kinande examples, IV and FV stand for Initial Vowel (augment) and Final Vowel, respectively.

couple of post-lexical rules³³ that apply to the last element in the complement of v , or if the complement is empty (i.e., all material to be pronounced has evacuated the domain), to the last element in the edge of vP . To account for this behavior, I propose a modification to the application of post-lexical rules which I call the Empty Complement Condition:

(242) EMPTY COMPLEMENT CONDITION

If the complement of a phase head α is empty, a post-lexical rule on α may apply to the edge of αP .

Kahnemuyipour (2004:125ff) proposes a similar condition to account for unexpected stress on sentence-final prepositional phrases (both adjunct and argument) in English.

- (243) a. John saw Mary in the park.
 b. John put the milk in the fridge.
 c. John gave the ball to Bill.

He suggests that the base order for such sentences is as follows:

- (244) [_{TP} Subj [_{vP} V DO [_{VP} t_V [_{PP} IO]]]]

The PP evacuates the vP , and then what remains of the vP moves around the indirect object. Thus nothing is left in the lowest spell-out domain (the complement domain of v). In such a case, stress is assigned to the closest element in the next higher domain.

³³See Hyman & Valinande (1985), Hyman (1990), Mutaka (1994) on the independence, and post-lexical nature, of these processes.

The Empty Complement Condition accounts for the phenomenon noticed by Kahnemuyipour, but as our discussion of Kinande above shows, it is not limited to stress assignment. The effects of the Empty Complement Condition are most apparent for *v*, because of ellipsis, intransitives, and object raising; however, we nevertheless predict that if the circumstances are right, the Empty Complement Condition will manifest itself in other domains. For instance, the complement of C is empty when there is clausal pied-piping of the type we will see in §5.6.1 for Basque. We might also expect to see Empty Complement Condition effects when D's complement is empty, either because of NP-ellipsis or with pronouns, assuming they are in D. However, to account for the fact that some rules may apply *only* if there is an overt complement (and never to the edge), I suggest that the Empty Complement Condition (ECC) is parameterized, so rules must be marked as $[\pm\text{ECC}]$. We might think of this option as switching the δ parameter on SEARCH in the case where there is nothing to search through in the originally-specified direction. If the phrasal high tone and penultimate vowel lengthening rules in Kinande are both $[\text{+ECC}]$, then there is nothing to contradict the (S V)(IO DO) phrasing predicted by PDbP as applied to McGinnis' analysis of symmetric languages.

5.5 Null syntactic elements

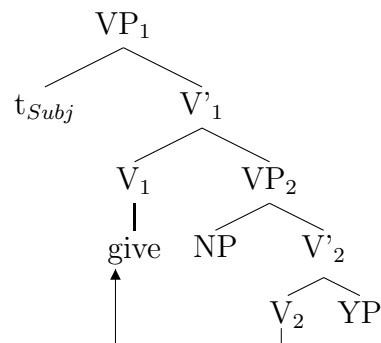
In a theory of phonology that is as closely tied to syntax as PDbP, it is important to consider the roles that various null syntactic elements play—or do not play—in the phonology. We have seen throughout this chapter that null phase heads (both morpheme-level and clause-level) are very important to the workings of PDbP. Even

though they do not have segmental content, they still trigger phonological rules of both lexical and post-lexical types. We have also acted as though phonology cannot see traces/deleted copies. The relevance of these elements to phonological processes has been investigated by Truckenbrodt (1999), Seidl (2000, 2001), and An (2007a,b), with varying conclusions. We will discuss each of these in turn. However, as I mentioned in our discussion of T-root interactions, much work in this area remains to be done and neither I nor anyone else can currently claim to have a comprehensive theory of all phonologically null elements.

5.5.1 Truckenbrodt (1999)

Truckenbrodt (1999) proposes the Lexical Category Condition, which exempts null syntactic elements and their projections (and also functional elements and their projections) from constraints pertaining to prosodic phrase-building. He argues that this is necessary to prevent VP_2 from enforcing mapping constraints in a ditransitive structure like the one below:

(245)



In §5.4.4 we already discussed a more sophisticated syntax for ditransitives, but this is orthogonal to the issue at hand. Truckenbrodt's argument on the basis of Chicheŵa for (the relevant clause of) the Lexical Category Condition goes like this:

in Chicheŵa, putting the subject aside, there is typically one phonological domain: (V DO IO). But when there is focus within the VP (i.e., VP_I), the IO is phrased separately, and sometimes the DO is phrased separately, too. The relationship between focus and phonological domains in Chicheŵa is stated by Kanerva (1990:157): “if there is focus inside the VP, (a) a domain starts at the verb and ends at the focused constituent; (b) any following nonfocus constituents each form their own domain. (c) Otherwise, the VP forms a single domain.”

For Truckenbrodt, this means that WRAP-XP, which penalizes an XP that is not contained in a phonological phrase, must not be able to see VP_2 . Otherwise, the extra violation of WRAP-XP incurred by splitting up the two objects (i.e., the specifier and complement of VP_2) would prevent the correct candidates from winning and keep the objects phrased together no matter what the focus structure of the clause.

A closer examination of Bantu syntax reveals that, while the phonological phrasing patterns may be correctly described (modulo the exclusion of the verb, as we have already discussed for Kinande), the syntactic analysis that Kanerva and Truckenbrodt assume is incorrect. It has been shown that in general, Bantu languages observe a constraint such that when one argument is focused, no others can remain inside VP (in our terms, vP); they must be right-dislocated (see Buell (To appear, 2008), Manfredi (2007), and references therein). Buell (To appear) calls this constraint Focus-Induced Extrapolation:

(246) FOCUS-INDUCED EXTRAPOSITION

When a focused element appears in the verb phrase, no other elements appear in the verb phrase.

This immediately accounts for why both objects constitute a single domain in a neutral sentence of Chicheŵa, but as soon as one is focused, the rest are phrased separately. It remains to be explained why Chicheŵa apparently allows a non-focused DO to remain within the *v*P when the IO is focused. This is not unprecedented—Buell discusses cases in which it is possible for some material to intervene between a verb and a focused object in Zulu, particularly when the focused argument is a locative, with good to marginal acceptability status. At this stage, though, the open question is how to restate (246) to accommodate the attested exceptions. What is relevant for our discussion of the Lexical Category Condition is that the variety of prosodic phrasing found in Chicheŵa should not be analyzed as variation in how ϕ -phrases are constructed for a single syntactic configuration, but rather, the differences in phrasing reflect differences in syntax. More to the point, it is not the case that Truckenbrodt's VP_2 must be split up in a focus construction, so whether WRAP-XP sees VP_2 is irrelevant. This nullifies the argument that null-headed projections are treated specially for phonological purposes.

5.5.2 Seidl (2000, 2001)

Seidl (2000, 2001) suggests that traces of movement³⁴ can disrupt phonological rule application, but that null heads (i.e., null C) do not. We should note that neither trace-sensitivity nor null-head-insensitivity are problematic for PDbP *per se*. It is just that we do not expect that this particular distinction should be appropriate. PDbP predicts that a trace in the position of a clause-level phase head like *v* will in effect

³⁴Read: unpronounced copies.

be a boundary for post-lexical rules, not because there is a trace there, but rather because transfer is triggered by virtue of the phase head being present *in the syntax*, regardless of whether any phonetic content is actually realized in that location. That is to say, null C and a trace in *v* enjoy the same status in PDbP, and can produce blocking effects; traces in non-phase-head positions, such as T, are simply irrelevant to phonology because non-phase-heads cannot trigger post-lexical rules, and lexical rules are “carried with” the lexical items to which they belong (that is, a lexical rule applies at the location in which the associated lexical item is pronounced).

How, then, do we account for what seems to be an asymmetry in Kimatuumbi between a trace and a null head? Seidl’s evidence for this comes from a rule of phrasal tone insertion in Kimatuumbi, which she describes as inserting a high tone between a specifier or adjunct and whatever comes after it within a maximal projection, provided those two elements are strictly adjacent. That is, if there are multiple specifiers/adjuncts, high tone insertion applies between all of them, and if one is directly adjacent to the head, it will also apply between those two elements. This is illustrated in the examples below from Odden (1990), with the inserted tones in bold face.

- (247) a. **iíjuma** kiyógoyó Mamboondó naampéi lí
 Friday kiyogoyo Mamboondo I-him-gave NEG
 ‘I did not give Mamboondo a kiyogoyo on Friday.’
- b. maná naantumbilé Mamboondó nduwae kuunwáaya
 if I-him-fell Mamboondo I-would him-nurse
 ‘If I had fallen on Mamboondo, I would have nursed him.’
- c. naampéi lí Mamboondo kiwikilyo iíjuma
 I-him-gave NEG Mamboondo cover Friday
 ‘I didn’t give Mamboondo a cover on Friday.’

In (247a), the three nouns (Friday, kiyogoyo, Mamboondo) are left-dislocated,

and Seidl considers them to be IP-adjuncts. High-tone insertion applies between the adjuncts, and between the final one and the verb. In (247b), high-tone insertion applies between the object of the first clause, Mamboondo, and the modal in the second clause; Seidl claims that this shows that a null complementizer in the second clause does not prevent the strict adjacency necessary for the tone insertion rule to apply. In (247c), however, no tone insertion occurs between the IO and DO, which Seidl argues is because of a trace getting in the way of strict adjacency. Further application contexts are given by Odden (1990). I summarize these below.³⁵

- (248) a. On a preverbal subject
 Mamboond**ó** aawíle
 Mamboondo died
- b. On a subject separated from the verb by a complementizer
 Mamboond**ó** keénda akáteléka
 Mamboondo if cooking
 ‘If Mamboondo is cooking’
- c. On a subject in a null-copula construction
 Mamboond**ó** nnaáso
 Mamboondo tall
 ‘Mamboondo is tall’
- d. On the the last word in a preposed relative clause
 mundu ywaanáampeíi mpuung**á** waabúui
 person REL-I-him-gave rice he-left
 ‘The person I gave rice left’
- e. On the last word of the first CP or NP conjunct
 panáakalangitée ñam**á** Mamboond**ó** akalangae kindoólo
 when-I-fry meat Mamboondo he-frying-PST sweet.potato
 ‘When I was frying meat, Mamboondo was frying a sweet potato’

³⁵Odden mentions one additional context in which high tone insertion occurs and which I omit from the list below, namely when a VP is followed by the adverb *píta* ‘really’ or *kwaáli* ‘perhaps.’ In the absence of evidence suggesting there is a broader generalization to be made about adverbials here, I will set this case aside.

Several things about this list suggest to me that Seidl's analysis should be revised. The fact that the subject can be followed by a complementizer (248b) shows that subjects can be very high, probably in TopicP, in this language; a complementizer would precede a subject in Spec,TP. The fact that subjects and preposed elements all behave alike with regard to this rule also suggests that this may be correct. I therefore propose that all subjects are topics in Kimatuumbi, as has been independently argued for Bantu (see Demuth & Mmusi (1997) and references therein on Sesotho and Bantu more generally; also Pak (2008) on Luganda). If this is the case, then (248d) should also involve topicalization of the entire subject, including the relative (see Samuels (To appear) on similar findings in Lycian and Hittite). Finally, with regards to (248e), Schein (1997) argues that conjunctions always combine propositions (i.e., CPs), and there is no true NP-conjunction. The combined effect of analyzing subjects as topics and all conjunction as involving full propositions is that the environment of phrasal high tone insertion reduces to the edge of CP.

There are several ways we might formulate a post-lexical rule that would apply in the desired environment. One way would involve admitting post-lexical rules that normally (even in the absence of an empty complement) apply into the specifier of a phase head (in this case, C).³⁶ This would be safe enough for the phenomenon in question, given that it would involve a domain-limit rule that applies to the rightmost edge of the highest clause-level phase. However, such practice could potentially lead

³⁶In order to prevent the rule from applying to *mundu* in (248d), we would have to say that the relative is not a full CP, which is not implausible; see Pak (2008) on reduced relatives in a related language, Luganda.

to a situation in which lexical rules apply *after* this type of post-lexical rule. This could happen if, for instance, a post-lexical rule on *v* applied to the *v*P-edge phase, at the end of the cycle on which C's complement entered the derivation, and then a lexical rule on C, entering on the final CP-edge phase, altered the subject in Spec,TP. It is an empirical question whether this possibility must be ruled out, and if so, how to go about this: perhaps by limiting this type of rule application to C, or by limiting it to the rightmost linear edge of the phase edge (i.e., the part that can be affected by lexical rules associated with a lower, but not higher, morpheme-level phase). Another option would be to forgo this approach entirely, and appeal to a phase head very high in the Left Periphery, above TopicP, that would allow this rule to apply in the usual direction, to its complement domain. I leave further investigation of this matter for future research.

However this issue is ultimately resolved, I believe it is at least clear that what prevents phrasal high tone insertion between the objects in (247c) is not a trace, but rather the fact this process has nothing to do with any elements lower than C.

5.5.3 An (2007)

An (2007a) argues for a theorem which he calls the Intonational Phrase Edge Generalization (IPEG).³⁷ This is stated below:

³⁷Or similarly in An (2007b), the Ban on Null Edge (BONE). The BONE is at once more general than the IPEG—banning an empty left *or* right I-phrase edge—and more specific, applying only to null vocabulary items and not traces/unpronounced copies. However, since the right edge of an I-phrase is usually only emptied by movement, it is effectively exempt from the BONE as well as the IPEG. For this reason it is unclear whether the BONE provides better empirical coverage than

(249) INTONATIONAL PHRASE EDGE GENERALIZATION

The edge of an I-phrase cannot be empty

(where the notion of edge encompasses the specifier and the head of the relevant syntactic constituent)

This accounts for why the sentence in (250a), with an empty edge in the embedded CP, is unacceptable. It also explains why such a sentence can be ameliorated by overt content in Spec,CP (250b) or C (250c), without running into the problems of previous accounts (for extensive discussion of the various competing proposals, I refer the reader to An's work).

- (250) a. ?? I saw the child yesterday [\emptyset_{spec} \emptyset_C Mary was waiting for]
 b. I saw the child yesterday [who \emptyset_C Mary was waiting for]
 c. I saw the train yesterday [\emptyset_{spec} that Mary was waiting for]

The generalization expressed by the IPEG seems robust, and makes sense from a PDbP perspective. I have already argued that traces/unpronounced copies are invisible to phonological processes, but null elements (and null phase heads in particular) are crucial to phonology. However, since PDbP rejects the prosodic hierarchy, we must come up with a different way of describing the environment to which the IPEG applies, one that does not make reference to the I-phrase. A footnote in An

the IPEG, or whether the trace/null inserted item distinction is necessary. This issue hinges the syntactic analysis of sentences like (250), specifically whether they contain a trace or operator in the offending Spec,CP, or whether there is no specifier projected at all. I leave this question open; if right edges turn out to be irrelevant, then (251b) is, too. But the contrast between (250a) and (251a) will remain in need of explanation.

(2007a:70), not pursued in the paper, suggests the direction we will take, namely to connect I-phrasing to spell-out domains. Support for this view comes from the list of obligatory I-phrase contexts taken to hold cross-linguistically. For instance, An (2007b:31) claims that “if a CP appears in extraposed position, subject position, topic position, complement position of a noun, and the target position of [Right Node Raising] and Gapping, it must be parsed as a separate I-phrase.” He provides evidence that the analysis should be extended to *v*P as well, lending further credence to the view to be presented here.

This distribution, and the involvement of *v*, is completely expected under a phase-based analysis. In any of these circumstances, because the clause has moved, it will be spelled out on its own—or at least, nothing from outside the phase head’s maximal projection will be transferred together with the specifier and head, so if those elements are null, the entire spell-out domain will be. As a first attempt to restate the IPEG, then, we may claim that a spell-out domain cannot be empty. But there are two important exceptions to this:

- (251) a. [_{CP} [_{TP} Mary was waiting for the child.]]
- b. [_{CP} Who did Mary [_{vP} see [_{VP} *t_v* *t_{obj}*]]]

The acceptability of these two structures shows that there is nothing wrong with having an empty spell-out domain *per se*: the highest domain is empty in (251a), and the lowest in (251b).³⁸ I take the contrast between these two examples on the one hand

³⁸An’s explanation for (251a) is that the highest projection involved is actually IP: following Chomsky (1995b), C can be inserted covertly at LF, and the highest phase is therefore *v*P’s non-empty edge. This analysis is unavailable in model like (P)DbP, in which we maintain that the

and (250a) on the other to show that something goes wrong in the derivation only when an empty spell-out domain is sandwiched between two others, as in embedding contexts. The problem may be one of linearization, or perhaps of parsing. This is an issue in need of further research.

To summarize the results of this section, we have seen that different null syntactic objects vary in their properties. Null clause-level phase heads can trigger both lexical and post-lexical rules, and the Phase Impenetrability Condition blocks any rules that could potentially apply across such null phase heads. Traces in clause-level phase head positions block but do not trigger lexical rules, whereas they both block *and* trigger post-lexical rules. Null morpheme-level phase heads both trigger and block lexical rules, but neither trigger nor block post-lexical rules. Traces of morpheme-level phase heads block lexical rules but do not trigger them, and neither trigger nor block post-lexical rules. Traces of non-phase heads, or of larger constituents, are totally invisible to phonological rules. And finally, the presence of an empty spell-out domain may cause a derivation to crash, but only in a restricted set of circumstances.

5.6 Case studies in PDbP

We have now discussed all the theoretical underpinnings of PDbP. In the remainder of this chapter, I will illustrate how the theory can be put to use. I present PDbP analyses of three processes: vowel assimilation in Lekeitio Basque, obstruent voicing

semantic and phonological cycles are synchronized. (251b) does not violate IPEG, which only requires a contentful left I-phrase edge. However, it runs afoul of the BONE, which applies to both edges. An (2007b) claims that the BONE applies after copy deletion, so the structure seen by the BONE is [Who did Mary see] with no empty edge.

in Korean, and clitic placement in Serbo-Croatian. The Basque case is an example of a lexical rule, the Korean case is a post-lexical rule, and the Serbo-Croatian case shows how spell-out domains interact with affixation and copy deletion.

The Korean and Serbo-Croatian data have been given accounts in terms of the prosodic hierarchy, while the Basque assimilation rule has been described in syntactic terms somewhat different from those presented here. I argue that PDbP gives us the machinery with which to identify the heterogeneous environments in which these processes apply, without recourse to the prosodic hierarchy.

5.6.1 Lekeitio Basque Vowel Assimilation

We will first consider vowel alternations in Lekeitio Basque, using data from Hualde & Elordieta (1992), Elordieta (1997, 1999, 2007), and Elordieta (p.c). This dialect has two vowel assimilation rules which apply in the same contexts:

(252) a. VOWEL RAISING (obligatory)

$$V_{[-HI]} \rightarrow [+HI] / -V$$

b. VOWEL ASSIMILATION (optional; fed by raising)

$$V_2 \rightarrow V_1 / V_{1-} \text{ when } V_2 \text{ is } [-RD]$$

Hualde & Elordieta (1992), focusing on the assimilation rule, note that it behaves in many respects like a lexical rule, yet it can apply across (some) word boundaries and appears sensitive to morphosyntactic information. Subsequently, Elordieta (1997 et seq.) has argued that the relevant environment for both of these rules is within a feature-checking chain: (C, T), (T, *v*), (T, D), (*v*, D), or (D, N). Of particular relevance to vowel assimilation are the T-*v* and D-N relationships. I will present an

alternative that takes the correlation with feature chains, which I show to be imperfect, as coincidental³⁹; in my analysis, these rules are both lexical, and as such, their application is restricted to two adjacent morpheme-level spell-out domains. Since lexical rules are carried on individual lexical entries, it is trivial to restrict these rules to items that are involved in feature chains, which should yield a perfect match with Elordieta's predictions. However, as we will see, there are in fact a few examples of assimilation in which there is no feature-checking involved. Moving forward, then, I will assume that the rules are carried on all lexical items, except for certain lexical exceptions, of which there must be some in anyone's analysis, and let the Phase Impenetrability Condition take care of the rest.

I will now demonstrate the environments in which assimilation (and raising, if applicable) applies, and those in which it does not. One context in which these rules apply is between a noun and an inflectional affix, which in this case signifies both definiteness and case. I give two examples below.

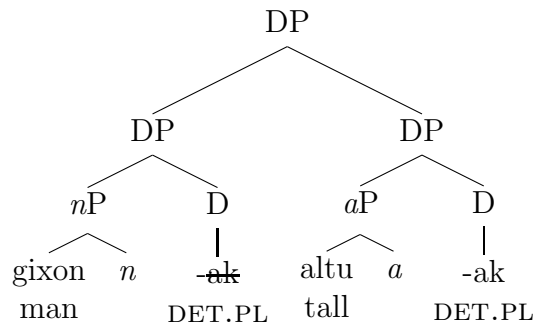
- (253) a. ume-en → umiin
 child-GEN.PL 'of the children'
 b. gixon altú-ak → gixon altúuk
 man tall-DET.PL 'the tall men'

Following Etxeberria's (2007) arguments from semantics, I posit a structure in which two Ds are present, but only one is pronounced. Such a structure accounts for multiple agreement, which is common cross-linguistically and present in some

³⁹However, given that feature-checking relationships are themselves subject to the Phase Impenetrability Condition, there is likely to be some degree of correlation. Indeed, depending on what one believes about the nature of phases, feature-checking may in fact underlie the phase cycle. But whatever the relationship between features and phases may be, note that my analysis, but not Elordieta's, can account for the cases in which two assimilating items are not part of the same feature chain.

dialects of Basque. It is also supported by the conclusions of Compton & Pittman (2007), who argue that some adjectives in Inuktitut, which act much like those in Basque, are actually DPs in apposition; I adopt their labeling here. For this and all subsequent structures in Basque and Korean, I assume head-finality. Note that in a structure like the *nP* and *aP* in the examples below, the complement and specifier are indistinguishable; the root is accessible to D. I also omit representing Spec,DP unless it is relevant to the example being discussed.

(254)



For the case at hand, the relevant property of this structure is that *altu* is accessible to *-ak*: the former is spelled out in the complement of D, and the latter in its edge, so assimilation can apply. Because the roots remain trapped in the complement domain of D, nouns are highly restricted in triggering the rules we are discussing. The following two examples demonstrate that a noun and an adjective cannot interact, as suggested by the structure posited above. This is true regardless of whether there is compounding involved; the only difference is that with a compound, there will be an additional *aP* layer on top of the DP, serving to lexicalize it.

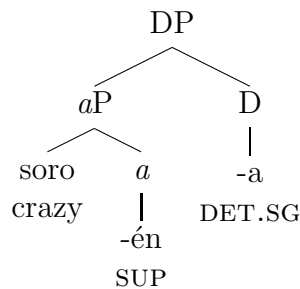
- (255) a. ortu estu-a → ortu estuu
 field narrow-DET.SG 'narrow field'
 b. buru-ándi
 head-big 'big-headed'

In these examples, the relation between the two roots is at issue, and assimilation is blocked because the roots are in the complement domains of two different determiners.

Assimilation can, however, occur between an adjective and a derivational affix. This case provides some of the best evidence that feature chains are not involved.

Note that there are very few vowel-initial derivational suffixes in Basque, and the superlative is in fact the only one that triggers assimilation; the comparative and excessive, which I assume involve the same syntactic configuration, trigger raising of the root-final vowel but are exceptions to assimilation. See Elordieta (1997 ch.2, ft. vii) for discussion.⁴⁰ Again, as above, the root is accessible to *a*. Assimilation can therefore apply on the cycle at which they are both spelled out.

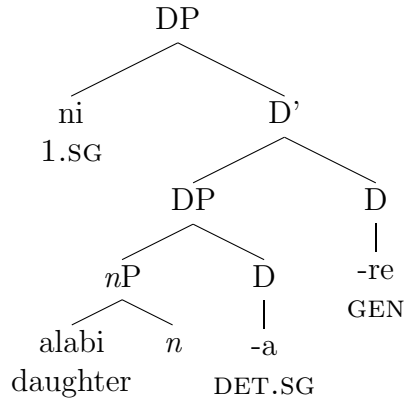
(256) soro-én-a → soruúna
 crazy-SUP-DET.SG 'the craziest'



There are a few other circumstances in the nominal domain in which elements that are linearly adjacent to one another do not undergo assimilation. As I have already mentioned, this is largely due to nominals being rendered inaccessible by D. This is the case with the possessive shown below:

⁴⁰There is also at least one derivational affix which does not trigger raising or undergo assimilation, *-árr*, as in *donostiárra* 'from Donosti.'

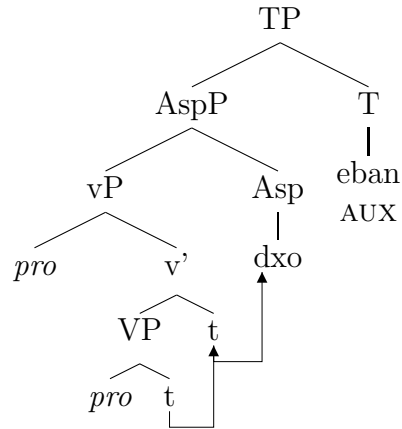
- (257) nire alabi-a → nire alabii
 my daughter-DET.SG 'my daughter'



Here, *alabi* is blocked from interacting with the genitival *-re* by the D (*-a*) in the lower DP. (There could also be more functional projections between the lower and higher DPs, but this is irrelevant.) Note that assimilation does occur between *alabi* and this lower D, as we have already seen for *umiin* ‘child.GEN.PL’ and *altuuk* ‘tall.DET.PL’.

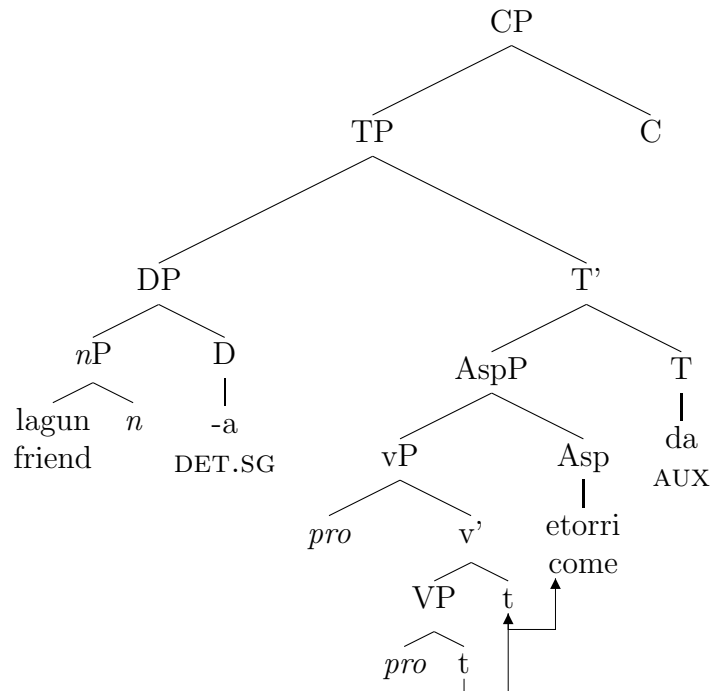
Outside of DP, there is only one context in which assimilation occurs: between a verb and an auxiliary. Following Laka (1990) and many subsequent works on Basque syntax, I show the main verb moving to AspP, with the auxiliary heading TP. This is not crucial to the analysis presented here; movement only to *v* (as in Irurtzun (2007)) or all the way to T (as in Elordieta (1997 et seq.)) will produce the same results. The verb and auxiliary are spelled out together in the complement of C regardless, so assimilation is free to apply to the auxiliary.

- (258) dxo e-ba-n → dxo oban
 hit 3ERG-NONPRES-PAST '(s)he hit it'



Assimilation cannot occur between (the D head of) a subject and a verb, but a syntactic reason for this is not readily apparent. As should be evident from the tree given below, the D in question will be spelled out by C, along with both the main and auxiliary verbs.

- (259) lagun-a etorri da
 friend-DET.SG come AUX 'the friend has come'

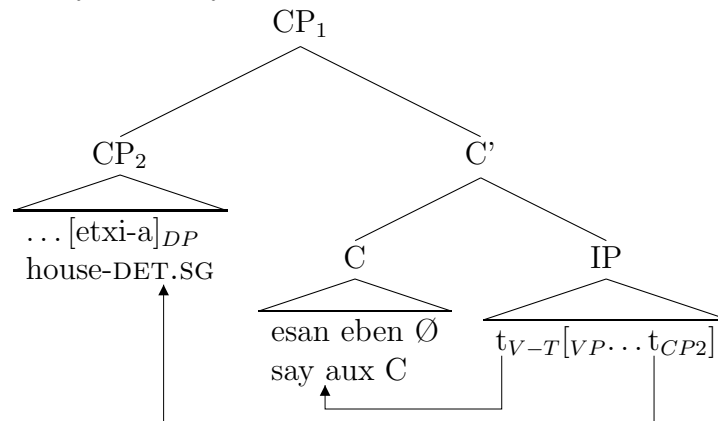


If feature-checking triggered the assimilation rule, this would also be unexpected, since the subject agrees in ϕ -features with the verbal complex. Since there are no cases in which a verb either triggers raising or undergoes assimilation—witness the object-verb and causative examples below, in which there is no assimilation—we may simply say that these rules are simply not triggered by verbs.

- (260) a. *arrañ-a erosi dau*
 fish-DET buy AUX ‘(s)he has bought fish’
 b. *paga eraiñ neutzan*
 pay make AUX ‘I made him/her pay’

This also rules out assimilation between the verb and object in the example below, even though it would otherwise be permitted. Following Irurtzun (2007), pied-piping of the embedded CP_2 triggers V-I-C movement of the main verb and auxiliary.

- (261) [*barristu egingo dabela plasan dagoen etxi-a*] $_{CP}$ *esan eben*
 renovate do AUX.C square is.REL house-DET.SG say AUX
 ‘They said they would renovate the house that is on the square.’



There is also no interaction between a topicalized/focalized argument or an adverb and a verb, as in the following examples. Assimilation of the verb to the topic would be ruled out even if verbs could trigger assimilation because two phase heads, D and C, separate the topic from the verb.

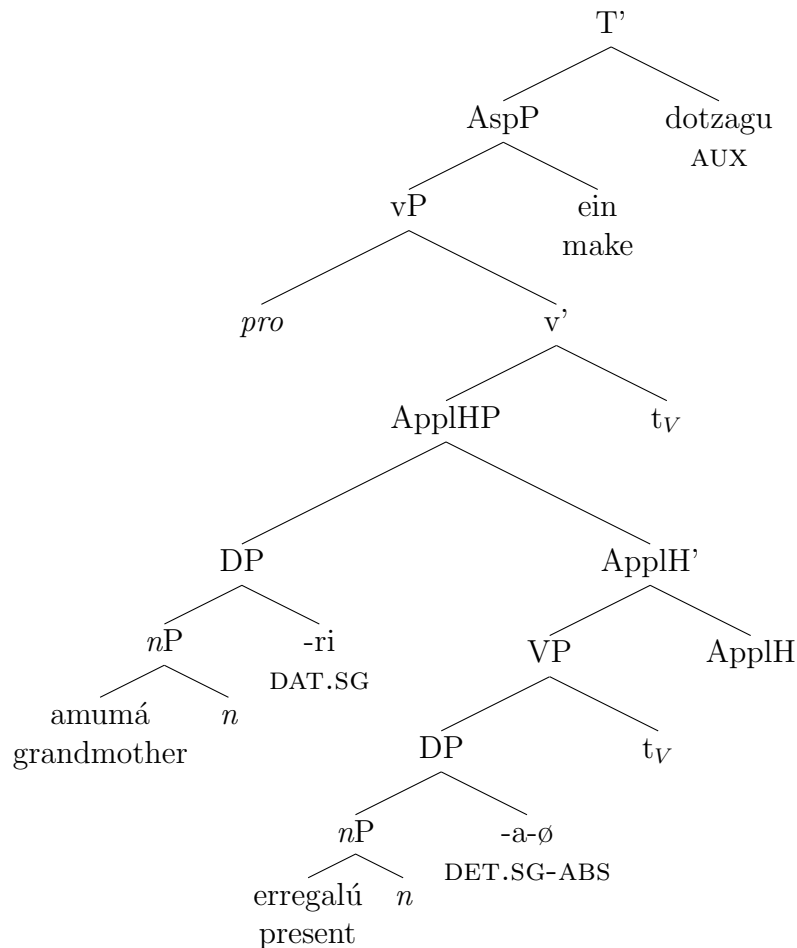
- (262) a. lagun-ari emon dotzo diru-a
 friend-DAT give AUX money-det.sg
 ‘To the friend, (s)he has given the money.’
- b. beti amaitxuten dau askanen
 always finish-PROG AUX last
 ‘(S)he always finishes last.’

Finally, assimilation between two objects is inadmissible, as the following shows:

- (263) amumá-ri erregalú-a-ø ein dotzagu
 grandmother-DAT present-DET.SG-ABS make AUX
 ‘We have made a present for grandmother.’

Basque has a high applicative (Arregi & Molina-Azaola 2004), so the structure posited for the above example is as follows.

(264)



As we have already seen, ApplH is a phase head. This means that the dative *-ri* is too many phases away from *erregalú*: the former will be spelled out by *v* and the latter by its own D, with ApplH transferring its own complement in between.

All the application and non-application environments of the Lekeitio Basque vowel raising and assimilation rules are compatible with a PDbP analysis. Specifically, we analyze both rules as lexical, and as being carried on all determiners and auxiliaries, plus derivational suffixes (subject to a few exceptions). Although we have seen cases in which the rules might be expected to apply but do not, this is not problematic for the analysis. The more crucial point is that the rules *never* apply when two

morpheme-level phase boundaries intervene, which is predicted to be impossible for lexical rules in PDbP. The only elements which we must say cannot trigger this rule are verbs; otherwise we would expect a wider range of application.

5.6.2 Korean Obstruent Voicing

We now turn to Korean for an example of how post-lexical rule application works in PDbP. In Korean, three processes—obstruent voicing, stop nasalization, and homorganic nasal assimilation—apply within the same domain, which has been called the (minor) phonological phrase (see Cho 1990, Jun 1993). I will discuss only the first of these because its effects are the most easily visible, though we will be able to see the others in a few cases. The data presented here comes from Cho (1990:48ff), but I have in some cases corrected glosses & terminology. The obstruent voicing rule, as stated by Cho, is given below.

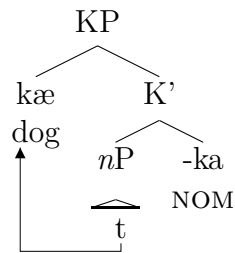
(265) OBSTRUENT VOICING

$$[-\text{CONT}, -\text{ASP}, -\text{TENSE}] \rightarrow [+ \text{VOICE}] / [+ \text{VOICE}] - [+ \text{VOICE}]$$

It is immediately apparent when we look at the pattern of application in the nominal domain that obstruent voicing in Korean applies more broadly than vowel assimilation in Basque. One reason for this is that post-lexical rules are restricted to a single *clause-level* phase domain; another reason is that Korean does not have determiners. In the present analysis I represent Case marking on the head of K(ase)P, with the noun moving to Spec,KP and no DP being projected. Nothing critical rides on this assumption; it could also be that there is a DP layer, but the noun moves into its edge.

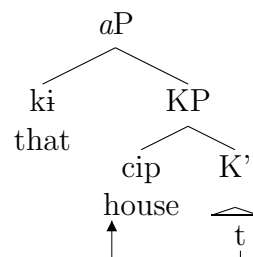
In this configuration, the root and case morphemes are in the same Spell-Out domain, so obstruent voicing can apply. For simplicity's sake, I do not show the structure internal to the *nP*, which will be the same as for Basque, modulo the movement of the root into the functional projection.

- (266) *kæ-ka* → *kæga*
 dog-NOM 'dog'



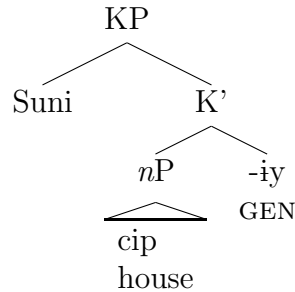
Unlike in Basque, a demonstrative/adjective and a noun can interact. Again, this is because no clause-level phase head intervenes. Here I show *aP* as adjoined to *KP*, loosely following Bošković's (2005) adoption of Abney's (1987) 'NP-over-AP' hypothesis for languages without DP, but a number of variations are possible; all that matters is that these elements are ultimately spelled out together, which they will be the case if there is no D in between them.

- (267) a. *ki cip* → *ki jip*
 that house 'that house'
 b. *motin kilim* → *modin girim*
 every picture 'every picture'



The same is true for a very similar structure with a possessor and a possessee:

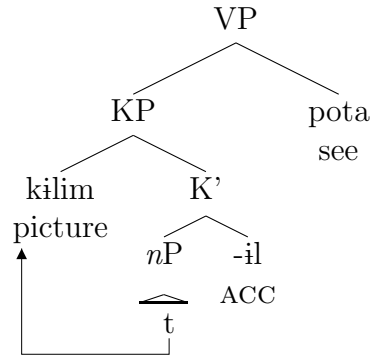
- (268) Suni-iy cip → Suni-iy jip
 Suni-GEN house ‘Suni’s house’



With nominals being so accessible, it is possible to see obstruent voicing between an object and a verb. In order for the object and verb to be spelled out together in the matrix clause, it is important that the verb does not move too high: if it is in *v* or higher, the verb and object would be in separate clause-level domains (unless the object also raises). Evidence for or against verb raising in Korean is very hard to come by (see Han et al. (2007)⁴¹). For the rest of this section I will leave both the object and verb in their base-generated positions. As we will see, this makes the correct predictions about where obstruent voicing can apply: between the verb and the (direct) object, but not between the indirect object and the verb in a ditransitive, or between the subject and the verb in an intransitive. I show the VP without movement (except, of course, the root moving into Spec,KP) below:

⁴¹In fact, Han et al. suggest that learners may choose a grammar with or without verb raising at random. A study of the correlations between the variation they found in scope judgments and possible variation in phonological domains could be enlightening.

- (269) kilim-il pota → kırımil boda
 picture-ACC see 'look at the picture'

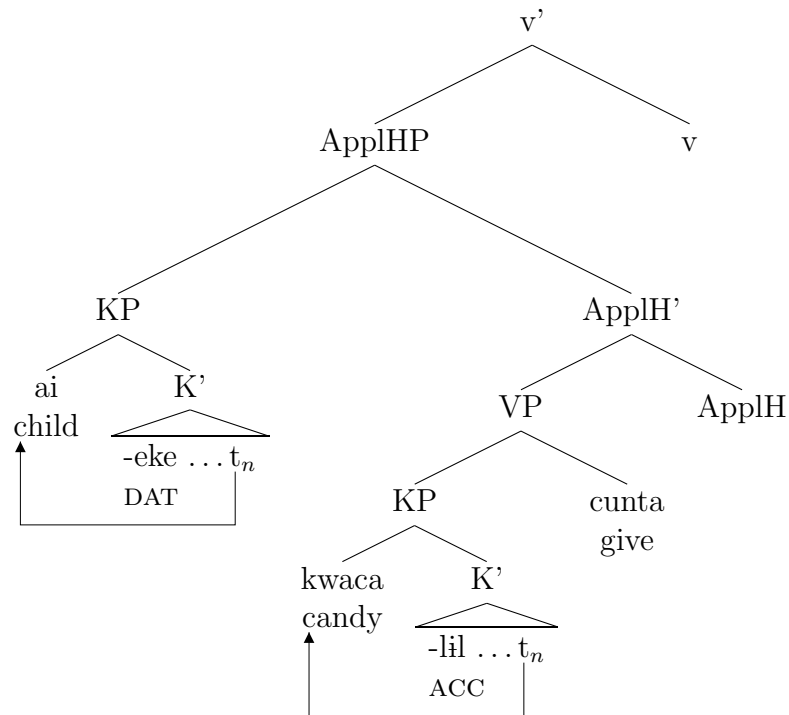


But only the *direct* object is within the same domain as the verb. This is apparent in a ditransitive sentence:

- (270) ai-eke kwaca-lil cunta → aiege kwajaril junda
 child-DAT candy-ACC give 'he gives a candy to the child'

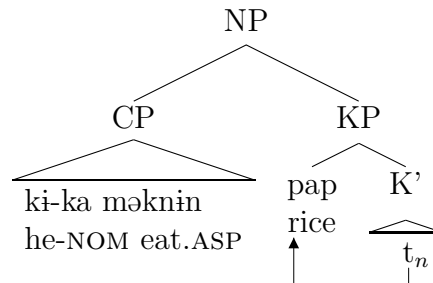
The reason for this is that, as we have already seen several times, ApplH is a phase head. The applied argument in its specifier is therefore spelled out by *v*, separately from the direct object and the verb, which are spelled out by ApplH.

(271)



Similarly, if the embedded verb in the relative clause structure below remains in VP, then v will spell it out along with the head noun *pap* ‘rice.’ Note that it is crucial here that the verb does not move to v or higher. If it did, then *pap* would be spelled out by v and the verb would not be spelled out until the next phase.

(272) $[[\text{ki-ka mək-nin}]_{CP} \text{pap}]_{NP} \rightarrow \text{kiga məɲnin bap}$
 he-NOM eat-ASP rice ‘the rice he is eating’



A lack of verb raising also explains why obstruent voicing is blocked between a subject and a verb, even in the absence of D: one is spelled out in the complement of v , and the other by C.

- (273) kæ-ka canta → kæga canda
 dog-NOM sleep ‘the dog is sleeping’

Further support for the PDbP analysis comes from the fact that no obstruent voicing applies between a subject and an object in a transitive sentence. This again suggests that the verb and object remain in VP; whether the subject stays in its base-generated position of Spec,*v*P or moves to Spec,TP is immaterial. Either way, (S)(OV) domains result.

- (274) kæ-ka pap-il mæk-ninta → kiga pabil mæɲninda
 dog-NOM rice-ACC eat-ASP ‘the dog is eating rice’

Given that a subject in its usual position is too far from the verb to trigger in obstruent voicing, it should come as no surprise that a topicalized argument is also blocked from participating:

- (275) sakwa-nin pəl-inta → sagwanin pərinda
 apple-TOP throw-ASP ‘apples, they throw away’

The final blocking context we will discuss involves conjunction: the second conjunct cannot undergo obstruent voicing that could ostensibly be triggered by the vowel-final conjunction *wa*.

- (276) horaŋi-wa koyaŋi → horaŋiwa koyaŋi
 tiger-CONJ cat ‘the tiger and the cat’

In our discussion of Kimatuumbi, I mentioned the proposal by Schein (1997) that conjoined elements are always CPs. This would explain the blocking effect, because the first conjunct and the conjunction itself would be spelled out by the C associated with that first clause, and the second conjunct would be spelled out separately by its own C. Alternately, we could suppose that the conjunction itself is a phase head. To my knowledge, this possibility has not been investigated. However, it is suggestive that

cross-linguistically, a conjunction forms a prosodic unit with the second conjunct in head-initial languages and with the first conjunct in head-final languages (Zoerner 1995:11). This is demonstrated by the contrast between English (277)-(278) on the one hand, and Japanese (279)-(280), which patterns with Korean (and apparently also Sanskrit; Mark Hale, (p.c.)), on the other:

- (277) a. Robin, and Kim, like apples.
 b. *Robin and, Kim, like apples.
- (278) a. Robin slept, and Kim slept.
 b. *Robin slept and, Kim slept.
- (279) a. Hanako-to, Naoko-wa kawai-i.
 Hanako-and Naoko-TOP pretty-PRES
 ‘Hanako and Naoko are pretty.’
 b. *Hanako, to Naoko-wa kawaii.
- (280) a. Ame-ga hur-u si, kaze-ga huk-u.
 rain-NOM fall-PRES and wind-NOM blow-PRES
 ‘Rain falls, and wind blows.’
 b. *Ame-ga hur-u, si kaze-ga huk-u.

We have seen in this section that obstruent voicing is a well-behaved post-lexical rule of the domain-span type. It applies uniformly within a clause-level spell-out domain, both within and across words, but is blocked from applying across two separate domains.

5.6.3 Serbo-Croatian Clitics

Our third and final case study in PDbP comes from South Slavic. I will concentrate on Serbo-Croatian, but I believe the analysis here can be extended to other languages with second position clitics, such as Slovenian (which we will discuss briefly

in this section as well).⁴² These cases are particularly important to PDbP because clitic placement in South Slavic has been described as depending on two levels of the prosodic hierarchy, the intonational phrase and the phonological phrase. Furthermore, the complex interaction between clitic placement, linearization, vocabulary insertion, and copy deletion provides insight into the ordering of operations at the syntax/phonology interface, which we have until now glossed over.

There are three distinct types of enclitics in Serbo-Croatian: pronominals, auxiliaries, and the interrogative complementizer *li*. Since Radanović-Kocić (1988), it has been recognized that clause-mate clitics must all cluster together in the location which has been described as second position in an I-phrase. The example below, from O'Connor (2002:1), shows that although many deviations from the basic SVO word order are possible due to scrambling and movement of topicalized/focalized elements, an enclitic (here, the 3.SG present auxiliary, *je*) nevertheless must occupy the second position.⁴³

⁴²I leave demonstrations of how PDbP applies to cliticization in other languages for the future but refer the reader to Bošković (2001) for a typology of clitics in South Slavic. I focus here on Serbo-Croatian because it seems to me that this represents a particularly difficult case: only encliticization is available for the clitics under discussion in this language, which makes the second-position phenomenon considerably more rigid. By contrast, in languages like Slovenian, it is possible to employ procliticization when a would-be second-position clitic fails to find a host to its left (i.e., clitics can appear in first position).

⁴³Throughout this section, enclitics are noted by a preceding =, and I have standardized the glosses across data sources to eliminate unnecessary detail.

- (281) a. Jovan =je voleo Mariju
 Jovan.NOM AUX.3SG.PRES loved Marija.ACC
 ‘Jovan loved Marija’
 b. Jovan =je Mariju voleo
 c. Voleo =je Mariju Jovan
 d. Voleo =je Jovan Mariju
 e. Mariju =je Jovan voleo
 f. Mariju =je voleo Jovan
 g. * =Je Jovan voleo Mariju
 h. * Voleo Mariju =je Jovan
 i. * Mariju Jovan voleo =je

When multiple clitics are found in a single sentence, the clitics all cluster in second position, and they strictly obey a particular order (Stjepanović 1999:14). (See Bošković (2001) for further discussion of why this order obtains.)

- (282) li - AUX - DAT - ACC - GEN - REFL - je (AUX.3SG.PRES)⁴⁴

The clustering and ordering properties of Serbo-Croatian clitics are illustrated by sentences like the one below, from Bošković (1995:245):

- (283) Mi =smo =mu =je predstavili juče
 we AUX.1PL.PRES 3M.SG.DAT 3F.SG.ACC introduced yesterday
 ‘We introduced her to him yesterday.’

I will have nothing further to say about the ordering of clitics within clusters; here I am concerned primarily with how clitics, singly or in clusters, come to be in second position and how to characterize that position.

The presence of an appositive, parenthetical, or fronted constituent (in terms of the prosodic hierarchy, anything that is parsed as a separate I-phrase) can ‘delay’

⁴⁴Note that there is a homophonous 3F.SG.ACC clitic *je* which need not come cluster-finally.

clitic placement to the second position after that constituent. This is shown in the examples below, from Bošković (2001:65).

- (284) a. Sa Petrom Petrovićem srela =se samo Milena
 with Petar Petrović met REFL only Milena
 ‘With Petar Petrović, only Milena met’
- b. Znači da, kao što rekoh, oni =će sutra doći
 means that as that said they AUX.3PL.FUT tomorrow arrive
 ‘It means that, as I said, they will tomorrow arrive’
- c. Ja, tvoja mama, obećala =sam =ti sladoled
 I your mother promised AUX.1SG.PRES 2SG.DAT ice cream
 ‘I, your mother, promised you an ice cream’
- d. Prije nekoliko godina sa Petrom Petrovićem srela =se samo Milena
 before several years with Petar Petrović met REFL only Milena
 ‘A few years ago, with Petar Petrović, only Milena met’

This type of data is taken to show that a phonological account of clitic placement is necessary: “the correct descriptive generalization for the distribution of Serbo-Croatian second-position clitics is then not that they are second within their clause, but within their I-phrase, which strongly indicates that the second position effect is phonological in nature” (Bošković 2001:66). The question, then, is how to achieve this placement. I concur with Bošković’s arguments (which I will not go through here; see Bošković (1995, 2001), Bošković & Nunes (2007)) that it occurs roughly as follows: when a piece of syntax is spelled out, the semantic and phonological systems each interpret only one copy in a chain. In the phonology, the highest copy is interpreted unless it cannot find an acceptable host, in which case the derivation crashes (cf. Lasnik’s (1995) Stranded Affix Filter). In this case, the lower copy is interpreted. In short, some derivations that are syntactically well-formed but prosodically illicit are filtered out at the syntax/phonology interface.

In contrast to analyses which depend on clitics undergoing movement “at PF” (i.e.,

Prosodic Inversion accounts; see Halpern (1992, 1995) and many others), Bošković (2001) shows that the filtering analysis correctly ties clitic placement to the mobility of the constituent in first position. That is, whatever appears in front of a clitic must be able to get there by independently-motivated syntactic movement; the mere need to host a clitic cannot induce movement. Furthermore, a clitic can never ‘split up’ a constituent that could not be split in syntax. For example, both of the following sentences (from Radanović-Kocić (1996:438)) are acceptable; in the (b) sentence, *moj* ‘my’ is focused:

- (285) a. Moj jorgan =je od perja
 my comforter AUX.3.SG.PRES of down
 ‘My comforter is made of down’
 b. Moj =je jorgan od perja
 ‘My comforter is made of down’

The possibility of placing the enclitic after *moj* corresponds with the possibility of left-branch extraction in Serbo-Croatian, which Bošković (2005) attributes to a lack of DP. Thus, *moj* can move alone (for focus reasons) to a position higher than the auxiliary. The example above contrasts with the one below (from Bošković (2001:14)), which shows that it is impossible for an enclitic to split *prema* ‘with’ from its object; this correlates with the fact that preposition stranding is not allowed in Serbo-Croatian, so *prema* could not have moved ahead of the auxiliary by itself.

- (286) a. *Prema =su Mileni Milan i Jovan išli
 toward AUX.3PL.PRES Milena.DAT Milan.NOM and Jovan.NOM walked
 ‘Toward Milena Milan and Jovan walked’
 b. Prema Mileni =su Milan i Jovan išli
 c. Milan i Jovan =su išli prema Mileni

Even if we accept these two main claims of Bošković’s proposal—that illicit derivations must be filtered out at the interface, and that the need to host a clitic cannot

motivate movement—there are still a number of very consequential issues to be worked out. In what follows, I give a proposal regarding where clitics are located in Serbo-Croatian and flesh in the details of how the ‘second position’ phenomenon is achieved.

My proposal follows along the lines of Franks (2000), though it differs from Franks’ theory in several respects. Along with Franks, I propose that the placement of clitics involves two crucial components: syntactic movement and linearization at the syntax/phonology interface; I also follow his argument that clitics are bundles of pure formal features. The three classes of clitics in Serbo-Croatian embody three different types of features: auxiliary clitics are associated with ϕ -features which they must check on T; pronominal clitics are K^o elements which need to check case on the various functional heads with which they are associated, and the interrogative *li* is a complementizer which always appears in C. The idea is that the clitics essentially piggyback on a verb/participle which moves through every functional projection above VP, and they cluster because movement to check one formal feature always pied-pipes the rest. However, the formal features of the verb continue climbing, along with the clitics, higher than the semantic features of the verb.⁴⁵ These semantic features are crucial to vocabulary insertion, so the verb can only be pronounced in positions where both its formal and semantic features are present. Since the clitics contain *only* formal features, they are free to be pronounced as high as possible.

This is where the peculiarities of clitics in this particular language come into play.

⁴⁵Bošković (1995 et seq.) and Franks (2000) argue that participles cannot to raise to C in Serbo-Croatian, though inflected verbs can. However, I follow Legate (2008), who argues that participles can in fact raise to C; the other authors have only shown that such movement is blocked by adverbs and the presence of the complementizer *da*.

If a clitic does not find a host, the derivation crashes. Serbo-Croatian clitics must lean to their left, unlike their counterparts in Slovenian, which can lean to the right as well—hence the acceptability of clitics occurring sentence-initially in this language. For example, in Slovenian the following sentences, taken from Bošković (2001:154), are acceptable:

- (287) a. Se= mi= je= smejal
 REFL 1.SG.DAT AUX.3SG.PRES laughed
 ‘He was laughing at me.’
 b. Mu= ga= je= že dala?
 3SG.M.DAT 3SG.N.ACC AUX.3SG.PRES already given
 ‘Has she already given it to him?’

Slovenian thus provides a minimal contrast with Serbo-Croatian. As Bošković (2001:154) puts it, “[w]hile [Serbo-Croatian] clitics are necessarily suffixes, i.e., they are lexically specified as attaching to the right edge of their host, Slovenian clitics are prosodically neutral, they can attach either to the left or to the right edge of their host.”

But in the absence of the ability to procliticize (i.e., as far as Serbo-Croatian is concerned), the requirement for a host can be satisfied in two ways: either by a head which sits in the same position as the clitic, or by a head to its left, within the same Spell-Out domain. I give examples of these two options below; the second example, illustrating a clitic hosted by C^o , is adapted from Franks (2000).⁴⁶ Note that in the first example, the highest copy of the auxiliary clitic is not pronounced, because it cannot find a host. Let us put aside for a moment the issue of how it comes to be

⁴⁶Franks assumes that the complementizer *da* in this example is inserted post-syntactically to support the clitics. However, the presence of *da* is required even in sentences without clitics, as has been noted at least since Čavar & Wilder (1994). We might tie this to the avoidance of an IPEG violation in the absence of a filled Spec,CP; recall our earlier discussion of An (2007b).

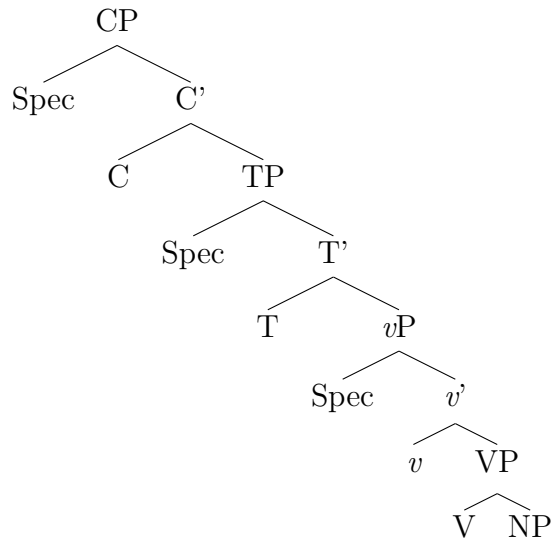
that the lower copy of the clitic is pronounced instead.

- (288) a. Jovan =je voleo Mariju
 Jovan.NOM AUX.3SG.PRES loved Marija.ACC
 ‘Jovan loved Marija’
 $[_{CP} \text{voleo je } [_{TP} \text{Jovan voleo je } [_{vP} \text{voleo Marija}]]]$
- b. Znam da =mu =ga Jovan svaki dan predstavlja
 know.1.SG that him.DAT him.ACC Jovan each day introduces
 ‘I know that Jovan introduces him to him every day’
 $[_{CP} \text{znam } [_{CP} \text{da predstavlja mu ga } [_{TP} \text{Jovan predstavlja mu ga } [_{vP} \text{svaki dan predstavlja mu ga } [_{VP} \text{predstavlja ga}]]]]]$

Two facets of this process require further explanation. The first of these is how the clitics come to be to the right of their hosts. In a derivation like (288a), where the host of the clitic in T^o is the subject in Spec,TP, this is a natural consequence of regular Spec-Head-Comp linearization. The ordering in (288b) is accounted for if the clitic, like any other affix in PDbP, comes with a set of parameter values specifying how it is to be concatenated: in this case, at the tail end of the string. As Bošković (2000) and Franks (2000) have noted, this is not PF movement of the sort proposed by advocates of the Prosodic Inversion approach to clitic placement, but rather an independently-needed linearization mechanism of exactly the same type that linearizes all affixes; recall discussion in Chapter 4.

Second, and perhaps most importantly, we want to understand how this captures the generalization that clitics must sit in second position within their I-phrase. On the view espoused here, the second position generalization emerges from two things: first, the fact that clitics can’t be first in this particular language (i.e., they can only lean to their left), and second, the nature of phrase structure. With regards to the latter, consider a basic left-branching tree:

(289)



When the terminals are linearized, the result will be an alternation of heads and non-heads; keep in mind that even though the coarse projections depicted here are standing in here for much more nuanced cartographies, these, too, will obey the head/non-head pattern. If clitics sit in every head position down the main spine of the tree and they are always pronounced in the highest possible position, then there will only ever be at most one specifier ahead of the clitic, plus any head that might be in its position as well. Then, so long as cliticizing to the specifier is preferred over cliticizing to the head when both are present, the clitic will end up in second position.

Since syntax is recursive, the non-heads (i.e., specifiers) may themselves comprise a head and a non-head. But from the perspective of the clitic on the main spine of the tree, this is irrelevant; only the head of the specifier is visible, and it will attach to that if need be. The head of the specifier essentially serves as a placeholder while the whole complex specifier is assembled in a separate derivational workspace, ultimately to be integrated with the main structure. When that occurs, the clitic will linearize to the right of this newly expanded string. Thus, there is really no difference at all

between second-position placement after the first word and after the first phrase. A clitic cannot ever truly split a phrase, because it cannot *see* phrases, only heads. The only time an apparent split occurs is when part of a phrase has independently been extracted, for focus or otherwise; if a clitic is sitting in the head of the projection into which that material has moved, and that head is the highest position in which the clitic finds a host, then it will be pronounced there.

This leaves only the question of why clitics are delayed by appositives, parentheticals, relative clauses, topicalized elements, and so forth. The answer is apparent when we consider why clitics move in the first place: we have already discussed Franks' proposal that auxiliary clitics move to check ϕ -features and pronominal clitics move to check case. Neither of these provide any reason for clitics to move into the topic/focus realm, into relative, parenthetical, or appositive clauses off of the main derivational cascade ('extra-clausal material'), etc. The clitics simply stick to the projections in which they have their feature-checking business. Furthermore, as in the case of complex specifiers, anything that is not in the main derivation will only be integrated after any clitics find their hosts, so it is impossible for a clitic to lean on any extra-clausal material. From this perspective, the generalization that clitics must sit in second position within their I-phrase is getting it backwards: the fact that these extra-clausal elements are treated as a distinct prosodic unit (i.e., they are usually set off by pauses and form their own post-lexical rule domains), and the fact that clitics cannot see them, are both symptoms of the same underlying cause: they are composed separately from the rest of the derivation.

5.7 Conclusions

In this chapter I hope to have given ‘proof of concept’ for PDbP: looking closely at syntax allows us to identify language-specific phonological domains, without the need to build up hierarchy in the phonology. At this point, I believe this is the most one can offer given that the syntax upon which a theory of the syntax/phonology interface must depend remains in flux. For this reason, the details of how to implement PDbP may well have to be re-negotiated later. Nevertheless, already there is a quickly-growing list of empirical successes which have been achieved by tying phonological rule application directly to spell-out domains: Seidl (2001) primarily on Bantu & Korean; Marvin (2002) on English & Slovenian; Kahnemuyipour (2004) on Persian, English, and German; Piggott & Newell (2006) and Newell (2008) primarily on Ojibwa; Sato (2006) on Taiwanese, French, Gilyak, Kinyambo, & Welsh; Ishihara (2007) on Japanese; Bachrach & Wagner (2007) on Portuguese; Michaels (2007) on Malayalam; Kamali & Samuels (2008a,b) on Turkish; and more programmatically, Embick (2008) and Scheer (2008a). While these approaches all differ from PDbP in substantial respects, I hope that the theoretical framework and case studies given here will provide a guide for the limited re-analysis that would bring these earlier studies compatible with my claims. The phonological literature is rife with obvious candidates for PDbP analyses: for instance, Rizzi & Savoia (1993) note that /u/-propagation in southern Italian dialects occurs between D and an associated noun/adjective, but not between a quantifier and noun/adjective, or between an adjective and noun (in either order). This is highly reminiscent of what we saw for Basque.

The ultimate message of this chapter is that, if we want to understand cross-

linguistic variation in phonology, we need to understand cross-linguistic variation in morphosyntax better. This calls for collaboration between phonologists, morphologists, and syntacticians, all working together towards the common goal of describing the range of linguistic structures that are available. My hope is that this could shed light on several outstanding issues, such as the intriguing phonological differences between polysynthetic and less agglutinative languages. In the languages we have seen in this chapter, a clause-level phase defines a “phonological phrase” which may consist of several words (recall the Maximal ϕ Condition of Richards (2006b)). This provides an interesting contrast with the conclusions of Compton & Pittman (2007), who argue that in Inuktitut, the phase defines a single prosodic word; Piggott & Newell (2006) argue the same for Ojibwa. This suggests that at the opposite end of the spectrum are isolating languages like Chinese, which are extremely analytic: for them, every terminal defines a prosodic word. This could perhaps be thought of as the prosodic word being defined as a morpheme-level phase rather than a clause-level one.

Chapter 6

The Biological Basis of Phonological Primitives

6.1 Introduction

In this chapter, we take the theory of phonology developed throughout the earlier portion of this dissertation as our starting point and discuss the implications of this theory for the language and cognition more broadly. First and foremost, this chapter can and should be construed as a plausibility argument for the theory presented in the previous chapters: to the extent we can show that other species can do what phonological computations require, then the model gains credibility from an evolutionary/biological standpoint. As Hornstein & Boeckx (2009:82) explain,

“[I]n light of the extremely recent emergence of the language faculty, the most plausible approach is one that minimizes the role of the environment (read: the need for adaptation), by minimizing the structures that need to evolve, and by predefining the paths of adaptation, that is, by providing preadapted structures, ready to be recruited, or modified, or third factor design properties that emerge instantaneously, by the sheer force of physical laws.”

Along these lines, I will demonstrate on the basis of behavioral and physiological studies on animal cognition that all the cognitive abilities necessary for the phono-

logical representations and operations which I proposed in the previous chapters are present in creatures other than *Homo sapiens* (even if not to the same degree) and in domains other than phonology or, indeed, language proper. This implies that nothing required by phonology is part of the faculty of language in the narrow sense (FLN, as opposed to the faculty of language in the broad sense, FLB), in the terms of Hauser et al. (2002a) and Fitch et al. (2005). In particular, the conclusion I draw from the studies described in this chapter is that phonology may be entirely explainable through Third Factor principles (recall discussion in Chapter 2) pertaining to general cognition and the Sensori-Motor system (Chomsky 2005, et seq.). I therefore reject the claim made by Pinker & Jackendoff (2005: 212) that “major characteristics of phonology are specific to language (or to language & music), [and] uniquely human,” and their statement that “phonology represents a major counterexample” to the hypothesis proposed by Hauser et al. (2002a), namely that FLN consists of only recursion and the mapping from narrow syntax to the interfaces. What I suggest, in effect, is that the operations and representations which underlie phonology were exapted, or recruited from other cognitive domains for the purpose of externalizing language.¹

Few authors have discussed phonology as it pertains to the FLN/FLB distinction. For example, Hauser et al. (2002a:1573) list a number of approaches to investigating a list of the Sensori-Motor system’s properties (shown below in (290)), all of which

¹On the possibility that language more generally is an exaptation, see among others Piattelli-Palmarini (1989), Uriagereka (1998), Boeckx & Piattelli-Palmarini (2005), Hauser et al. (2002a), Fitch et al. (2005).

are taken to fall outside FLN. However, none of these pertain directly to phonological computation.

(290) a. Vocal imitation and invention

Tutoring studies of songbirds, analyses of vocal dialects in whales, spontaneous imitation of artificially created sounds in dolphins

b. Neurophysiology of action-perception systems

Studies assessing whether mirror neurons, which provide a core substrate for the action-perception system, may subserve gestural and (possibly) vocal imitation

c. Discriminating the sound patterns of language

Operant conditioning studies of the prototype magnet effect in macaques and starlings

d. Constraints imposed by vocal tract anatomy

Studies of vocal tract length and formant dispersion in birds and primates

e. Biomechanics of sound production

Studies of primate vocal production, including the role of mandibular oscillations

f. Modalities of language production and perception

Cross-modal perception and sign language in humans versus unimodal communication in animals

While all of these issues undoubtedly deserve attention, they address two areas—how auditory categories are learned and how speech is produced—which are peripheral to the core of phonological computation. The most interesting two issues from my

perspective are (c) and (f), which we discussed in §3.2.1. These are of course very relevant to the idea of emergent features and learned phonological categories, to which I am sympathetic. And the instinct to imitate, addressed in (a) and (b), is clearly necessary to language acquisition. However, investigating neither these nor any of the other items in (290) has the potential to address how phonological objects are represented or manipulated, particularly in light of the substance-free approach to phonology, which renders questions about the articulators (e.g., (d, e)) moot since their properties are totally incidental and invisible to the phonological system.

Two papers by Yip (2006a,b) outline a more directly relevant set of research aims. Yip suggests that, if we are to understand whether ‘animal phonology’ is possible, we should investigate whether other species are capable of the following:²

- (291)
- a. Grouping by natural classes
 - b. Grouping sounds into syllables, feet, words, phrases
 - c. Calculating statistical distributions from transitional probabilities
 - d. Learning arbitrary patterns of distribution
 - e. Learning/producing rule-governed alternations
 - f. Computing identity (total, partial, adjacent, non-adjacent)

This list can be divided roughly into three parts (with some overlap between them): (291a,b) are concerned with how representations are organized, (291c,d) are concerned with how we arrive at generalizations about the representations, and

²Yip mentions two additional items which also appear on Hauser et al.’s list: categorical perception/perceptual magnet effects and accurate production of sounds (mimicry).

(291e,f) are concerned with the operations that are used to manipulate the representations. I would add three more areas to investigate in non-linguistic domains and non-human animals:

- (292)
- g. Exhibiting preferences for contrast/rhythmicity
 - h. Performing numerical calculations (parallel individuation and ratio comparison)
 - i. Using computational operations: search, copy, concatenate, delete

In the rest of this chapter, I will present evidence that a wide range of animal species are capable of the tasks in (a-i), though it may be the case that there is no single species (except ours) in which all these abilities cluster in exactly this configuration. I show (contra Yip) that there is already a substantial amount of literature demonstrating this, and that it is reasonable to conclude on this basis that no part of phonology, as conceived in this dissertation, is part of FLN. In §6.2, I focus on the abilities which underlie (a,b,h)—that is, how phonological material is grouped. Next, in §6.3, I turn to (c-g), or the ability to identify and produce patterns. Finally, in §6.4, I discuss (e,i), the abilities which have to do with symbolic computation.

Before turning to these tasks, though, I would like to address one major concern which might be expressed about the discussion to follow. This concern could be phrased as follows: how do we know that the animal abilities for which I provide evidence are truly comparable to the representations and operations found in human phonology, and what if these abilities are only analogous, not homologous? Admittedly, it is probably premature to answer these questions for most of the abilities we will be considering. But even if we discover that the traits under consideration are

indeed analogous, all is not lost by any means. In connection with this, I would like to highlight the following statement from Hauser et al. (2002a:1572):

“Despite the crucial role of homology in comparative biology, homologous traits are not the only relevant source of evolutionary data. The convergent evolution of similar characters in two independent clades, termed ‘analogies’ or ‘homoplasies,’ can be equally revealing [(Gould 1976)]. The remarkably similar (but nonhomologous) structures of human and octopus eyes reveal the stringent constraints placed by the laws of optics and the contingencies of development on an organ capable of focusing a sharp image onto a sheet of receptors. [...] Furthermore, the discovery that remarkably conservative genetic cascades underlie the development of such analogous structures provides important insights into the ways in which developmental mechanisms can channel evolution [(Gehring 1998)]. Thus, although potentially misleading for taxonomists, analogies provide critical data about adaptation under physical and developmental constraints. Casting the comparative net more broadly, therefore, will most likely reveal larger regularities in evolution, helping to address the role of such constraints in the evolution of language.”

In other words, analogs serve to highlight Third Factor principles which might be at play, and help us to identify the set of constraints which are relevant to the evolutionary history of the processes under investigation. For example, both human infants and young songbirds undergo a babbling phase in the course of the development of their vocalizations. Even though we do not want to claim that the mechanisms responsible for babbling in the two clades are homologous, nevertheless

“their core components share a deeply conserved neural and developmental foundation: Most aspects of neurophysiology and development—including regulatory and structural genes, as well as neuron types and neurotransmitters—are shared among vertebrates. That such close parallels have evolved suggests the existence of important constraints on how vertebrate brains can acquire large vocabularies of complex, learned sounds. Such constraints may essentially force natural selection to come up with the same solution repeatedly when confronted with similar problems.” (Hauser et al. 2002a:1572)

We may not know what those constraints are yet, but until we identify the homologies *and* analogies between the mechanisms which underlie human and animal cognition, we cannot even begin to tackle the interesting set of questions which arises regarding the constraints on cognitive evolution. The present study, then, provides a place for us to begin this investigation in the domain of human phonological computation.

6.2 Grouping

One might consider the broad category of grouping processes in phonology to be the best candidate for harboring a process unique to language. After all, the hypothesis put forward by Hauser, Chomsky, and Fitch takes recursion to be the central property of FLN (along with the mappings from narrow syntax to the Conceptual-Intensional and Sensori-Motor interfaces), and recursion can be described as the nesting of one object within another object of the same type: a group within a group.

The theory of phonology for which I have argued has fewer groupings, and consequently fewer chances for those groupings to exhibit recursion or hierarchy, than most contemporary approaches. This is true at virtually every level, from the sub-segmental to the utterance: I posit no feature geometry; no subsyllabic constituency; no bracketing of morphemes; and I have argued that phonological representations are fundamentally ‘flat’ or ‘linearly hierarchical.’ The illusion of hierarchy is created by the pervasive processes of ‘chunking’ (discussed in this section) and repeated concatenation (discussed in §6.4):

(293) Concatenation	Chunking
_____+()	()()()()
_____+()	
____+()	

Even given a slimmed-down theory like this one, nobody can deny that grouping/chunking is an integral part of phonology (and there is evidence that infants use this strategy in non-linguistic domains as well; see Feigenson & Halberda (2004)): features group into segments and segments group into longer strings such as syllables, morphemes, and phonological phrases. Of these last three types of groups, only the first is a truly phonological concept, since on my view phonology is a passive recipient of morphemes (i.e., morpheme-level spell-out domains; recall Chapter 5) and the chunks which correspond to phonological phrases (i.e., clause-level spell-out domains). Note that the model I assume is recursive in the sense that there are two types of spell-out domain, with the potential for several morpheme-level domains within a single clause-level one. However, these domains come directly from the narrow syntax, which is totally compatible with Hauser et al.'s hypothesis that syntax is the source—but crucially *not* the exclusive domain—of all recursive structures, and that once syntax is available, the modules with which it interfaces may be subject to modification. Additionally, as we have already discussed in Chapter 3, segmenting the speech stream into words or morphemes (or syllables) also depends on what is essentially the converse of grouping, namely edge detection. We will discuss edge

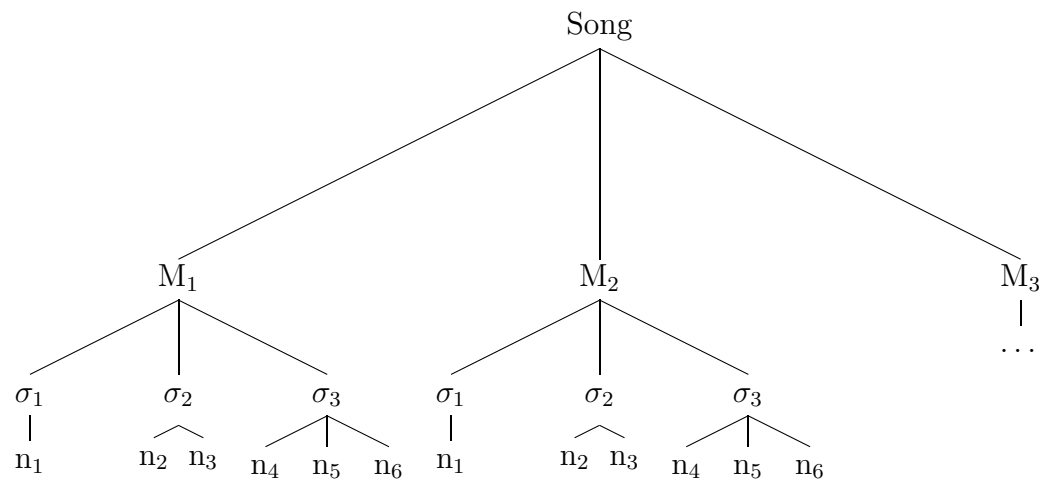
detection and the extraction of other patterns in §6.3.

Human beings are masters at grouping, and at making inductive generalizations. Cheney & Seyfarth (2007:118) write that “the tendency to chunk is so pervasive that human subjects will work to discover an underlying rule even when the experimenter has—perversely—made sure there is none.” This holds true across the board, not just for linguistic patterns. With respect to other species, I have already mentioned in §3.2.1 that many studies beginning with Kuhl & Miller (1975) show that mammals (who largely share our auditory system) are sensitive to the many of the same acoustic parameters as define phonemic categories in human language. Experiments of this type provide the most direct comparanda to the groupings found in phonology. Also, relevantly to the processing of tone and prosody, we know that rhesus monkeys are sensitive to pitch classes—they, like us, treat a melody which is transposed by one or two octaves to be more similar to the original than one which is transposed by a different interval (Wright et al. 2000). They can also distinguish rising pitch contours from falling ones, which is an ability required to perceive pitch accent, lexical tone, and intonational patterns in human speech (Brosch et al. 2004). However, animals are generally more sensitive to absolute pitch than they are to relative pitch; the opposite is true for humans, and it is relative pitch contrasts which are used linguistically (see Patel 2008).

We can also approach the question of whether animals can group sensory stimuli in ways that are relevant to phonology by investigating whether their own vocalizations contain internal structure. The organization of bird song is particularly clear, though it is not obvious exactly whether/how analogies to human language should be made.

Yip (2006a) discusses how zebra finch songs are structured, building on work by Doupe & Kuhl (1999) and others. The songs of many passerine songbirds consist of a sequence of one to three notes (or ‘songemes’ as Coen (2006) calls them) arranged into a ‘syllable.’ The syllables, which can be up to one second in length, are organized into motifs which Yip considers to be equivalent to prosodic words but others equate with phrases. There are multiple motifs within a single song. The structure of a song can be represented graphically as follows, where M stands for motif, σ stands for syllable, and n stands for note (modified from Yip (2006a)):

(294)



There are a few important differences between this birdsong structure and those found in human phonology, some of which are not apparent from the diagram. First, as Yip points out, there is no evidence for binary branching in this structure, which suggests that the combinatory mechanism used by birds cannot be equated with binary Merge (but it could be more along the lines of the concatenation mechanism which creates flat phonological structures). Second, the definition of a ‘syllable’ in birdsong is a series of notes/songemes bordered by silence (Williams & Staples 1992, Coen 2006). This is very unlike syllables in human language. Third, the examples from numerous species in Slater (2000) show that the motif is typically a domain of

repetition (as I have represented it above); the shape of a song is $((a^x)(b^y)(c^z))^w$ with a string of syllables a, b, c repeated in order. This is quite reminiscent of reduplication. Payne (2000) shows that virtually the same can be said of humpback whale songs, which take the shape $(a \dots n)^w$, where the number of repeated components, n , can be up to around ten.

Both the birdsong and whalesong structures are what I have called ‘flat’ (in the sense of Neeleman & van de Koot (2006)) or ‘linearly hierarchical’ (in the sense of Cheney & Seyfarth (2007))—exactly what I have argued for human phonology. It is interesting to note in conjunction with this observation that baboon social knowledge is of exactly this type, as Cheney & Seyfarth have described. Baboons within a single tribe (of up to about eighty individuals) obey a strict, transitive dominance hierarchy. But this hierarchy is divided by matriline; individuals from a single matriline occupy adjacent spots in the hierarchy, with mothers, daughters, and sisters from the matriline next to one another. So an abstract representation of their linear dominance hierarchy would look something like this, with each x representing an individual and parentheses defining matrilines:

$$(295) \quad (xxx)(xx)(xxxx)(xxx)(xxxxxxxx)(xxx)(x)(xxxx)$$

The difference between the baboon social hierarchy and birdsong, which I translate into this sort of notation below, is merely the repetition which creates a motif (think of baboon individuals as corresponding to songemes and matrilines as corresponding to syllables):

$$(296) \quad \underbrace{(n_1)(n_2n_3)(n_4n_5n_6)}_{\text{motif}_1} \underbrace{(n_1)(n_2n_3)(n_4n_5n_6)}_{\text{motif}_2}$$

There is evidence to suggest that, as in phonology (but strikingly *unlike* narrow syntax), the amount of hierarchy capable of being represented by non-human animals is quite limited. In the wild, apes and monkeys very seldom spontaneously perform actions which are hierarchically structured with sub-goals and sub-routines, and this is true even when attempts are made to train them to do so. Byrne (2007) notes one notable exception, namely the food processing techniques of gorillas.³ Byrne provides a flow chart detailing a routine, complete with several decision points and optional steps, which mountain gorillas use to harvest and eat nettle leaves. This routine comprises a minimum of five steps, and Byrne reports that the routines used to process other foods are of similar complexity. Byrne further notes that “all genera of great apes acquire feeding skills that are flexible and have *syntax-like* organisation, with hierarchical structure.... Perhaps, then, the precursors of linguistic syntax should be sought in primate *manual* abilities rather than in their vocal skills” (Byrne 2007:12; emphasis his). I concur that manual routines provide an interesting source of comparanda for the syntax of human language, broadly construed (i.e., including the syntax of phonology). Fujita (2007) has suggested along these lines the possibility that Merge evolved from an ‘action grammar’ of the type which would underlie apes’ foraging routines.

Other experiments reveal that non-human primates may be limited in the com-

³Interestingly, apes’ food processing routines appear to be at least partially learned through imitation rather than trial-and-error, as evidenced by the techniques of young chimpanzees who have been injured by snares. Such disabled individuals do not exhibit novel techniques, which we would expect if they learned how to forage independently; instead, even the most severely affected chimps use techniques which very closely resemble those of able-bodied individuals.

plexity of their routines in interesting ways. For instance, Johnson-Pynn et al. (1999) used bonobos, capuchin monkeys, and chimpanzees in a study similar to one done on human children by Greenfield et al. (1972) (see also discussion of these two studies by Conway & Christiansen (2001)). These experiments investigated how the subjects manipulated a set of three nesting cups (call them A, B, C in increasing order of size). The subjects' actions were categorized as belonging to the 'pairing,' 'pot,' or 'subassembly' strategies, which exhibit varying degrees of embedding:⁴

- (297) a. Pairing strategy: place cup B into cup C. Ignore cup A.
b. Pot strategy: first, place cup B into cup C. Then place cup A into cup B.
c. Subassembly strategy: first, place cup A into cup B. Then place cup B into cup C.

The pairing strategy is the simplest, requiring only a single step. This was the predominant strategy for human children up to twelve months of age, and for all the other primates—but the capuchins required watching the human model play with the cups before they produced even this kind of combination. The pot strategy requires two steps, but it is simpler than the subassembly strategy in that the latter, but not the former, requires treating the combination of cups A + B as a unit in the second

⁴The situation is actually substantially more complicated than this, because the subjects need not put the cups in the nesting order. To give a couple examples, putting cup A into cup C counts as the pairing strategy; putting cup A into cup C and then placing cup B on top counts as the pot strategy. I refer the reader to the original studies for explanations of each possible scenario. The differences between the strategies as I have described them in the main text suffice for present purposes.

step. (We might consider the construction of the A + B unit as being parallel to how complex specifiers and adjuncts are composed ‘in a separate derivational workspace’ in the syntax; see Fujita (2007).) Human children use the pot strategy as early as eleven months (the youngest age tested) and begin to incorporate the subassembly strategy at about twenty months. In stark contrast, the non-human primates continued to prefer the pairing strategy, and when they stacked all three cups, they still relied on the pot strategy even though the experimenter demonstrated only the subassembly strategy for them. Though we should be careful not to discount the possibility that different experimental methodologies or the laboratory context is responsible for the non-humans’ performance, rather than genuine cognitive limitations, the results are consistent with the hypothesis that humans have the ability to represent deeper hierarchies than other primates. This is what we predict if only humans are endowed with the recursive engine that allows for infinite syntactic embedding (Hauser et al. 2002a).

Many other types of experimental studies have also been used to investigate how animals group objects. It is well known that a wide variety of animals, including rhesus monkeys, have the ability to perform comparisons of analog magnitude with small numbers (<4). They can discriminate between, for instance, groups of two and three objects, and pick the group with more objects in it. As Hauser et al. (2000) note, such tasks require the animal to group the objects into distinct sets, then compare the cardinality of those sets. Further data come from Schusterman & Kastak (1993), who taught a California sea lion named Rio to associate arbitrary visual stimuli (cards with silhouettes of various objects printed on them). On the basis of being taught to

select card B when presented with card A, and also to select card C when presented with card B, Rio transitively learned the A-C association.⁵ Rio also made symmetric associations: when presented with B, she would select A, and so forth.

We might consider the groups Rio learned to be akin to arbitrary classes such as which sounds are allophones of a particular phoneme, or which phonemes participate in a given alternation. This even comes close to the idea of a natural class (recall Chapter 3), which has also been studied in animals to a certain degree, though not presented in those terms. We can think of natural classes as multiple ways of grouping the same objects into sets according to their different properties (i.e., features). Alex the parrot had this skill: he could sort objects by color, shape, or material (reported by his trainer in Smith 1999).

With respect to the ability to group objects, then, I conclude that animals—especially birds and primates in particular—are capable of everything phonology requires. They perceive (some) sounds categorically like we do; their vocalizations show linearly hierarchical groupings like ours do; they can assign objects arbitrarily to sets like we do; they can categorize objects into overlapping sets according to different attributes like we do. Their main limitations seem to be in the area of higher-degree embedding, but this is (a) not a property of phonology as proposed in this dissertation and (b) an expected result if, as Hauser et al. (2002a) hypothesize, recursion is a part of FLN and therefore not shared with other species.

⁵See also Addessi et al. (2008) on transitive symbolic representation in capuchin monkeys, and Cheney & Seyfarth (2007) on transitive inference involving social hierarchy in baboons. Cheney & Seyfarth also discuss both transitive social dominance and learning of symbolic representations in pinyon jays.

6.3 Patterns

The next set of abilities we will consider are those which deal with extracting patterns from a data stream and/or learning arbitrary associations. As I mentioned in the previous section, I view pattern-detection as the flipside of grouping: a pattern is essentially a relation between multiple groups, or different objects within the same group. Thus, the ability to assign objects to a set or an equivalence class is a prerequisite for finding any patterns in which those objects participate, so the abilities discussed in the previous section are very much relevant to this one, as well.

Several experimental studies on animal cognition bear on the issue of abstract pattern learning. One such study, undertaken by Hauser et al. (2002b), tested whether tamarins could extract simple patterns ('algebraic rules') like same-different-different (ABB) or same-same-different (AAB) from a speech stream. They performed an experiment very similar to one run on infants by Marcus et al. (1999). The auditory stimuli in both of these studies were of the form $C_1V_1C_1V_1C_2V_2$ (the AAB condition) or $C_1V_1C_2V_2C_2V_2$ (the ABB condition), such as *li-li-wi* or *le-we-we*. After habituating the infants/tamarins to one of these conditions, they tested them on two novel test items: one from the same class to which they had been habituated, and a second from the other class. The item with a different pattern than the habituated class should provoke a dishabituation response if the subjects succeed in learning the appropriate generalization based on the pattern in the stimuli presented during the training phase. Both infants and tamarins evidenced learning of these simple patterns; they were more likely to dishabituate to the item with the new pattern.

This type of pattern-extraction ability could serve phonology in several ways,

such as the learning of phonological rules or phonotactic generalizations. Recall that Heinz (2007) showed that phonotactics can be captured without any exceptions if three segments at a time are taken into account, so it seems on the basis of tamarins' success in the Hauser et al. experiment that learning phonotactics would not be out of their range of capabilities (though as we will soon see, tamarins may have independent problems with consonantal sounds that would interfere with this potential). Furthermore, phonotactics (and all attested phonological rules) can be modeled with finite-state grammars, as has been known since Johnson (1970). Here the somewhat controversial findings of Fitch & Hauser (2004) may also be relevant. At least under one interpretation of the data obtained by Fitch and Hauser, tamarins succeed at learning finite-state grammars but fail to learn more complicated phrase-structure grammars. If we accept these conclusions, then in theory—problems with consonants notwithstanding—we would expect that tamarins could learn any attested phonotactic restriction or phonological rule.

One of the most important obstacles facing a language learner/user falls into the category of pattern-extraction. This difficult task is parsing the continuous speech stream into discrete units (be they phrases, words, syllables, or segments). This speaks directly to (291b,c). Obviously, segmenting speech requires some mechanism for detecting the edges of these units. Since the 1950's, it has been recognized that one way to detect the edges of words is to track transitional probabilities, usually between syllables. If $\text{Pr}(AB)$ is the probability of syllable B following syllable A, and $P(A)$ is the frequency of A, then the transitional probability between A and B can be represented as:

$$(298) \quad TP(A \rightarrow B) = \frac{Pr(AB)}{Pr(A)}$$

The transitional probabilities within words are typically greater than those across word boundaries, so the task of finding word boundaries reduces to finding the local minima in the transitional probabilities. Numerous experimental studies suggest that infants do in fact utilize this strategy (among others) to help them parse the speech stream, and that statistical learning is not unique to the linguistic domain but is also utilized in other areas of cognition (see references in Gambell & Yang (2005)). With respect to the availability of this strategy in non-humans, Hauser et al. (2001) found that tamarins are able to segment a continuous stream of speech into three-syllable “words” based solely on the transitional probabilities between the syllables. Rats are also sensitive to local minima in transitional probabilities (Toro et al. 2005).

While transitional probabilities between syllables are strictly local calculations (i.e., they involve adjacent units), some phonological (and syntactic) dependencies are non-adjacent. This is the case with vowel harmony, for instance, and is also relevant to languages with ‘templatic’ morphology, such as Arabic, in which a tri-consonantal root is meshed with a different group of vowels depending on the part of speech which the root instantiates in a particular context. Comparing the results obtained by Newport & Aslin (2004) and Newport et al. (2004) provides an extremely interesting contrast between human and tamarin learning of such patterns. Newport et al. tested adult humans and cotton-top tamarins on learning artificial languages, all with three-syllable CVCVCV words, involving the three different kinds of non-adjacent dependencies which I list below.

- (299) a. *Non-adjacent syllables*: the third syllable of each word was predictable on the basis of the first, but the second syllable varied.
- b. *Non-adjacent consonants*: The second and third consonants of each word were predictable on the basis of the first, but the vowels varied.
- c. *Non-adjacent vowels*: The second and third vowels of each word were predictable on the basis of the first, but the consonants varied.

Both humans and tamarins succeeded at learning the languages tested in the non-adjacent vowel condition. Humans also succeeded at the non-adjacent consonant condition. These results are expected, at least for the humans, because both of these types of dependencies are attested in natural language (in the guises of vowel harmony and templatic morphology, as I have already noted). Tamarins failed in the non-adjacent consonant condition, though this does not cast aspersions on the fact that they were able to learn non-adjacent dependencies; rather, it suggests that they have the cognitive capability needed to create the appropriate representations, but they might have difficulty distinguishing consonant sounds. In other words, their failure may not be due to the pattern-detection mechanism, but in the input to it. This interpretation is supported by the fact that tamarins also succeeded at establishing dependencies between non-adjacent syllables.

From a phonological perspective, perhaps the most intriguing result is that humans failed at this non-adjacent syllable condition. Newport et al. (2004:111) ask:

“Why should non-adjacency—particularly syllable non-adjacency—be difficult for human listeners and relatively easy for tamarin monkeys? [...]his is not likely to be because tamarins are in general more cognitively capable than adult humans. It must therefore be because human speech is processed in a different way by humans than by tamarins, and particularly

in such a way that the computation of non-adjacent syllable regularities becomes more complex for human adults.”

They go on to suggest that perhaps the syllable level is only indirectly accessible to humans because we primarily process speech in terms of segments (whereas tamarins process it in more holistic, longer chunks).⁶ This is a possible contributor to the observed effect, but other explanations are available. I will propose one here.

Perhaps these results tell us that, in effect, tamarins fail to exhibit a minimality effect.⁷ Let us interpret the tamarins’ performance in the non-adjacent consonant condition as suggesting, as I did above, that they either (for whatever reason) ignore or simply do not perceive consonants. Then for them, the non-adjacent syllable task differs minimally from the non-adjacent vowel task in that the former involves learning a pattern which skips the middle vowel. So rather than paying attention to co-occurrences between adjacent vowels, they have to look at co-occurrences between vowels which are one away from each other. It seems likely, as Newport et al. also suggest, that the adjacent vs. one-away difference represents only a small increase in cognitive demand. But for us, the non-adjacent syllable condition is crucially

⁶Alternatively, Newport et al. suggest, it could be that tamarins’ shorter attention span reduces the amount of speech that they process at a given time; this would restrict their hypothesis space, making the detection of the syllable pattern easier. It is not obvious to me how this explains the tamarins’ pattern of performance across tasks, however.

⁷Such effects have been discussed in terms of Relativized Minimality (Rizzi 1990) or the Minimal Link Condition (Chomsky 2000, 2004) in syntax and the No Line-Crossing Constraint (Goldsmith 1976) in autosegmental phonology. In the theory proposed here, minimality in phonology (and perhaps in narrow syntax as well) emerges from a directional SEARCH mechanism which traverses strings of segments (detailed in Ch. 4; see also Mailhot & Reiss (2007) and Samuels (In press)).

different—and this is true no matter whether we are actually paying attention to syllables, consonants, or vowels. These categories have no import for tamarins, but for humans, they are special. The dependency we seek in this condition is between two non-adjacent elements of the same category, *which are separated by another instance of the same category*. This is a classical minimality effect: if α, β, γ are of the same category and $\alpha \succ \beta \succ \gamma$ (\succ should be read for syntax as ‘c-commands’ and for phonology, ‘precedes’), then no relationship between α and γ may be established. This restriction is captured straightforwardly if linguistic dependencies (Agree, harmony, etc.) are established by means of a search procedure which scans from α segment by segment until it finds another instance of the same type (i.e., β), then stops and proceeds no further. If I am on the right track, then there are two possible reasons why tamarins succeed where humans fail. It could be either because their search mechanism does not work this way—which would be odd if minimality/locality restrictions arise from Third Factor principles such as efficiency of computation—or more likely, that they do not represent the portions of the stimuli which they track as all belonging to the same abstract category of ‘vowel’ which is sufficient to trigger minimality effects for us.

A variety of other studies on primate cognition focus on the ability to learn sequences. Given that sequencing or precedence relationships are extremely important to language, particularly given the Minimalist emphasis on Merge in syntax and my parallel emphasis on linearization and the concatenation of morphemes in morphophonology, these studies are quite intriguing from a linguist’s perspective. One apparent cognitive limitation of non-human primates relative to our species in the

domain of pattern-learning is that they have extreme difficulty with non-monotonic sequences. Conway & Christiansen (2001) report on a number of studies which compare primates' performances on this kind of task. In certain respects, non-human primates have abilities in this area which are comparable to those of human children: for example, when presented with an "artificial fruit" requiring four arbitrary actions to open it and thereby reveal a treat, chimpanzees and human preschoolers perform similarly; both succeed at learning the sequence.

However, another study highlights what seems to be a difference in the way humans and other primates plan and perform sequential actions. One experiment undertaken by Ohshiba (1997) tested human adults, Japanese monkeys, and a chimpanzee on the ability to learn an arbitrary pattern: they were presented with a touch screen with four different-sized colored circles on it and had to touch each one in sequence to receive a reward; the circles disappeared when touched. All the species succeeded in learning a monotonic pattern: touch the circles in order from smallest to largest or largest to smallest. They also all succeeded at learning non-monotonic patterns, though they were slower at learning them.⁸ But as we will discuss in §6.4, measurements of reaction times suggest the humans and other species used different strategies in planning which circles to touch.

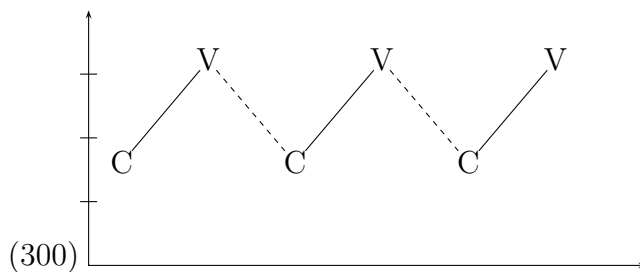
⁸In some situations, non-human primates fail entirely at learning non-monotonic patterns. For example, Brannon & Terrace (1998, 2000) found that while rhesus macaques who had been taught the first four steps in a monotonic pattern could spontaneously generalize to later steps, they failed to learn a four-member non-monotonic pattern even with extensive training. It is not clear what accounts for the worse performance in the Brannon & Terrace studies; there are too many differences between the paradigm they used and the one reported in the main text, including the species tested.

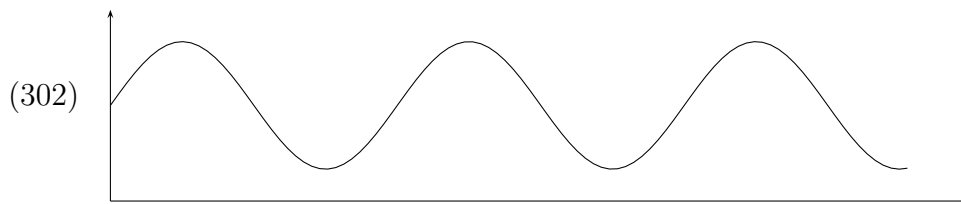
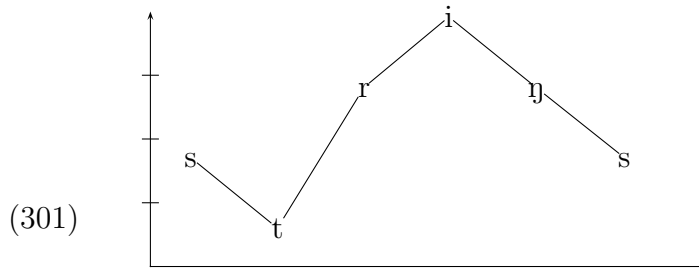
Rhythm, too, is a type of pattern. Rhythmicity, cyclicity, and contrast are pervasive properties of language, particularly in phonology. Everything that has been attributed to the Obligatory Contour Principle (Leben 1973) fits into this category. As we have already discussed, Walter (2007) argues that these effects should be described not with a constraint against repetition, but as emerging from two major physical limitations: the difficulty of repeating a particular gesture in rapid succession, and the difficulty of perceiving similar sounds (or other sensory stimuli) distinctly in rapid succession. These are both extremely general properties of articulatory and perceptual systems which we have no reason to expect would be unique to language or to humans.

To date, perhaps the most direct cross-species test of the perception of human speech rhythm (prosody) comes from Ramus et al. (2000). In Ramus et al.'s experiment, human infants and cotton-top tamarins were tested on their ability to discriminate between Dutch and Japanese sentences under a number of conditions: one in which the sentences were played forward, one in which the sentences were played backward, and one in which the sentences were synthesized such that the phonemic inventory in each language was reduced to /s a l t n j/. The results of these experiments showed that both tamarins and human newborns were able to discriminate between these two unfamiliar and prosodically different languages in the forward-speech condition, but not in the backward-speech condition. A generous interpretation of these results would suggest "at least some aspects of human speech perception may have built upon preexisting sensitivities of the primate auditory system" (Ramus et al. 2000:351). However, Werker & Voloumanos (2000) caution that

we cannot conclude much about the processing mechanisms which serve these discrimination abilities; this is of particular concern given that the tamarins' ability to tell Dutch and Japanese apart was reduced in the reduced phonemic inventory condition. This may indicate that tamarins rely more strongly on phonetic cues rather than prosodic ones. Given the apparent importance of prosody for syntactic acquisition in human children—specifically, babies seem to use prosodic information to help them set the head parameter—Kitahara (2003:38) puts forth the idea that “cotton-top tamarins fail to discriminate languages on the basis of their prosody alone, because syntactic resources that require such prosodic-sensitive system [sic] might not have evolved for them.” Though it is unclear how one might either support or disprove such a hypothesis, it is at the very least interesting to consider what prosody might mean for an animal which does not have the syntactic representations from which prosodic representations are built (recall Ch. 5, though the same would hold even for indirect reference approaches to phonological phrasing).

Another example of rhythmicity in speech is the wavelike sonority profile of our utterances, as we discussed in Chapter 3. Recall that all syllables, from CV (300) to CCCVCC (301), combine to yield a sonority profile roughly as in (302):





I suggested earlier that the ability to break this wave up into periods aids with the identification of word boundaries: they tend to fall at the local minima or maxima in the wave. And as we saw earlier in this section, we already know that both human infants and tamarins are sensitive to local minima (of transitional probabilities) in speech, which I believe suggests that this is a legitimate possibility.

Animals from a wide variety of clades show preferences for rhythmicity in their vocalizations and other behaviors as well, though it is important to note that our own (non-musical) speech has no regular beat; while language does have a rhythm, it is not a primitive (see discussion in Patel 2008). Yip (2006b) mentions that female crickets exhibit a preference for males who produce rhythmic calls, and Taylor et al. (2008) discovered that female frogs prefer rhythmic vocalizations as well. Rhythmic behaviors, or the ability to keep rhythm, appear to be widespread in the animal kingdom. Gibbons produce very rhythmic ‘great calls,’ and while Yip (2006b:443) dismisses this, saying that “the illusion of rhythm is probably more related to breath-

ing patterns than cognitive organization,” this should hardly disqualify the data. For example, the periodic modulation of sonority in our speech is closely connected to the opening and closing cycle of the jaw (Redford 1999, Redford et al. 2001), and it is widely accepted that the gradual downtrend in pitch which human utterances exhibit has to do with our breathing patterns. Whether or not these properties are ‘cognitive’ in Yip’s narrow sense is immaterial.

There are also some animals which synchronize the rhythms produced by multiple individuals. For example, frogs, insects, and bonobos all synchronize their calls; some fireflies synchronize their flashing, and crabs synchronize their claw-waving (see Merker 2000 and references therein). However, while elephants can be taught to drum with better rhythmic regularity than human adults, they do not synchronize their drumming in an ensemble (Patel & Iversen 2006).

Finally, we should note that it is extremely common for animals to exhibit ‘rule-governed’ behavior in the wild, and in their communicative behavior in particular. Cheney & Seyfarth (2007) make the case that baboon vocalizations are rule-governed in that they are directional and dependent on social standing. That is, a baboon will make different vocalizations to a higher-ranked member of the group than she will to a lower-ranked member. By this same rubric, vervet monkey grunts and chimpanzee calls should also be considered rule-governed; a number of articles on species ranging from treefrogs to dolphins to chickadees in a recent special issue of the *Journal of Comparative Psychology* (August 2008, vol. 122.3) devoted to animal vocalizations further cement this point. And as we saw in the previous section, both bird and whale songs obey certain combinatorial rules—in other words, they have

some kind of syntax (in the broad sense of the term). Here the distinction made by Anderson (2004) and suggested in earlier work by Peter Marler is useful: plenty of animals have a “phonological” syntax to their vocalizations, but only humans have a “semantic” or “lexical” syntax which is compositional and recursive in terms of its meaning. Again, this reiterates Hauser et al.’s view that what is special about human language is the mapping from syntax to the interfaces (and particularly the LF interface, as Chomsky emphasizes in recent writings; see, e.g., Chomsky (2004)), not the externalization system.

6.4 Operations

The final set of abilities which we will discuss are those which pertain to the phonological operations for which I have argued in this dissertation: SEARCH and COPY as well as concatenation (as distinct from Merge).⁹

Searching is ubiquitous in animal and human cognition. It is an integral part of foraging and hunting for food, to take but one example. The Ohshiba (1997) study of sequence-learning by monkeys, humans, and a chimpanzee is an excellent probe of searching abilities in primates because it shows that, while various species can perform the multiple sequential searches required to perform the experimental task (touching four symbols in an arbitrary order), they plan out the task in different ways. The humans were slow to touch the first circle but then touched the other three in rapid succession, as if they had planned the whole sequence before beginning

⁹I have little to say about the third operation, DELETE, but nothing suggests to me that this should be considered a domain-specific or species-specific ability.

their actions (the ‘collective search’ strategy). The monkeys, meanwhile, exhibited a gradual decrease in their reaction times. It was as if they planned only one step before executing it, then planned the next, and so forth (the ‘serial search’ strategy).

Perhaps most interestingly of all, the chimpanzee appeared to use the collective search strategy on monotonic patterns but the serial search strategy when the sequence was not monotonic. That chimpanzees employ collective searches is corroborated by the results of a similar experiment by Biro & Matsuzawa (1999). The chimp in this study, Ai, had extensive experience with numerals, and she was required to touch three numerals on a touchscreen in monotonic order. Again, her reaction times were consistently fast after the initial step. But when the locations of the two remaining numerals were changed after she touched the first one, her reactions slowed, as if she had initially planned all three steps but her preparation was foiled by the switch. It is not clear to me exactly what should be concluded from the disparity between humans, chimps, and monkeys, but notice that the SEARCH mechanism proposed by Mailhot & Reiss (2007) and adopted here operates in a manner consistent with the collective search strategy: scan the search space to find all targets of the operation to be performed, and then perform the operation to all targets sequentially.

A parallel to the COPY operation in phonology, particularly the copying of a string of segments as in reduplication, would be the patterns found in bird and whale songs. As we saw in §6.2, Slater (2000) shows that for many bird species, songs take the shape $((a^x)(b^y)(c^z))^w$: that is, a string of syllables a , b , c , each of them repeated, and then the whole string repeated. We also saw that whale songs are similarly structured (Payne 2000). Schneider-Zioga & Vergnaud (2009) question the parallel

which I suggest here, contending that human phonology (i.e., reduplication) exhibits repetition at the planning level, while birdsong exhibits repetition only at the level of execution. While this is a possible interpretation of the type of repetition found in bird and whale song, I would counter that we simply do not know enough about the mental representations in these species to rule out the possibility that there *is* repetition at the planning level in their vocalizations. Of course, we all should hope that future research can shed light on this matter, but it is important to keep in mind that regardless of the level at which repetition occurs, there must be a computational mechanism which makes it possible.

With respect to the copying of a feature from one segment to another (as in assimilatory processes), the relevant ability might be transferring a representation from long-term memory to short-term memory: retrieving a lexical representation (or extracting a feature from one) and bringing it into the active phonological workspace (Phillips & Lau 2004). This seems like a prerequisite for any task which involves the recall/use of memorized information, and perhaps can be seen as a virtual conceptual necessity arising from computational efficiency (a prime source of Third Factor explanation; see Chomsky (2005, 2007)).¹⁰

As I mentioned in the previous two sections, concatenation serves both the ability

¹⁰If we think of copying as including imitative or mimicking behaviors, then this, too, is a very common ability. However, as Hauser (1996) stresses, monkeys and apes are not very strong vocal learners, as opposed to songbirds and cetaceans, which are quite skilled in this area. Nevertheless, monkeys' learning is facilitated by watching a demonstration (Cheney & Seyfarth 2007), and Arbib et al. (2005) argue that chimpanzees have the capacity for simple imitation that monkeys lack; humans have the capacity for complex imitation that chimps lack.

to group and the ability to perform sequential actions. Without the ability to assign objects to sets or combine multiple steps into a larger routine, neither of grouping nor sequential planning is possible. We have already seen that bird and whale songs have the kind of sequential organization which is indicative of concatenated chunks, and primates can perform multi-step actions with sub-goals.

Concatenation is relevant to the “number sense” common to humans and many other species as well (for an overview, see Dehaene 1997, Butterworth 1999, Lakoff & Nuñez 2001, Devlin 2005). This is perhaps clearest in the case of parallel individuation/tracking, or the ability to represent in memory a small number of discrete objects (< 4 ; see Feigenson & Halberda (2004), Hauser et al. (2000), and references therein) and perform basic operations, such as comparisons of magnitude, over those representations. Shettleworth (1998) provides an overview of animal abilities in this domain, which have been shown for species as diverse as parrots and rats.

The idea that there is a connection between parallel individuation and concatenation is suggested by the fact that the speed of recognizing the number of objects in a scene decreases with each additional object that is presented within the range of capability (Saltzman & Garner 1948). This leads me to suspect along with Gelman & Gallistel (1978) (but contra Dehaene) that such tasks require paying attention to each object in the array separately, albeit briefly. Lakoff & Nuñez (2001) also discuss a number of studies showing that chimpanzees (most notably Ai, whom we met previously as the subject of Biro & Matsuzawa’s (1999) study), when given rigorous training over a long period of time, can engage in basic counting, addition, and subtraction of natural numbers up to about ten. These tasks clearly involve the as-

signment of (sometimes abstract symbolic) objects to sets, which is the fundamental basis of concatenation. Conversely, subtraction or removal of objects from a set could be seen as akin to the DELETE operation; the ability to subtract has also been shown in pigeons. This and a number of other studies showing that primates, rats, and birds can both count and add with a fair degree of precision are summarized in Gallistel & Gelman (2005).

6.5 Conclusions

I believe the studies of animal cognition and behavior which I have presented in this chapter provide ample evidence that Pinker & Jackendoff's (2005) criticism of Hauser et al. (2002a) concerning phonology is unfounded, at least if the theory of phonological representations and operations proposed in this dissertation is close to correct. A wide range of animal species have been shown to group objects, extract patterns from sensory input, perform sequential objects, perform searches, engage in copying behaviors, and manipulate sets through concatenation. As conceived of here, phonology thus provides no challenge to the idea that FLN is very small, perhaps consisting of just recursion and the mappings from syntax to the Conceptual-Intensional and Sensori-Motor interfaces. Moreover, such a conclusion lends credence to the theory developed in Chapters 2-5 as a biologically plausible model of phonological competence. The human phonological system is, in short, a domain-general solution to a domain-specific problem, namely the externalization of language.

Nevertheless, another one of Pinker & Jackendoff's qualms with Hauser et al.—that the latter implicitly reject the popular hypothesis that ‘speech is special’—should

also be viewed skeptically. I do not deny the wide range of studies showing that speech and non-speech doubly dissociate in a number of ways which should be familiar to all linguists, as evidenced by aphasias, amusias, Specific Language Impairment, Williams Syndrome, autism, studies of speech and non-speech perception (some of which were discussed in §3.2.1), and so on. Pinker & Jackendoff (2005) provide numerous references pointing to this conclusion, as does Patel (2008) with regards to language and music specifically.¹¹

But on the other hand, there is also a great deal of literature which shows that many species' vocalizations are processed in a different way from non-conspecific calls, or from sounds which were not produced by animals. This is true of rhesus macaques, who exhibit different neural activity—in areas including the analogs of human speech centers—and lateralization in response to conspecific calls (Gil da Costa et al. 2004). Perhaps we should amend the 'speech is special' hypothesis: speech is special (to us), in just the same way that conspecific properties throughout the animal kingdom often are, but there is nothing special about the way human speech is externalized or perceived in and of itself.

As a final note, consider the following set of characteristics which Seyfarth et al. (2005) ascribe to baboon social knowledge: it is representational, discretely-valued,

¹¹In this area the state of the art is changing rapidly, and the presence of a language/music dissociation is still an open and interesting question. Mithen (2005) explores one interesting hypothesis which was advanced by Charles Darwin, namely that music/singing (re-)emerged in the human lineage prior to the emergence of compositional language (see also Fitch (2005)). This could explain the complicated mixture of shared and distinct abilities and anatomy which serve music and language, discussed in great detail by Patel (2008).

linear-ordered, rule-governed, open-ended, modality-independent, combinatoric or concatenative, propositional, and linearly hierarchical. With the arguable exception of propositionality (though cf. Bromberger & Halle (2000) on phonemes as predicates), this describes phonology perfectly. How can we maintain in light of this that the core properties of phonological computation are unique to language or to us?

Chapter 7

Conclusions

In this dissertation, I sought the answer to a very open-ended question posed by Chomsky:

“Throughout the modern history of generative grammar, the problem of determining the character of [the faculty of language] has been approached ‘from top down’: how much must be attributed to UG to account for language acquisition? The [Minimalist Program] seeks to approach the problem ‘from bottom up’: How little can be attributed to UG while still accounting for the variety of I-languages attained[...]?” (Chomsky 2007: 3)

As a means of approaching this question, we first discussed methodological issues. In Chapter 2, I introduced Minimalism, the substance-free approach to phonology, and Evolutionary Phonology, the three major underpinnings of a Galilean research program for phonology that strives to go “beyond explanatory adequacy” (Chomsky 2004). The conclusion of this chapter was essentially that, if the goal of a phonological theory is to characterize the properties of possible synchronic phonological systems, the role of diachrony must be factored out: what is *diachronically* possible must be separated from what is *computationally* possible, which is still different from what is *learnable*.

In Chapter 3 we turned to the task of defining the phonological representations. The intent throughout this chapter was to provide a workspace for the operations to be defined in Chapter 4. I began by presenting arguments from spoken and signed languages for the idea that phonological features are emergent rather than innate and providing evidence that such emergent phonological categories are indeed learnable. The discussion then turned to the issue of underspecification. I argued for a theory of ‘archiphonemic’ underspecification along the lines of Inkelas (1995), as opposed to ‘radical’ or ‘contrastive’ underspecification, and further argued in favor of a distinction between perseverant underspecification, which persists throughout phonological computation, and underspecification which is repaired by application of the SEARCH and COPY operations specified in the next chapter.

A large portion of Chapter 3 was concerned with how segmental and suprasegmental material is organized into strings, and the idea that phonological representations are ‘flat’ or ‘linearly hierarchical.’ We engaged in a comparison of phonological syllables and syntactic phrases and I argued that the analogies which have been made between these two objects are false. I provided evidence that the properties commonly attributed to syllabic structure can be explained as well or better without positing innate structure supporting discrete syllables in the grammar.

With Chapter 4, we began to put the representations discussed in Chapter 3 to use. I established the formalisms for the repertoire of primitive operations which, I argued, account for all (morpho)phonological processes. These three operations are as follows:

- SEARCH provides a means by which two elements in a phonological string may establish a probe-goal relation.
- COPY takes a single feature value or bundle of feature values from the goal of a SEARCH application and creates a copy of these feature values to the probe.
- DELETE removes an element from the derivation.

I illustrated the application of these operations with analyses of data from domains such as vowel harmony, reduplication, affixation, and subtractive morphology. I then argued for the extension of ‘generalized SEARCH and COPY’ to the rest of phonology, showing that these three parameterized operations yield a restricted typology of possible phonological processes and can achieve the necessary empirical coverage without positing any additional structure to representations or constraints on operations.

Chapter 5 took us from the representations and operations developed in the previous chapters to the syntax-phonology interface. This chapter provided proof of concept for “phonological derivation by phase” (PDbP), which combines elements of Lexical Phonology (Kiparsky 1982), Distributed Morphology (Halle & Marantz 1993), and Derivation by Phase (Chomsky 2001). PDbP allows us to maintain a direct reference conception of the syntax-phonology interface (Kaisse 1985, Odden 1990, Cinque 1993). The basis for this theory is the notion that phonology is cyclic, and moreover that this is the *direct consequence* of cyclicity (i.e., phasality) in syntax. Such a theory has numerous benefits, including computational efficiency and the ability to account for large amounts of data which can only be handled by a cyclic model of the grammar.

With this model in place, I presented arguments that the domains of phonological rule application, both above and below the word level, come for free when we assume Distributed Morphology and a phasal syntax. Specifically, phonological processes and operations are restricted by the Phase Impenetrability Condition: lexical rules may apply across two adjacent morpheme-level spell-out domains, while post-lexical rules may apply within one clause-level spell-out domain but no more. The overarching claim supported in Chapter 5 was that morpheme-level phases can replace Lexical Phonology's hierarchy of strata, and that phrase-level phases can replace the prosodic hierarchy. These arguments were supported with analyses of segmental and suprasegmental processes including detailed case studies from Basque, Korean, and Serbo-Croatian. Though the specifics of the model presented in Chapter 5 will surely require change as syntactic theory evolves—such is the nature of an interface theory—I hope that the discussion will stand the test of time, at least as a guide to how one might go about pursuing this type of analysis, and as a skeleton to be fleshed out in the coming years.

In the final body chapter, Chapter 6, we focused on the broader implications of the theory presented in the earlier chapters. I demonstrated on the basis of behavioral and physiological studies conducted by other researchers on primates, songbirds, and a wide variety of other species that all the cognitive abilities necessary for human phonological representations and operations are present in creatures other than *Homo sapiens* (even if not to the same degree) and in domains other than phonology or, indeed, language proper. This discussion had two complementary purposes: to give a plausibility argument for Chapters 2-5 as a biologically viable theory, and to

situate phonology within the faculty of language. The conclusion from looking at a wide range of animal studies was that phonology may be entirely describable through ‘Third Factor’ principles pertaining to general cognition and the Sensori-Motor system. I therefore rejected the claim made by Pinker & Jackendoff (2005: 212) that “major characteristics of phonology are specific to language (or to language & music), [and] uniquely human,” and their conclusion that “phonology represents a major counterexample” to the hypothesis proposed by Hauser et al. (2002a), namely that the faculty of language in the narrow sense consists of only recursion and the mapping from narrow syntax to the interfaces.

Though I attempted in this dissertation to give as comprehensive a theory of phonology as possible within time and space constraints, in several cases I have been unable to discuss important issues as thoroughly as they deserve, or indeed at all. One is the answer to a question which has never, I believe, received an adequate answer: where does phonological variation come from? To address this will surely require deeper investigation into phonology and phonetics as well as studies of our perceptual and articulatory systems, our learning biases, and human cognition more broadly. A second issue within phonology proper is how to characterize the unusual properties of suprasegmentals—I have largely neglected tone here. Related to this is the issue of prosody/intonation, which I have also set to the side.

Moreover, though one of my main aims in writing this dissertation was to bring syntactic and phonological theory closer together, at least in the way they are approached, in doing so I have created more questions than I have answered. An important one, hinted at in Chapters 3-6, concerns the relationship between syntactic and

phonological representations and operations. It is tempting to equate, for example, SEARCH and COPY with Agree in syntax, but one gets the feeling that intriguing differences may lie beneath the surface similarities—just we found in our comparison of syllables and syntactic phrases. The third group of questions left open also pertains to syntax and phonology: what is the nature of the syntax/phonology interface and what is the order of operations (linearization, copy deletion, vocabulary insertion, etc.) which occur there? Some hints can be found in Chapter 5, but more thorough investigation of this highly complex issue is certainly warranted. Phenomena like clitic positioning and stress assignment to elements which move in the syntax seem to provide good entry points into such a study. In short, several large outstanding issues remain, and I hope that the present study will inspire others to begin tackling these problems with me.

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